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IN ALL ITS APPLICATIONS TO

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THE CHEMICAL NEWS.

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SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Researches on the Relations existing between Atomic Weights, by M. J. S. STAS, *Member of the Royal Academy of Belgium.*

(*Continued from page* 337.)

Determination of the Oxygen in Chlorate of Potash by means of the Action of Hydrochloric Acid.—To effect this operation, and the weighing of the vessel containing the substances, I took a hard glass globe, of a litre and a-half capacity. I caused to be ground with emery into its neck a hard glass stopper, pierced with two holes, in which two hard glass tubes could be tighty fitted. Exteriorly the neck of the hard glass globe was roughened to the depth of six centimètres; on this ground portion I fitted a cap of natural caoutchouc furnished with a stopcock, taking all the precautions pointed out when speaking of the means employed to effect the weighings in vacuo. After having first weighed the globe free from air, and then free from air but containing the dried chlorate of potash—counterpoised in each case with a closed globe of the same capacity,—I remove the caoutchouc cap and preserve it in vacuo with the flattened metallic wire which binds it round.

On the other hand, I arrange an apparatus furnishing pure hydrochloric acid; for this purpose I disengage gaseous hydrochloric acid, by means of sulphuric acid acting upon concentrated liquid hydrochloric acid free from sulphurous acid. To make more certain, I wash the gas by passing it through two Woulf's bottles containing pure hydrochloric acid. I then place the apparatus for the disengagement of hydrochloric acid in communication with the globe, in which I have poured before putting in the stopper a hundred cubic centimètres of absolutely pure water. With this object I arrange through one of the holes in the glass stopper a long hard glass tube, descending into the body of the flask, which is inclined until the neck is almost horizontal.

This tube is connected at its opposite end with a caoutchouc pipe adapted to the disengagement tube of the hydrochloric acid. Through the other opening of the stopper I fix a tube which extends half-way down the neck of the flask. The portion of this latter tube which extends outside the flask, is doubly curved, and has two strong bulbs blown in it which act as washing-bottles. All the gas which is evolved from the globe should traverse the liquid contained in the bulbs before arriving at a Woulf's bottle containing pure water, and intended to retain the traces of chloride, which the first system of condensation might have allowed to escape. To this Woulf's bottle was adapted a tube through which the gases were conducted into a good chimney.

After having surrounded the body of the flask with ice, I disengaged the hydrochloric acid. The chlorate of potash, covered with water, is immediately attacked with disengagement of chlorine and even of chlorous acid, which goes out of the flask through the second tube traversing the liquid contained in the bulbs with which it is furnished. If the disengagement of hydrochloric acid is rapid, the temperature rises too much in the globe in spite of the ice which surrounds it, and the chlorous acid which is formed detonates in presence of the hydrochloric acid. This accident happened to me on one occasion and destroyed the apparatus. If, on the contrary, the hydrochloric acid enters the globe too slowly, hydrate of chlorine is formed which surrounds the chlorate and renders the experiment almost interminable. To avoid, at the same time both these inconveniences, I have, in the last two analyses, effected the decomposition of the chlorate on the previously fused salt. The salt had been weighed before fusion; I weighed it afterwards, and am enabled to state that chlorate of potash can be fused and kept in fusion without losing oxygen. The chlorate of potash, fused and covered with water, is decomposed by hydrochloric acid always kept in excess without sensible production of chlorous acid gas. I have been able to decompose, in five or six hours, from 100 to 150 grammes of chlorate without perceiving in the globe the white vapours which are always produced in the simultaneous presence of hydrochloric and chlorous acid. When all the chlorate of potash is decomposed, which may be immediately known by the decolouration of the atmosphere of the globe, I interrupt the current, remove the ice-water, and replace it by water made gradually warmer; finally, I remove the stopper from the globe and pour into it all the liquid which is contained in the bulbs of the disengagement tube. I then proceed to evaporate all the liquid in the flask. In none of the three experiments which I have made by this method, has the quantity of liquid been sufficient to dissolve, at 0°, the half of the chloride of potassium produced.

The globe being conveniently inclined, I fix its neck in a flask of hard glass and heat it to a temperature near to, but always below, the boiling-point of the liquid contained. The evaporation of the liquid and the desiccation of the chloride being effected, the residuary chloride retains hydrochloric acid, which cannot be removed from it without fusing the chloride,—an operation which it is impossible to perform in a glass vessel, even when refractory. M. Penny has already shown the presence of hydrochloric acid in this case.

To eliminate this acid I had recourse to the following artifice:—I added to the chloride a small quantity of chlorate of potash, carefully dried and weighed (one to five grammes), and then I poured absolutely pure water on the whole. I evaporated the liquid again. During the evaporation, a sensible odour of chlorine was disengaged. The flask was then placed in a bath of magnesia and raised to a high temperature. The chlorate contained in the chloride was destroyed, with disengage-

ment of oxygen, and there remained chloride of potassium, pure, colourless, and perfectly neutral.

While the globe was in the magnesia bath, I adapted to it the caoutchouc stopper, and, after it was thoroughly cool, made a vacuum in it.

The washing-water contained in the Woulfe's bottle was evaporated over the water-bath in a small porcelain capsule, and left a residue, which, being dried, weighed 0·0038, consisting of a yellow matter, which heat and air completely destroyed. There was, therefore, no hloride carried over.

The liquid obtained by evaporation from the chloride in the flask having been evaporated to dryness, did not leave, in either of the three experiments, any trace of chloride. Thus, whatever may have been said, I am convinced, that solutions of alkaline chlorides may be evaporated down without the slightest trace of these compounds being carried over by the vapour of water.

The three analyses made by this method agree very well among themselves, and the result is identical with that obtained by the calcination of chlorate of potash.

Analysis of Chlorate of Potash.

Second Series.

Amount of Oxygen disengaged by the action of Hydrochloric Acid.

Numerical Order.	Apparent Weight of the Chlorate.	Weight of the Chlorate reduced to a vacuum.	Weight of the Supplementary Chlorate.	Total Weight of the Chlorate employed.	Apparent Weight of the Oxygen disengaged.	Actual Weight of the Oxygen disengaged.	Oxygen disengaged from 100·000 parts of Chlorate.	Chloride of Potassium produced by 100·000 parts of Chlorate.
	grammes.	grammes.	grammes.	grammes.	grammes.	grammes.		
VI.	58·7360	58·727	1·000	59·727	23·3865	23·3830	39·150	60·850
VII.	90·8810	90·7975	5·000	95·7975	37·5075	37·5020	39·147	60·853
VIII.	142·3390	142·318	5·000	147·318	57·6925	57·6840	39·156	60·844
						Mean . . .	39·1510	60·8490
						General Mean	39·1540	60·8460

(To be continued.)

Chemical Researches on the Assay of Silver by the Moist Way, by G. J. MULDER, *Math. Mag. Phil. Nat. Medicinæ, Artis Obstetriciæ et Pharmaceuticæ Doctor, Professor of Chemistry at the University of Utrecht, Grand Cross of the Order of the Netherlands Lion; of the Luxembourg Order of the Couronne de Chene; of the Swedish Orders of Wasa and Polar Star, &c., &c.*

(*Continued from page* 322.)

4. Determination of the Completion of the Analysis.—There exist three methods of finishing an assay experiment.

1. At the end just so much of the decimal solution of salt is added that a trace of precipitate is seen, but a precipitate only of a somewhat peculiar nature and of such peculiar character, that by experience it is already known to be final, while, at the same time, one is quite sure that if more salt solution had been added nothing more would have been seen. It is called the confirmative precipitate. As soon as this precipitate is seen, the quantities of salt used—*i.e.*, the quantities of standard solution and decimal solution used—are added together and calculated as equivalent to the quantity of silver.

2. It is a usual practice, also, to add repeatedly small portions of decimal solution of salt to the solution under experiment, until no more precipitate is seen. The half only of the last added portion is taken from the portion added just previous to the last. This, however, readily becomes a source of error, unless care be taken to add very small portions of salt (decimal solution).

3. Another method, again, is to find the neutral point at which in the two half portions of the liquid an equally large precipitate is produced both by addition of decimal solution of salt and by addition of decimal solution of silver.

While we have only given here the principles of the three methods, and while we shall have to treat more particularly of them hereafter, we must not neglect to take into consideration some scientific points which may duly aid us in estimating the divers influences which affect all three, or one of the three, methods just alluded to.

The first question which arises in every one of these methods is, whether or not all the silver is precipitated, and if not, whether the result will not be hereby affected. When the experiment has been well executed, *all* the silver is precipitated according to the first method, and it is needless to say that this is sure to be the case according to the second method, for if the liquid is filtered off from the precipitate and tested with sulphide of ammonium, after having saturated the nitric acid with ammonia, no trace even of colouration of the clear liquids, obtained by the first and second methods can be perceived. (The expression "all the silver precipitated" means that no re-agent will detect any of that metal in any of the liquids.) It has already been proved that if we work according to the third method, silver, indeed, is left in the assay liquid, for common salt produces a precipitate, and by treating the clear liquid as indicated with ammonia and sulphide of ammonium there will be seen in the liquid, brought to the neutral point, a very perceptible colouration occasioned by the formation of sulphide of silver. How much silver is dissolved, and does not this render the experiment incorrect? are two questions which must now be answered. As far as the quantity of silver kept in solution is concerned, this depends on the temperature of the test bottle. I merely remark here that at from 15° to 17° C. the quantity is 0·5 milligramme, less at lower, and more at higher temperatures (see page 321).

The question now arises, whether the deficiency is just the half of 0·54 milligramme—*i.e.*, 0·27 milligramme salt, equivalent to 0·5 milligramme silver?

The answer is No, for exactly so much excess of salt has been added. At the neutral point, the temperature being 15° to 17° C., the liquid contains ½ milligramme of silver, and therewith an equivalent quantity of salt—viz., the half of 0·54 = 0·27 milligramme.

At this neutral point the quantity of salt used was quite sufficient, which quantity of salt is chemically equivalent to all the silver, even when some silver remains dissolved in the liquid. At 15° to 17° C. the last half cubic centimètre of decimal salt solution added is not used to form chloride of silver. But that is not now the question. What is to be answered here is, whether the total quantity of salt added does exactly express the quantity of silver, and the answer is in the affirmative—viz., it is so.

Let us now return and review the first method, in which just so much salt has been added that a larger quantity of salt would not have yielded a precipitate; and it will be quite evident from what has just been stated, that at 15° to 17° a quantity of 0·27 milligramme of salt has been added in excess, that therefore 0·5 milligramme of silver too much has been calculated. The assay made according to the first method at from 15° to 17° gives the quantity of silver 0·5 milligramme too high, and the second method does the same, leaving out of the question the chance of greater error by applying too much salt if the experiment be somewhat roughly conducted. Common salt in solution fulfils a very peculiar function when it is added to chloride of silver dissolved in nitrate of soda after the point of neutrality has been reached. The function alluded to is not a chemical function either in the case of common salt or nitrate of silver, for the silver which is in solution requires no additional chlorine, and the chlorine in solution requires no additional silver.

It is of the more importance to consider the functions of each of these re-agents in this case, since chloride of silver is somewhat soluble in common salt. Curiously enough, this property is suspended in this case, and chloride of sodium exerts the power of expelling chloride of silver from its solution in nitrate of soda. A solvent, therefore, expels a dissolved substance from its solution without, however, exerting any chemical force upon it. I ask leave to tarry awhile and discuss this phenomenon so as to point out its great interest. For instance, let us assume that we have brought an assay experiment to the neutral point. The liquid then contains chloride of sodium and nitrate of silver, or, if it is preferred, chloride of silver dissolved in nitrate of soda. What, now, is the action exerted by the common salt when it expels chloride of silver? The most simple expression of the fact is this, that the peculiar solubility of chloride of silver in nitrate of soda becomes suspended by the addition of common salt, and also by nitrate of silver, and that there is formed in small quantity a similar combination between nitrate of soda and common salt (or nitrate of silver) as was previously formed between nitrate of soda and chloride of silver—a combination, however, in which chloride of silver is not soluble. In order to elucidate this, I will express the phenomenon in the form of a chemical formula.

At the neutral point there exists in solution Cl.Ag. and NO_5 NaO. The addition of common salt, while it expels Cl.Ag., produces Cl.Na and NO_5 NaO. Or the addition of nitrate of silver produces, while it expels Cl Ag., NO_5 Ag.O and NO_5 NaO.

For those who prefer the explanation according to Berthollet's views:—

1. The solution at the neutral point contains Cl.Na + No_5 AgO + NO_5 NaO.

2. Addition of chloride of sodium produces, while Cl.Ag. is expelled, Cl.Na + NO_5 NaO.

3. Addition of nitrate of silver produces, while it expels Cl.Ag., NO_5 Ag.O + NO_5 NaO.

According to the last formulæ, which I think the correct and true one, there is expressed in 1 the state of equilibrium between chemical force and power of solution: while in 2 and 3 there is expressed the creation of a new state of equilibrium between nitrate of soda and common salt or nitrate of silver, whereby the Cl.Ag. is expelled in equivalent proportion. If I may attach some value to any facts occurring in this treatise, I think that the last mentioned peculiarity is by far the most interesting, for if I do not mistake I have proved that there is here a relation between what we call physical and chemical action and one which may even be expressed in chemical equivalents. The fact is, that the solubility of chloride of silver in nitrate of soda can become suspended by a quantity of chloride of sodium or of nitrate of silver equivalent to the quantity of chloride of silver dissolved. I do not in the least doubt but this is generally so in all cases where it refers to the solution of such substances, the nature of which when in solution can be referred to what was first called attention to by Berthollet.

From the above mentioned, it is clear that in the first and second methods of assaying more common salt is used than is required for the formation of chloride of silver, the last portions of common salt being applied to expel from its solution the chloride of silver already formed. According to the third method, the quantity of chloride of sodium applied is just equivalent with the silver submitted to experiment. Viewed, therefore, from a chemical point of view, the third method is the only true and correct one.

Those circumstances which could disturb and influence in one of the three methods the final result, viz. the quantity of silver present, have been generally treated of in the foregoing section. They are the influence due to the quantity of silver submitted to experiment, the quantity of the nitric acid, and the quantity of that frequently occurring substance, copper. Finally, the influence of the temperature of the test bottle has to be considered. It is now our duty to look into these influences more deeply as far as regards methods 1 and 3, while it is not necessary to do this for the second method, since it is closely connected with the first.

Quantity of Silver.—I have previously observed that the quantity of chloride of silver which becomes dissolved in the liquid is three times larger in case 3 grammes of silver, from 3 to 5 cubic centimètres of nitric acid and 300 cubic centimètres of standard solution are used, than when only 1 gramme of silver is applied. That this, however, does not affect the result is proved by the following experiment for the first and third methods, performed at 15° C.

a 1 gramme of silver required 1003·15 c.c. salt
b 3 ,, ,, 3009·5 ,,

The experiment was finished by the addition of salt unto the limit.

The quantity of decimal solution required to come to the utmost limit of the silver was,—

For *a*, 20 drops = 1 cubic centimè re
,, *b*, 60 ,, = 3 ,, ,,

I then added to *a* 10 drops of decimal solution of salt

= 0·5 cubic centimètre; to *b*, 30 drops of the same solution = 1·5 cubic centimètre. The liquid having been thus brought to the point of neutrality, I found that in order to make *a* and *b* give equally large precipitates, *a* would require half a drop of decimal solution of salt, and *b* one drop of decimal solution of silver, after the liquids of each of them had been divided into two equal portions, to which in the one case five drops of nitrate of silver in the other five drops of salt, both in decimal solution, had been added.

It is, therefore, clear, that neither of the results of these methods is in any way dependent upon the quantity of chloride of silver in solution. The quantity of silver is this:—

	Precipitate unto the limit.	Neutral point.
a,	1003·15 + 0·025 = 1003·175	1002·675
b,	3009·5 − 0·05 = 3009·45	3007·95
⅓ of 3009·45 = 1003·15; ⅓ of 3007·95 = 1002·65.		

a and *b*, or one gramme of silver and three grammes of silver, yield the same result, while the quantity of chloride of silver dissolved in the liquid from *b* was three times larger than in that from *a*, while also in *b* three times more common salt had not given rise to any precipitate than was the case in *a*.

Nitric Acid and Copper.—Three experiments were mentioned before, which had been made for the express purpose of investigating the influence of this acid. I dissolved *a*, 1 gramme of pure silver in 5 cubic centimètres of nitric acid, *b*, 1 gramme in 10 cubic centimètres, and *c*, 1 gramme of pure silver with 1 gramme of pure copper in 7·5 cubic centimètres nitric acid; when all three were precipitated with salt solution unto the limit, and respectively required:—

a, 1003 c. c., *b*, 1003 c. c. and *c*, 1002·75 c. c. salt.

From which results it is sufficiently evident that nitric acid, although its quantity be doubled, has no influence on the results, notwithstanding we have seen that chloride of silver is somewhat soluble in stronger as well as in weaker nitric acid.

As regards the copper, it is not easy to see with great accuracy in such a case as this where one gramme of each metal was used; for that reason, and for one which I will presently mention, I feel inclined to attribute the ¼ milligramme difference to difficulty of observation; while the quantities of decimal silver solution also to reach the limit of precipitability required were as follows:—*a*, 20 drops: *b*, 22 drops; *c*, 20 drops. I added of decimal solution of salt, *a*, 10 drops; *b*, 11 drops; *c*, 10 drops. The three solutions had been brought so perfectly to the point of neutrality that at first one single drop only of solution of silver appeared to be required; but this having been added, a drop of decimal solution of salt appeared necessary again. There may be consequently a difference of half a drop.

It is clear, therefore, that nitric acid and copper do not change the neutral point, which, however was not to be expected. The results are recorded below, according to first and third method:—

	First Method. cub. cents.	Third Method. cub. cents.
Pure silver in the ordinary quantity of nitric acid	1003	1002·5
Pure silver in double the quantity of nitric acid	1003	1002·45
Silver and copper	1002·75	1002·25

The Temperature.—It is now absolutely necessary to consider the influence of the temperature of the test-bottle on the final result in the application of either the first or the third method. In the whole series of the following experiments 1 gramme of pure silver was dissolved in 5 cubic centimètres of nitric acid and precipitated with 100 cubic centimètres of standard solution at 16·5° C., the bottles having been brought to the undermentioned temperatures, at which they were kept as much as possible throughout the whole experiment. I also employed the decimal solution of salt. The quantity of solution of salt required to reach the limit of precipitability, which may, therefore, indicate the quantity of silver, was:—

a,	at 5° C.	1002·65 cubic centimètres
b,	„	1002·50 „
c,	„	1002·60 „
d,	6°	1002·70 „
e,	16°	1003·00 „
f,	16°	1002·90 „
g,	16·5°	1002·90 „
h,	17°	1003·15 „
i,	56°	1003·85 „
k,	56°	1003·85 „

It is, consequently, unnecessary to indicate further the influence of the temperature upon the first method; for in all these experiments the standard solution had a temperature of 16·5° C., and always one gramme of pure silver was used. The differences are only due to the difference of temperature of the test-bottle. Some of the above-mentioned liquids were brought to the point of neutrality by means of decimal solution of silver, the quantities required being the following:—

a,	at 5° C.	0·35 cubic centimètres
b,	5°	0·375 „
c,	5°	0·325 „
f,	16°	0·525 „
g,	16·5°	0·55 „
i,	56°	1·65 „
k,	56°	1·65 „

Here also the influence of the temperature is very apparent.

In order to be enabled to compare all these results of one gramme of pure silver treated with standard solution of the same temperature, according to methods the first and third, we have to subtract the last figures from the first, in which case we obtain:—

Temperature of test-bottle.	First Method. Precipitation with salt unto the limit. cubic cents.	Third Method. Bringing to neutral point. cubic cents.
a, 5° C.	1002·65	1002·30
b, 5°	1002·50	1002·125
c, 5°	1002·60	1002·275
f, 16°	1002·90	1002·375
g, 16·5°	1002·90	1002·35
i, 56°	1003·85	1002·20
k, 56°	1003·85	1002·20

The differences here enumerated are sufficiently clear From 5° to 16° the difference, according to the first method, is one-third of a milligramme, solely in consequence of the temperature of the test-bottle; while from 16° to 56° it amounts to almost one milligramme. The third method is, it will be seen, independent of the temperature. The differences there are differences due to the experiment, as it is found impossible that this and the temperature and other influences should not exert some slight disturbing effect over which the experimenter

has not a perfect control. When the first method is used, and care is not taken that the temperature of all the test-bottles be the same, that method can readily give rise to very great inexactness. A contra test or witness,[1] when used may readily, in this way (*i. e.*, when the effect of the temperature is neglected), become a false test.

How is it that the influence of the temperature of the test-bottles has not been long since discovered from the results obtained from the experiments?

Mohr[2] has quite neglected and totally overlooked the solubility of chloride of silver, even when heat is applied. He has, therefore, given very bad advice when he said, that the liquid, in making a silver assay, has to be heated. At 56° C. the quantity of chloride of silver dissolved is 4·39 milligrammes, already equal to 3·3 of silver.

From the facts here alluded to, the following deductions may be drawn:—The neutral point is exactly situated in the middle of the ultimate limits of precipitation which can be produced either by salt or silver; and this holds good without any disturbing effect of lower, middling, or high temperature. If we take, therefore, invariably one gramme of pure silver as a starting-point, and have brought the precipitation with salt unto the limit, but not further, taking care to add the salt *guttatim*, and we find that we require at different temperatures of decimal solution of silver to reach again the limit of silver, the following quantity in drops,—viz., 14, 16, 18, 20, 22, 24; then, on addition of 7, 8, 9, 10, 11, 12 drops of decimal solution of salt, we shall reach the neutral point again with perfect ease and certainty.

(To be continued.)

On the Estimation of the Carbon of Cast Iron, by M. le Dr. E. MULDER.

M. MULDER treats, in the first place, of the causes of the errors which may arise in working out the analysis by M. Regnault's method. Thus, he inquires whether the mixture of chromate of lead and chlorate of potash, at the high temperature of combustion, would not produce free chlorine. This is the known opinion of MM. Erdmann and Marchand.[3] They have even stated that nascent oxygen disengages chlorine from chlorate of potash. M. Marignac has also observed that oxygen proceeding from the decomposition of chlorate of potass is not free from chlorine.

The author is convinced that the mixture of chromate of lead and chromate of potash, as advised by M. Regnault, does not develope the faintest trace of chlorine, but that this is disengaged as soon as the chlorate of potass predominates over the chromate of lead. Chlorate of lead is then formed, which, while decomposing, furnishes free chlorine. M. Mulder also observes, that in following M. Regnault's directions there is no fear of losing the carbonic acid by the formation of carbonate of potash, the large quantity of chromate of lead preventing this. These analyses prove that chromate of lead alone is insufficient to induce the combustion of carbon, especially carbon in the state of graphite. Cast iron at first burns by the oxygen of the chlorate, and the combustion is finished by the chromate of lead; but towards the end of the operation this does not suffice for the combustion of graphite.

The results obtained by M. Kudernatsch's method of effecting combustion with oxide of copper alone are uncertain, at least if a current of oxygen is not at the same time passed through the combustion-tube, as M. Rose advises.

From his researches, M. Mulder comes to the conclusion that M. Regnault's method is insufficient to determine the total quantity of carbon, and that chromate of lead cannot occasion the combustion of graphite. He prefers direct combustion in an oxygen current. He fills two-thirds of the combustion-tube with pure sand, which has been heated to redness in oxygen; next he introduces a plug of asbestos and the mixture of pulverised cast iron with pumice-stone; then another plug of asbestos, a small layer of oxidised copper, and closes it with another plug of asbestos. This he heats with oak charcoal. In this way M. Mulder has discovered from 5·3 to 5·13 of carbon in cast iron which, analysed by M. Regnault's method, yielded but 3·77 per cent.—*Scheikundige Verhandelingen en Onderzoekingen*, vol. iii., p. 1.

[1] Professor Mulder understands by this word, that while an assay is performed, say with an alloy, another assay is at the same time made with perfectly pure silver under perfectly the same conditions.

[2] "Fitrir Methode," part ii., p. 34, German Edition.

[3] *Journal für praktische Chemie*, bd. xxxi., s. 275.

Contributions to the History of Picric Acid, by M. CAREY LEA, *Philadelphia.*

Solubility in Sulphuric Acid.—It is stated in the text-books that picric acid is insoluble in sulphuric acid. It is, however, soluble to a small degree in strong sulphuric acid; in a more dilute acid it is apparently wholly insoluble, until the dilution reaches a certain point, when it increases again. If picric acid be left in contact with oil of vitriol, and the latter be decanted, and mixed with two or three times its volume of water, the picric acid is deposited on cooling in what appear to be very minute square or nearly square scales.

Picric acid crystallises in the rhombic system, and if we suppose these scales to be formed by predominating $\infty \overline{P} \infty$ planes bounded at the edges by octahedral planes, they should be rhombs approaching very nearly to squares, having their axes as ·9374 : 1 0000.

If cold saturated aqueous solution of picric acid be mixed with sulphuric acid diluted with an equal volume of water, the following results are obtained:—

1 vol. solution of picrate, 4 vols. dilute sulphuric acid (1 vol. acid, 2 vols. water).	No precipitate, solution remaining as colourless as water.
1 vol. sol. picric acid, 2 dilute sulphuric acid, same dilution.	No precipitate, solution very nearly colourless, faintest tinge of yellow only visible.
2 vols. solution picric acid, 1 vol. sulphuric acid, same dilution.	Nearly the whole of the picric acid was precipitated.

The amount remaining in solution continued in further trials to diminish as the sulphuric acid became more dilute, until a maximum was reached with

3 vols. solution picric acid, 1 vol. dilute sulphuric acid, same dilution.	

It thus appears that mixtures of sulphuric and water reach their minimum of solvent power for picric acid when the mixture consists of about 1 vol. acid to 11 vols. water. The proportion of water may be still further increased without materially increasing the solvent

power for picric acid. If a cold saturated solution of picric acid be mixed with even but $\frac{1}{17}$th of its volume of sulphuric acid, almost the whole is thrown down.

The fact that the characteristic colour of picric acid, which it maintains so persistently through all its combinations, and which is so powerful that, as I have found by actual experiment, a milligramme will distinctly tinge a kilogramme of water, or in other words, that water is coloured by one-millionth of its weight of picric acid—the fact that this colour is totally destroyed by sulphuric acid of a certain strength, without in any way decomposing the acid, is very remarkable. Four volumes of sulphuric acid diluted with five volumes of water exhibit this property, and picric acid dissolves in such a mixture to a colourless solution. This peculiar property has no doubt led to the supposition of the insolubility of picric acid in sulphuric acid above referred to.

Water containing $\frac{1}{10000}$th of picric acid exhibits a bright yellow colour. With $\frac{1}{300000}$th the colour is still distinct, even in a stratum of not over an inch in thickness. But in large quantities a millionth gives a distinct colour as above mentioned.

Tests for Picric Acid.—The best tests for picric acid are:—Ammoniacal solution of sulphate of copper, which gives a greenish crystalline precipitate; alkaline sulphide with excess of alkali, which with heat gives a deep red liquid; alkaline cyanide with ammonia, which when heated gives also a red liquid.

The following table will exhibit the relative sensibility of these reagents:—

Strength of aqueous solution of picric acid.	Ammonio sulphate of copper.	Potash liver of sulphur (heat).	Cyanide of potassium.
$\frac{1}{1000}$	Immediate precipitate.	Solution becomes sherry wine red.	Pale sherry red, with heat becoming deep red.
$\frac{1}{4000}$	No precipitate at first, but by standing a few minutes a distinct one.	Deep yellow with a tinge of sherry colour.	Deep yellow, tinge of sherry colour, deepened by heating.
$\frac{1}{6000}$	Distinct precipitate by standing.		
$\frac{1}{10000}$	No precipitate.	The yellow colour was slightly deepened; the cyanide test is the more delicate of the two.	

Purification of Picric Acid.—Since my former observations on the purification of picric acid, I have had occasion to prepare considerable quantities of the acid for my examinations, and find that all purifications, by converting into potash salt, are inapplicable except for very small quantities. The picrate of potash crystallizes out by so small a fall of temperature that the filters, even when kept heated by a double funnel, become immediately clogged, and the operation becomes to the last degree tedious and troublesome. As the picrate of lime is very soluble, it seemed probable that it might afford a convenient means of solution; it has, indeed, been already recommended for that purpose.[1] But I find it wholly inadmissible. A basic salt is formed which falls to the bottom with the excess of hydrate of lime, and great waste ensues. The insolubility of alkaline picrates in cold alkaline solutions, which I have described in a previous number of this Journal, furnished me with an excellent process. The crude acid is saturated with carbonate of soda, an excess of which is to be avoided, as it tends to dissolve resinous matter. The hot solution is then easily filtered, and into the filtrate a few clear crystals of carbonate of soda are placed. On cooling, the picrate of soda crystallizes out almost as completely as the potash salt would have done, and all the wearisome delay in filtration is avoided. From the mother water more picric acid may be recovered by the addition of a little carbonate of potash. In decomposing alkaline picrates to separate the acid, sulphuric (and not as usually recommended, chlorhydric) acid should be used, because a moderate excess of sulphuric acid throws down a great portion of the acid which would otherwise remain in the mother water. A moderate but decided excess of acid is absolutely necessary, because otherwise a portion of alkaline picrate escapes decomposition. Even then, it is advisable to recrystallize the acid from alcohol. If picric acid be dissolved by the aid of heat in a solution of sulphate, nitrate, or almost any salt of potash, more or less picrate of potash will crystallize out on cooling. I have thought this process not devoid of interest, because picric must become more extensively known in the laboratory and in the arts than hitherto.

Effect of Reducing Agents.—The effects of reducing agents when alkali is not present, or not present in excess (in presence of excess of alkali, picramic acid is formed), are very variable, depending upon slight differences which it is very difficult to seize. I subjoin some of the best marked results obtained.

A mixture of picric acid, alcohol, iron filings, and acetic acid, were digested for an hour at a heat a little below 212°. The filtrate was intensely blue, by standing for half-an-hour or less, became brown and muddy, depositing a blackish powder in small quantity, and without trace of crystallization. This filtrate was not changed in colour by acids, or apparently affected by them. Alkalies decolorized it. Its shade of blue varied considerably in different experiments, sometimes full blue, sometimes violet, sometimes greenish.

Other experiments were made by acting on picric acid by zinc and dilute sulphuric acid. After an action of some hours, the solution was mixed with alcohol and filtered. The filtrate heated with bicarbonate of potash in successive portions, gave a fine violet liquid, which with further addition of alkali became deep blue with a tinge of violet. According as acid or alkali were present in excess, there was more of the violet or blue shade. The colours were always very fugitive, and changed to dirty brown by standing, with deposit of an amorphous blackish powder (very small in quantity compared with the picric acid used), which was soluble in acids, and insoluble in alkalies.

These experiments, although many times repeated, did not lead to the isolation of any substance of interest. There is a certain amount of resemblance between these reactions and those of some of the decomposition products of phenylamine: the latter contains the radical $C_{12}H_5$, which exists in a substituted form in picric acid. —*American Journal of Science*, No. 95.

[1] See Gmelin, Eng. Ed., vol. xi. p. 214.

On an Arsenical Thermal Water, by M. Guyon.

The author gives an analysis of water from a thermal source situated in the village of Bou-Chater, anciently Utica (rendered famous by the death of Cato). This water contains, besides the ordinary mineralizing elements, particularly sea salt (four to five thousandths), a comparatively enormous proportion of arsenic.

The numbers given by M. Guyon are 0·106 gramme per litre of arsenic acid, or 0·1684 of arseniates of potash

and soda, in a total weight of 0·9689 of salts; that is to say, more than a sixth of the dry residue. The temperature of the Bou-Chater water is 40°; it is clear, limpid, without any unpleasant taste; the inhabitants drink it after letting it cool.

The source forms a basin, about two metres in diameter; the water is confined by a dam of rough stones serving as an abode for an old tortoise (*emyde leprosa*) which has lived there from time immemorial, and is looked upon by the inhabitants as the genius of the place. The overflowings of the basin form a stream which, in default of a marked bed, spreads on all sides, forming a marsh of considerable extent, covered with rushes, Indian grass, and other plants peculiar to this kind of earth. The inhabitants make use of the stream for washing their linen and sheep's wool; and it is in the course of this same stream that their flocks, of all kinds, drink daily.

It would be interesting if physiologists would study the influence which these waters exercise on men and beasts, according to the recently raised questions on the economic action of arsenious acid.

M. Guyon believes this source of Bou-Chater to be that mentioned by Julius Cæsar in his "Commentaries," as having such a terrible effect on the army of the Curio. He thinks, considering the present innocuousness of these waters, that their mineral richness must have diminished. "There is nothing," says the author, "to contradict the supposition that the waters of Bou-Chater were formerly more charged with saline principles than they are at present; and perhaps this impoverishment may take place with all known thermal waters, the origin of which can be traced to a remote period.—*Repertoire de Chimie.*

On the Estimation of Platinum Diffused through Metallic Beds or in the Alpine Rocks of Dauphiny and Savoy, by M. Gueymard.

I have published, in the *Annales des Mines* and the *Comptes-Rendus* of the Institute, five memoirs on the discovery of platinum in metallic beds or in the Alpine rocks of Dauphiny and Savoy. Platinum is there found only in small quantities; and also in veins often containing silver or gold.

I assayed 100 grammes of material by the dry way, using the most convenient fluxes and a very pure litharge containing neither gold nor platinum.

As only a very small quantity of platinum was found in the button of lead, I added to it a little pure silver; the residuary beads on the cupil then contained silver, platinum, and frequently a little gold.

I consulted M. Berthier on the means of estimating, in the beads of metal, a quantity of platinum so small as not to be weighable in the most accurate balance. The problem was pronounced very difficult. After many researches I came to the conclusion that the solution might be arrived at by standard solutions of platinum. I will describe this unexpectedly successful process.

Dissolve 10 milligrammes of platinum in aqua regia, then add sufficient distilled water to make 250 cubic centimètres in bulk.

One cubic centimètre of this solution contains $\frac{1}{25}$th of a milligramme of platinum, or 2 cubic centimètres 0·08 milligramme.

Into eight little capsules put 2 cubic centimètres of the solution, containing 0·08 milligramme; 0·04 milligramme; 0·01 milligramme; 0·005 milligramme; 0·0025 milligramme; 0·00125 milligramme; and 0·000625 milligramme of platinum.

Into these eight capsules, placed in a row and each containing 2 cubic centimètres of standard solution, add a little powdered tin salt, mix with a glass rod, and the colour of the platinum will soon assume shades corresponding with the above figures from 0·08 to 0·000615 milligramme.

Treat the metallic beads by nitric acid and then add hydrochloric acid. Soluble chloride of platinum and insoluble chloride of silver are obtained. Add two drops of hydrochloric acid, then two cubic centimètres of distilled water. Let them stand for a few minutes and then decant into other little capsules.

To the capsules containing the solutions of the metallic beads add also powdered tin salt, and the colour of the platinum becomes very apparent in five or six minutes. This colour I compare with that of the eight capsules, and I select which ever contains the identical colour produced by the metallic beads. If this happens to be that of the fifth capsule, containing 0·005 milligramme of platinum, it may be concluded that the substance treated contains 0·005 milligramme of platinum to 100 grammes of material. If the colour is identical with that of the first capsule, the proportion in the material assayed will be 0·08 milligramme of platinum to 100 grammes.

Were the colour intermediate to those of the two capsules, Nos. 2 and 3, the platinum for 100 grammes would be

$$\frac{0{\cdot}04 \text{ milligramme} + 0{\cdot}002 \text{ milligramme}}{8} = 0{\cdot}03 \text{ milligramme.}$$

Were estimation by weighing possible, the results would not be so precise as those given by this process, which I regret not having devised in the early part of my investigations.

When the substance assayed contains a little gold with platinum, I have succeeded also in estimating it by a standard solution prepared with 20 milligrammes of gold dissolved in aqua regia, diluting the solution sufficiently to make the bulk 250 cubic centimètres. One cubic centimètre then contains $\frac{2}{25}$th of a milligramme of gold (0·08 milligramme), or 2 cubic centimètres, 0·16 milligramme.

Then take eight small capsules, into which put 2 centimètres cube of a solution containing 0·16 milligramme; 0·04 milligramme; 0·02 milligramme; 0·01 milligramme; 0·005 milligramme; 0·0025 milligramme; 0·00125 milligramme of gold.

Add to each a small quantity of powdered tin salt, and mix with a glass rod. In a few seconds the colour, more or less intense, of the purple precipitate of Cassius will be obtained.

Treated in this way, the substance assayed gives the yellow colour of platinum, and, when gold is also present, in less than a quarter of an hour the purple precipitate falls to the bottom of the capsule. Decant gently, and then add water enough to make the quantity in the capsule 2 centimètres cube. Compare the colours, as with platinum, and take which ever gives the same tint. I thus obtain, in fractions of a milligramme, all the gold contained in 100 grammes of substance.

I must add that the estimation of gold by standard solutions is less precise than the estimation of platinum, because the colours of the precipitate are less easy to appreciate. The colours of platinum are perfectly clear, and the estimation mathematically precise.

I have made several hundred analyses of platinum and

gold by this process; there cannot be a more certain and easy method; and I trust that, though somewhat late, the publication of it may not be without interest.—*Comptes-Rendus.*

On the Presence of Cæsium and Rubidium in certain Natural and Commercial Alkaline Substances, by M. L. GRANDEAU.

THE research of which the present note is an abstract has been commenced for many months, by the advice and with the assistance of M. Bunsen, who has kindly kept me *au courant* with all the practical details of the admirable method which he has published jointly with M. Kirchhoff. I ought first to say that if I now publish the principal results of my researches, it is after having experimented for a long time under the direction of the illustrious chemist of Heidelberg, and after having submitted to him almost all the results of my analyses.

I at first sought for the two new alkaline metals, rubidium and cæsium, in the mineral waters, and the minerals presenting some analogy with the waters, of Durckheim, which furnish cæsium, and with the lepidolite of Rozena, whence M. Bunsen has extracted rubidium. I then successively examined the salt mother liquors of the Meurthe, the water of the Mediterranean, of the ocean, of the Dead Sea, and, finally, of the mineral waters of Bourbonne-les-Bains and of Vichy. The waters of the sea and of the salt-springs have only hitherto yielded me lithia, as already announced by MM. Kirchhoff and Bunsen. The water of the Dead Sea, recently sent me by M. Delesse, gave me the characteristic rays of lithia and strontia.

The mother liquors obtained from the evaporation of many thousands of litres of Vichy water, which I owe to the kindness of M. J. Lefort, yielded me nearly two grammes of double chloride of platinum and cæsium, and of platinum and rubidium in an unknown proportion. That shows that the new alkalies only exist in small quantities in Vichy water. It is not the same with the thermal waters of Bourbonne-les-Bains. I have been enabled, thanks to the kind assistance of M. le Dr. Cabrol and M. le Dr. Tamissier, to evaporate down forty hectolitres of mineral water obtained from the new source in the Jardin des Bains Civil. By this evaporation, made in a tinned copper vessel of about the capacity of a hectolitre, the water deposited considerable quantities of chloride of sodium and lime salts containing a small quantity of strontia. The mother liquors contain much lithia and chlorides of cæsium and rubidium, which have served to prepare a portion of the products which I have the honour to submit to the Academy. Ten litres and a-half of Bourbonne water were reduced by evaporation to 250 cubic centimètres. This mother liquor, treated by an insufficient quantity of chloride of platinum, furnished immediately a slightly coloured precipitate, weighing 1·029 grammes, which introduced into the spectrum apparatus directly made evident the rays Lia, Ka, (8z, Csβ, Rbα, and Rbβ, characteristic of lithia, potassa, cæsia, and rubidia. The quantities of lithia and of potassa rendered evident by spectrum analysis were so small that these salts may be considered as being almost pure salts of cæsium and rubidium—a fact which leads M. Bunsen to think that I have found in the Bourbonne waters the most abundant source hitherto known for the two alkaline metals. On precipitating the mother liquor by an excess of chloride of platinum, I obtained a yellow salt, in which the presence of rubidium and cæsium only became apparent after repeated washings in boiling water. The quantity of these bodies contained in the second precipitate, which weighed 1·260, was nevertheless very notable. I have, moreover, recognised in the precipitate yielded by carbonate of ammonia the presence of strontia and of lithia, and I likewise think, arguing on the known reactions of boracic acid, that I shall discover the presence of this body; but I dare not state it positively on account of the uncertainty which still exists in the method of determination of boron, when it exists in small quantities mixed with many foreign substances.

I have also examined a certain number of mineral waters, which have given me negative results as to the presence of cæsium and rubidium. I will return hereafter to the results of their spectral analysis.

M. H. Troost prepared, some years ago, in the laboratory of the Ecole Normale, several kilogrammes of lithia salts. He operated upon about 100 kilogrammes of lepidolite of Bohemia, of petalite from Uto, and of triphylline from Finland. He took the precaution to preserve intact all the residues derived from the attack of these different minerals—residues which amount to a considerable bulk of material, which he has kindly placed at my disposal with a generosity for which I here beg of him to accept my thanks.

With these residues I have been enabled to prepare considerable quantities of the two new alkalies. I was struck, in performing the analysis of these last-named materials, to find cæsium and rubidium present in almost equal quantities. I have performed the same experiment on the products of a special attack of lepidolite which was used by M. Troost. This lepidolite was furnished to the Ecole Normale by M. Batka, of Prague, and it differs essentially by its richness in cæsium from the lepidolite of Rozena, analysed by MM. Kirchhoff and Bunsen, who only found in it rubidium with but traces of cæsium.

Finally, among the artificial products which I have examined, are some residues of the manufacture of saltpetre obtained from a Paris refinery. M. Captain Caron extracted from it a platinum salt, which he has been good enough to send to me, and in which I have determined the presence of considerable quantities of the new metals, and in almost equal proportions. I have likewise submitted to spectrum analysis the platinum salt obtained on treating some residues in the manufacture of Belgium saltpetre, in which I have recognised much rubidium, but no trace of cæsium. However, Chili nitrate of soda, as MM. Kirchhoff and Bunsen have stated, and as I have myself verified on specimens met with in commerce, only contains soda and traces of potash.—*Comptes-Rendus.*

TECHNICAL CHEMISTRY.

On the Coloured Derivatives of Naphthaline, by M. Z. ROUSSIN.

IN a former paper I stated that by the reaction of metals and of carbon on binitro-naphthaline dissolved in concentrated sulphuric acid an intense bright red colouring matter is produced, analogous in all its various properties, and even in the formula of its formation, with the colouring principle of madder (alizarine or purpurine). The following table exhibits this similitude:—

Colouring matter of madder

Is precipitated in the form of jelly from its solutions. Sublimates between 215° and 240°. But little soluble in water; soluble in alcohol, ether, and solution of alum.

Unalterable by sulphuric acid heated to 200°, and by hydrochloric acid. Alterable by nitric acid.

Soluble in caustic or carbonised alkalies, to which it gives a purple colour.

Ammoniacal solution yields purple precipitates with salts of baryta and lime.

Artificial red matter

Is precipitated in the form of jelly from its solutions. Sublimates between 215° and 240°. But little soluble in water; soluble in alcohol, ether, and solution of alum.

Unalterable by sulphuric acid heated to 200°, and by hydrochloric acid. Alterable by nitric acid.

Soluble in caustic or carbonised alkalies, to which it gives a blue-violet colour.

Ammoniacal solution yields purple precipitates with salts of baryta and lime.

Two dyeing experiments conducted by M. Balard with this colouring matter gave results differing from those obtained with the alizarine of the madder. Thus, to quote a single fact, the reds obtained from the madder become brighter under the influence of soap, while those of the new product change to violet under the same circumstances.

The elementary analysis furnishes the following figures:—

Carbon . . .	63·26	63·51
Hydrogen . . .	2·1	2·3

This substance is not nitrogenised. The formula of alizarine requires:—

Carbon	68·96
Hydrogen	3·45

The formula of purpurine requires:—

Carbon	66·67
Hydrogen	3·70

I am convinced that the new product is a derivative very nearly allied to alizarine or purpurine of madder, and that these inquiries will probably lead to the resettlement of the properties and composition of the colouring principle of this root. Meanwhile I am proceeding with my researches.—*Comptes-Rendus.*

On the Oxides of Iron and Manganese and Certain Sulphates considered as Carriers of Oxygen of the Air to Combustible Matters. Artificial Production of a new Cement acting in the Cold from the Residuums of Soda Manufactories, by M. Fred. Kuhlmann.

I believe I have before proved that when a spot of rust is produced on iron this spot causes a corrosion which gradually penetrates to the interior of the metal; that the extension of the spot is not the result of direct combination of new parts of the metal with atmospheric oxygen, or oxygen resulting from the decomposition of water; but that it is the result of a more complicated cause, in which the oxide of the first formation is the principal agent. The parts of the metal in contact with the peroxide take away a third of its oxygen, forming at its expense protoxide of iron, which by a subsequent absorption of oxygen borrowed from the air passes in its turn to the state of peroxide.

It results from this succession of effects that the peroxide of iron is in an unstable and transitory state of equilibrium, by turns partially reduced and re-oxidised. For the production of this succession of reductions and oxidations it is necessary that the oxide should be in the state of peroxide, for were it only in the state of magnetic oxide the oxidation would not spread to fresh parts of the metal. A covering of magnetic oxide, instead of inducing alteration, would be, on the contrary, a most efficacious preservative against oxidation. Such is the opinion of M. Thiraut, of Saint-Etienne, who first succeeded in utilising this property. His process consists in rusting the surface of iron artificially, and then changing the peroxide to the state of magnetic iron, probably anhydrous, by plunging the oxidised pieces into water at 80° or 100°.

"Under these circumstances," says M. Thiraut, "a new phenomenon is produced. No more peroxide is formed. In fact, the peroxide itself is modified and magnetic oxide Fe_3O_4 is formed. This magnetic oxide being almost unalterable, and no longer forming a voltaic element with the iron, the metal when covered is preserved from oxidation."

Some experiments previously described by me support this opinion, and show that magnetic oxide is the most stable of all the oxides of iron, best resisting in contact with the water the deoxidising action of certain bodies and the oxidising action of others. Moreover, this is the opinion already generally accepted by geologists.

I have recently applied the oxidising properties of sesquioxide of iron to a subject which forms a sequel to my researches touching the hygienic improvement of chemical manufactories.

After having studied the condensation of acid vapours, which in these manufactories are too often allowed to escape into the air, to the great prejudice of vegetation, and having advised, as a complementary means of condensation, the employment of natural carbonate of baryta, witherite, I turned my attention to the means of freeing these manufactories of chemical products, from the acid residues proceeding from the production of chlorine. These various researches have given rise to an industry altogether new—that of the wholesale fabrication of salts of baryta by processes so economical that several of these salts, before employed only as reagents, have been put to the most important uses.

Oxysulphide of calcium, residue from the washing of rough soda, has also particularly engaged my attention.

Many attempts have already been made to utilise profitably the sulphur of this oxysulphide. Without exception, all have hitherto failed on account either of the complication of practical processes or of the expense of the application of these processes. Thus the residues of soda have continued to embarrass our manufactories by quickly accumulating in considerable masses and emitting fetid emanations which spread through the air to some distance.

Spontaneous combustion often takes place on various points of this mass of residue, and then a large quantity of sulphurous acid is added to the constant disengagement of hydrosulphuric acid. These local combustions, which give rise to a high degree of heat, are made manifest to the eye by octahedral crystals of sulphur, like those of the solfateros which are deposited at the orifice of volcanic fissures, where hydrosulphuric acid is decomposed by sulphurous acid. In the interior of a mass of residues which for several years has been exposed to the air are found cavities, or *géodes*, adorned with beautiful gold-coloured crystals, whose composition may be represented by a combination of one equivalent of sulphite of lime, two equivalents of sulphide of calcium,

and six of water. Exposed to the air, these crystals lose their yellow colour, and they whiten as oxidation progresses.

My first experiments with the view of utilising the residues of soda were devoted to the decomposition of these oxysulphides by the chlorine residues, after saturating them by means of chalk. In effecting this reaction in a reverberatory furnace, a fritted mass is obtained, which when washed yields very pure chloride of calcium. As yet this chloride has been but little used for manufacturing purposes; and sulphide of manganese, another product of the re-action, I have hitherto used only for the construction of footways.[1] A remunerative utilisation of the residues of the washing of crude sodas was yet to seek, when it occurred to me to test the value of another residue not less embarrassing—oxide of iron, resulting from the combustion of pyrites, which in the fabrication of sulphuric acid has lately been pretty generally substituted for sulphur on account of the dearness of the latter mineral.

I naturally supposed that if the action of oxide of iron as a combustible is energetic enough to burn organic bodies, it must also suffice to burn the sulphur of oxysulphide of calcium, and to transform this oxysulphide into sulphate of lime.

These expectations have been satisfactorily realised. I mixed equal parts of the residues of soda just as it came from the vats, and of the residues of the combustion of pyrites, and make of them a homogeneous paste by crushing them under vertical grinding wheels. By moulding this paste while cold into the form of bricks or architectural ornaments, I obtained by a speedy consolidation substances as hard as baked bricks, which became harder the longer they were exposed to slightly humid air, and in the end acquired great sonority. The colour is a brown red, like earthenware.

When the new cement has been sufficiently hardened by several months' exposure to the air, it resists the action of frost, especially if, in the first instance, it has been rendered less porous by compression. For greater security against the action of extreme cold, the surface of this kind of pottery should be washed with a solution of silicate of potass, but only after solidifying for some time in the air.

Residues of soda recently obtained give better results than those which have been long exposed to the air. In all cases, the addition of a tenth part of slaked lime to the mixture of the two residues is a great improvement.

I trust that manufacturers of artificial soda will generally benefit by the results of my observations on this subject. I am convinced they will be found not only a means of economically freeing factories of two inconvenient and cumbrous residues, but of giving to them a value, whether they are employed in a concrete state for road mending, for stonework foundations, or for the constructions themselves, to the replacing of walls *en pisé* to the making of bricks, architectural ornaments, mosaic flooring, or objects modelled on the spot.

There are, in fact, an infinite number of building purposes in which the new cement may supersede lime, plaster, and mortar. If used as bricks, they can be connected together by the cement itself serving as mortar.

For agricultural purposes, residues of soda, treated by my method of oxidation, will be found of profitable application where lime alone would have a salutary effect. Lime which has been used for purifying gas is equally good.

As to the theoretical question touching this transformation, it has no further difficulty from the moment it is shown with what facility oxide of iron transports atmospheric oxygen to combustible matters by a chemical *shuttle movement* on which I have sufficiently insisted.

The formula of the composition of oxysulphide of calcium (residues of soda) is generally $3\,CaS + CaO$. That of sesquioxide of iron being F_2O_3, if it were admitted that the oxygen of sesquioxide of iron ought exclusively to serve for oxidising oxysulphide of calcium, it would be necessary to employ 12 equivalents of sesquioxide before passing to the state of protoxide; but it has been seen that the reaction cannot be so interpreted. As soon as one equivalent of sesquioxide is transformed into two equivalents of protoxide, more sesquioxide is formed at the expense of atmospheric oxygen, which oxidises a fresh quantity of oxysulphide.

Sesquioxide of iron acts, then, under these circumstances continuously exactly the same as when it takes part in the combustion of organic matters.

The phenomenon of the oxidation of the residues of soda can also be proved in another way. As soon as sesquioxide of iron comes in contact with oxysulphide of calcium, instead of passing to the state of protoxide, the sesquioxide loses all its oxygen and passes to the corresponding state of sulphide. This sulphide is gradually transformed by contact with the air into sulphate of iron, which yields its sulphuric acid to lime, whence sulphate of lime and oxide of iron. The final result, however, is always the same. It is always the atmosphere which yields the oxygen necessary to burn the sulphur of oxysulphide of calcium. No doubt more complicated reactions than simple oxidation take place in the new cement. There are some points of resemblance between it and iron cement. Some modifications are perceptible on breaking cakes which have been prepared for some months. The exterior becomes denser by time, and has a different molecular arrangement. This gradually reaches the centre.

It remains for me to complete this inquiry on the oxides of iron and manganese considered as means of oxidation to connect in a *résumé* the result of my former and recent investigations, and to determine the relation existing between the phenomena of combustion, of nitrification, of the fertilization of the ground, of decoloration, and of disinfection.

This *résumé* will be the object of an early communication to the Academy.—*Comptes-Rendus.*

[1] I have sometimes applied the residues of soda to the same purpose, but it cannot be used where the roads are bordered with trees, as it is pernicious to vegetation. In Schœningen's factory, near Brunswick, a fence in the form of a thick wall has been made of these residues compressed, but it is not very substantial.

PHARMACY, TOXICOLOGY, &c.

Preservation of Proto-Iodide of Iron, by M. Vezu.

Since M. Blancard made the use of pills of iodide of iron general, by indicating a sure means of preserving them, many persons, in whose hands his formula did not succeed, have proposed other methods, now forgotten. M. Vezu proposes to shield the iodide from contact with air, by dissolving it in a fatty body—cocoa-nut butter. To four parts of iodine, dissolved in the melted butter of cocoa-nut, he adds six parts of reduced iron, and keeps the mixture in a semi-liquid state for three or four hours, until it takes a bottle green hue, and does not colour moistened starch paper, when a thin layer is spread

over it. According to M. Vezu, the iodide dissolves in butter of cocoa-nut without sensibly attacking it.—*Repertoire de Pharmacie.*

On Chinese Chemistry and Medicine, from a Letter from R. P. Hellot *to M.* Barreswil.

The chemistry of the Chinese is a subject which had already excited my curiosity. I expected to find something in the books of medicine or pharmacy, or in the drug shops themselves,—for in China physics and chemistry are absolutely unknown—but my expectations were unfulfilled, for in the books I only found a series of receipts analagous to those of our ancient medicine; the basis of these receipts is almost always roots, gums, stalks of simples; and it is remarkable that the more elements it contains, the better is the receipt; this makes it exceedingly difficult to distinguish the efficient element. Furthermore, the Chinese theory of medicine is very limited. For this reason it is difficult to find out from it the effect expected to be produced by each medicament, which would enable one to understand a formula. Another difficulty is the identification of the plant which enters into the receipt, which is not easily done. There are many books, or kinds of dictionaries, wherein the names of Chinese plants are translated into English, Latin or French; but as regards the plants not exported by Europeans, these books afford no reliable information.

You will now understand why the work I commenced has not been finished. I could only translate formulas of which I understood as little as did the Chinese doctor who assisted me. "This is not the way to proceed," was the answer the good fellow made to my questions; "these plants are known to the apothecary; take this written formula to him, he will give you each of the substances indicated, in packets, with no mistake in the weights; then by boiling or infusing them according to the prescription, you will have a good medicine." I went myself to an apothecary's; it was a plain shop, with simples broken into little morsels, no laboratory and scarcely any medicaments from the mineral kingdom. . . . . Everything of any importance which could be learnt from these formulas is already known to, and has been translated by, former missionaries; and if Chinese pharmacy is but little considered in Europe, it is, in fact, because it is so difficult to get at the principal substance in the receipt, in order to study it chemically, as we study simples and roots. What may be done when the opportunity occurs, is to observe their manufacturing processes, as I have already done in the case of Chinese green. But for this we must see with our own eyes, examine and follow the operations, which can only be done when a fitting occasion presents itself. As for myself, I not only miss no opportunity, but seriously pursue my researches, and if I make any discovery, I shall have great pleasure in communicating it to you . . . ."

PHYSICAL SCIENCE.

On the Solidification of Certain Substances, by M. Dufour, *of Lausanne.*

In a recent communication, I showed that water held in suspension in the midst of a liquid of equal density would reach a degree of cold below 0° without freezing. It was to be inferred that other bodies under similar conditions behave in the same manner. The following are three examples:—

Sulphur.—It has been already shown by MM. Person and Faraday that it is possible to preserve this body in a liquid state below 115°—an exceptional phenomenon authors rarely mention. It is easy to prepare a solution of chloride of zinc of the same, or of a density rather greater than that of liquid sulphur. This solution can be heated without boiling beyond 115°, and balls of sulphur floating in it are melted. In order to keep these balls surrounded with a fluid, pour a layer of oil on the solution. On cooling it is very seldom solidified at the melting temperature. The liquid globules generally reach 70°, 50°, &c., before becoming solid. Solidification is spontaneous, or can be produced by contact with a solid body, particularly with a fragment of sulphur; but under the peculiar circumstances of these experiments, the liquid state of the body is remarkably persistent. It is sometimes possible to introduce at 60° into globules, six millimètres in diameter, saline crystal wire, &c., without inducing immediate solidification. Globules half a millimètre in diameter frequently remain fluid at a temperature of 5°, and continue in this state for many days.

When these balls of sulphur again become liquid at 50° or 60° below their ordinary solidifying temperature, it becomes exceedingly interesting to watch their change of state. A transparent dark red liquid mass is suddenly transformed into a hard, yellow, and opaque substance. This clear and easily performed experiment is eminently calculated to show the curious phenomenon of superfusion.

Phosphorus.—M. Desains has already described the preservation of this body in a liquid state at a temperature below 44°.

The same method is applicable to both sulphur and phosphorus. The solution of chloride of zinc of the proper density is covered with a layer of oil to keep it from contact with air. The liquid and transparent globules of phosphorus will be clearly visible, and will remain liquid below 44°. Globules from half to two millimètres in diameter easily reach 5° and even 0°. Phosphorus in its liquid state is also remarkably stable, and its change of condition gives rise to phenomena analogous to those which sulphur exhibits.

Naphthaline.—Its fusion and solidification generally take place at 79°. This body has evidently the same density as water, but it is, however, somewhat less in the liquid state. With proper precautions, the phenomenon of superfusion will also take place. The body must be melted in a globe quite full of boiled water. Then incline the balloon so that the liquid naphthaline may come to the top of the vessel, and slightly pressed against the sides of the glass. Owing to the very little difference of density, the liquid takes an obviously spherical form, and does not adhere to the side of the glass. I have also seen globules eight millimètres in diameter remain liquid to 55°.

Many other bodies under fitting circumstances would probably present a phenomenon similar to that which is so marked in the case of water. It is unfortunately difficult with numerous bodies to realise the essential condition, which is to exceed the ordinary temperature of change of state, during the time the body is suspended in a liquid of the same density. The liquid used as a medium should realise the four following conditions:—It should have the same density as the body experimented upon, it should remain liquid above and below

its fusing temperature, and should neither penetrate the body nor exercise on it any chemical action. Notwithstanding these requirements, I have no doubt that means will be devised to apply successfully to yet other substances the method which succeeds so easily and surely in retarding the solidification of water, sulphur, and phosphorus.—*Comptes-Rendus.*

PROCEEDINGS OF SOCIETIES.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, December 10, 1861.

Dr. J. P. JOULE, *President, in the Chair.*

Mr. Wm. K. Deane was elected an Ordinary Member.

Mr. BAXENDELL made the following communication :—

A paragraph, headed "*Rain following the Discharge of Ordnance,*" appears in the number of the *London Review* for November 16, 1861. In this paragraph some new facts, drawn from the American war, are adduced by Mr. J. C. Lewis, in support of the view that a violent concussion of air by the discharge of heavy artillery has a tendency to cause a copious precipitation of rain. Now, if we may be allowed to regard this effect as an established fact, it seems to me to be one of some interest in connexion with the disputed question whether, in thunderstorms, a discharge of lightning is the cause or the consequence of the sudden formation of a heavy shower of rain. Almost every day's experience, in this climate at least, shows that the production of rain is not dependent upon sudden discharges of electricity from the clouds; and no evidence has ever been brought forward to prove that a high degree of electrical tension in a cloud has a tendency to prevent the resolution of the cloud into rain. Heavy showers often fall from highly electrified clouds without any visible discharge of electricity taking place. We are, therefore, not entitled to assume that the sudden diminution of the electrical tension of a cloud by a lightning discharge can have any material influence upon the rain-forming processes going on in the cloud. As, however, very heavy showers of rain do almost invariably follow lightning discharges, it seems necessary to seek some other cause to account for them. But if we admit that a violent concussion of the air has a tendency to facilitate the conversion of rain-forming material into actual drops of rain, then we may well suppose that the violent concussions produced by lightning discharges, acting on such enormous and dense masses of rain-forming material as are usually collected in heavy thunder clouds, are amply sufficient to produce these sudden and heavy showers of rain. I am aware that the effect of a discharge of ordnance is usually supposed to be produced by an upward current of air caused by the heat and the gases evolved during the combustion of the gunpowder; but as an hour's sunshine through an opening in the clouds, especially when the sun is at a considerable altitude, would produce a much greater effect in heating and increasing the bulk of the air, this cannot be received as the true explanation of the mode in which the effect of a discharge of heavy artillery is produced.

Mr. FAIRBAIRN stated that he had been making experiments on the process of cold rolling, as applied to iron. He had tested specimens of cold rolled iron manufactured both by Mr. Lauth and Earl Dudley. In the former case, a black bar from the rolls broke with 26·173 tons per square inch, a similar turned bar with 27·119 tons, and a cold rolled bar of the same iron sustained 39·388 tons. The elongations, which may be considered as the measure of ductility, were ·200 and ·220 per unit of length in the case of the ordinary iron, and ·079 in the cold rolled iron. A plate of cold rolled iron, from Earl Dudley, sustained no less than 51·3 tons per square inch. Endeavours were being made to apply the invention to railway bars.

Mr. BROCKBANK described the Bessemer process of manufacturing iron and steel, and stated his belief that the variously-coloured flames on the surface of newly run steel would afford the means of detecting the presence of metals and other bodies by the new method of spectrum analysis.

A Paper entitled, "*Nouveau Système de Communication Télégraphique, rendant impossible toute collision de trains sur les chemins de fer,*" by Professor Baulet, of Perpignan, communicated by William Fairbairn, Esq., LL.D., &c., was read by Professor ROSCOE.

In this plan an insulated wire placed between the rails, and divided in its middle, affords a connexion between the instruments at the stations and others situated in the trains themselves. The details of the arrangement could not be understood without the drawings accompanying the Paper.

Mr. DODWELL, the Superintendent of the Magnetic Telegraph, described Mr. Clark's system, which is now in full operation between London and Rugby, and which, he thought, left little further to be desired.

PHYSICAL AND MATHEMATICAL SECTION.

October 10, 1861.

G. V. Vernon, Esq., F.R.A.S., was elected a Member of the Section.

A letter from Mr. HEELIS, F.R.A.S., having been read, resigning the Secretaryship of the Section on account of the state of his health, it was resolved unanimously,—That the Section receive with regret the resignation, by Mr. Heelis, of his office as Secretary, and sincerely hope that his health may soon allow of his resumption of official connection with them.

Mr. VERNON, F.R.A.S., read a Paper, "*On the Irregular Barometric Oscillations at Geneva and on Great St. Bernard, and their Relations to Mean Temperature and the Fall of Rain.*"

November 7, 1861.

Professor Clifton, B.A., F.R.A.S., was elected Secretary of the Section, in place of Mr. Heelis, resigned.

A letter from Mr. HEELIS, F.R.A.S., was read, communicating the following observation of the Zodiacal Light, recently made by him at Smyrna. "September 13th, 1861, 4h. a.m. Observed the Zodiacal light very distinctly; the breadth of it was not observed, but the position of the apex was ascertained, as carefully as an eye unaccustomep to such observations would permit, to be in a line and about midway between Pollux and Procyon. This would give an approximate length of 75° 11′. The cluster Præsepe was seen distinctly through the light shining like a white cloud, and many meteors about equal in light to the brightness of any point in the Præsepe, (by which I would be understood to mean not the aggregate light of the cluster, but the light which any part of it, if separated from the rest would appear to have,) were seen crossing the beam of light in various directions. The sky was quite clear the morning just dawning, and the horizon to seaward covered, as is usual at that hour in summer at Smyrna, with a low fog bank. Few or no meteors were at the time of observation noticed in any other part of the sky."

Mr. W. L. DICKINSON read a Note "*On the Transit of Mercury,*" giving the results of his calculations for Manchester. In these calculations he had used the Nautical Almanac Elements, and found that the planet would leave

the Sun's disc on the morning of the 12th instant, at 9h. 18m. 44s., Greenwich mean time, at an angle from the north point of the Sun's disc of 24° towards the west, and from the vortex of 1° towards the west for direct image.

Mr. Vernon, F.R.A.S., exhibited diagrams, showing the variations of the barometer and thermometer at various stations in England and Scotland during the storm which occurred on the 2nd instant.

December 5, 1861.

Murray Gladstone, Esq., F.R.A.S., was elected a Member of the Section.

Mr. Baxendell, F.R.A.S., read a Paper "*On the Influence of the Seasons on the Rate of Decrease of the Temperature of the Atmosphere with Increase of Height in different Latitudes in Europe and Asia.*"

NOTICES OF PATENTS.

750. *Colour.* F. Versmann, Bury Court, St. Mary Axe, London. Dated March 25, 1861. Not proceeded with.

This specification relates to the economical employment of the compounds of titanium as pigments or colouring agents.

It is to be regretted that the sources of colouring matter here referred have not yet been practically utilised. The sources of titanium are by no means rare, titanic acid is found tolerably pure in the mineral rutile, and titaniferous iron is actually abundant. The chemical reactions of titanium give rise to a fair proportion of coloured products, —among others there is the pale blue oxide, somewhat similar to the corresponding tungsten compound, this appears, however, to be of an alterable nature. If titanic acid be heated in a porcelain tube through which a current of dry ammonia gas is passed, it is converted into a violet powder of a permanent character, having the composition TiN. Other nitrides of titanium have been prepared by the action of the same gas upon the bichloride, when brilliant purple scales are formed, which in turn yield a lustrous yellow product by afterwards passing hydrogen through the tube.

As long since as the year 1846, Elsner proposed the substitution of "titanium green" for the poisonous arsenical pigment commonly used in the manufacture of paper hangings; we are not aware that this material—the ferrocyanide of titanium—has ever met with any extensive application, but it may be of interest to repeat here M. Elsner's original directions for its preparation:—Washed rutile, or iserine, is decomposed with twelve parts by weight of bisulphate of potassa in a Hessian crucible; the fused mass is pulverised, digested at 50 degrees Centigrade with hydrochloric acid previously diluted with twice its weight of water, and filtered. The filtrate is evaporated until a drop solidifies upon a cold plate; at this stage it forms, in the evaporating dish, a paste which is well drained upon a filter, and then continuously boiled in a porcelain basin with solution of sal-ammoniac in order to prevent the formation of a basic iron salt. This process and subsequent washing and filtering renders the difficultly soluble titanic acid almost white, and for the purpose of preparing the ferrocyanide of titanium the pasty mass obtained in the above manner is digested, at about 50 or 60 degrees, with dilute hydrochloric acid until, if possible, complete solution ensues, and the acid liquid after the addition of ferrocyanide of potassium rapidly heated to ebullition. The beautiful green precipitate is washed with water acidulated with hydrochloric acid, and dried with great care, since it decomposes at temperatures above 100 degrees. The colour, however, is certainly inferior to the arsenical green.

777. *Shear Steel.* R. A. Brooman, Fleet Street, London, A communication. Dated March 28, 1861.

For the purpose of preventing the access of atmospheric air, and the gases from the fire, operating injuriously on the puddled steel during the heating process to which it is subjected as a preliminary to welding, it is directed to enclose the piles of raw steel in a covered crucible or other conveniently-shaped receptacle, such as a clay retort or muffle, luted at all the joints, with exception of a small aperture left open to enable the temperature of its heated contents, being determined by inspection. The welding heat approaching so nearly that of the melting point of steel, necessitates constant watching, so that the metal may be withdrawn from the furnace at the proper moment, and hammered or passed between the rollers. Such an arrangement offers facilities for long-continued heating of the steel before rolling, by which its working qualities are said to be much improved, the deteriorating action of oxygen being at the same time excluded.

Grants of Provisional Protection for Six Months.

2694. William Smith, Leek, Staffordshire, "Improvements in the preservation of stone, brick, and other such materials used in building, applicable also to the waterproofing of walls."—Petition recorded October 26, 1861.

2710. Richard Gibbon, Royal Brewery, Brentford, Middlesex, "Improvements in machinery or apparatus for preparing grain for brewers."

2810. Aristide Balthazard Berard, Avenue Montaigne, Paris, "Improvements in apparatus for separating metals from their ores."—Petitions recorded November 8, 1861.

2845. Michael Henry, Fleet Street, London, "An improvement in, and composition for, treating iron and steel, and articles manufactured thereof."—A communication from Alphonse Muller de la Mothe, Boulevart St. Martin, Paris.—Petitions recorded November 12, 1861.

2846. Thomas Littleton Holt, Brook House, Brentford, Middlesex, "A new method of making paper from the cochleria armoracia, or horse radish."

2863. George Tomlinson Bousfield, Loughborough Park, Brixton, Surrey, "Improvements in the manufacture of soap."—A communication from Campbell Morfit, New York, U.S.

2871. Frederick Robert Hughes, Borrowstounness, and Thomas Richardson, Newcastle-upon-Tyne, "Improvements in treating certain natural saline compounds to fit them for agricultural use, and in order to obtain potash and other salts."—Petitions recorded November 14, 1861.

2879. Louis Anthony Soupart, Brussels, Belgium, "Improvements in the mode of preparing and subsequently tanning hides or skins."—Petitions recorded November 15, 1861.

2903. Theophilus Redwood, Montague Street, Russell Square, London, "Improvements in the manufacture of starch and of a vegetable sizing powder."

2911. George Gwilliam, Savoy, Strand, London, "Improvements in the manufacture or production of plate glass."

2920. John Henry Johnson, Lincoln's Inn Fields, London, "Improvements in the treatment of zinc ores, and in the apparatus employed therein, which improvements are also applicable to the manufacture of phosphorus."—A communication from Adrien Muller, Paris.—Petitions recorded November 20, 1861.

2922. James Parkinson and Charles Henry Minchen, Manchester, "Improvement in the 'Davy' or other safety lamps for miners."

2926. John Stubbs, Winsford, Cheshire, "Improvements in apparatus for heating and evaporating brine in the manufacture of salt."

2930. William Hirst, Halifax, Yorkshire, "Improvements in the manufacture of paste, which is also applicable to sizing purposes."

2952. Jean Baptiste Hulard and Louis Guillaume Poupel, Paris, "An improved process for hardening stones and plaster of Paris, and making them impervious to water."

2988. Horatio Mearing, Great Randolph Street, Camden Town, London, "An improved lucifer match, and prepared paper for igniting the same."

2994. Michael Henry, Fleet Street, London, "Improvements in the manufacture of soap, and the preparation of materials for the purpose."—A communication from Louis Marie Théophile Riot, Boulevart St. Martin, Paris.

2127. Frederick Tolhausen, Boulevart Bonne-Nouvelle, Paris, "A new and economical method of producing dynamic electricity, thereby obtaining useful chemical compounds."—A communication from Jean Baptiste Théodore Rousse, Imperial Lyceum, St. Etienne, France.—Petition recorded August 26, 1861.

2351. John Oliver, Colchester, Essex, John Granthan, Nicholas Lane, London, William Sinnock, Sylvan Cottage, Woodford, Essex, and Montague Richard Leverson, St. Helen's Place, London, "Improvements in the mode of obtaining certain chemical substances, and in the treatment of vegetable fibre, and in obtaining manurial and other products therefrom."—Petition recorded September 20, 1861.

Notices to Proceed.

1893. Wentworth Lascelles Scott, Bayswater, London, "Improvements in preparing red, purple, and certain other dyes."—Petitions recorded July 29, 1861.

2810. Aristide Balthazard Berard, Avenue Mentaigne, Paris, "Improvements in apparatus for separating metals from their ores."—Petition recorded November 8, 1861.

2831. George Fergusson Wilson and George Payne, Sherwood Works, Battersea, Surrey, "Improvements in treating fatty and oily matters."—Petitions recorded December 11, 1861.

2863. George Tomlinson Bousfield, Loughborough Park, Brixton, Surrey, "Improvements in the manufacture of soap."—A communication from Campbell Morfit, New York, U.S.—Petition recorded December 13, 1861.

MISCELLANEOUS.

ROYAL INSTITUTION OF GREAT BRITAIN.

WE have great pleasure in announcing to our readers that, having received Professor Tyndall's special permission to report his Course of Six Lectures "On Light" (adapted to a juvenile auditory), now in course of delivery at the Royal Institution, the First Lecture will appear, fully illustrated, in our next Number.

The following are the subjects of the course:—

The Propagation of Light—The Reflection of Light from Plane Surfaces—The Reflection of Light from Curved Surfaces.

The Refraction of Light—Prisms and Lenses.

The Solar Microscope—The Eye.

Combined Action of Refraction and Reflection—Mirage.—Colours: Analysis and Synthesis of White Light.

The Solar Spectrum—The Spectra of Incandescent Metallic Vapours.

Elementary Facts of Interference—Elementary Facts of Polarization.

Polytechnic Institution.—The Christmas programme of this old and deservedly popular scientific Institution includes several new features of interest. The tastefully arranged decorations in the hall and galleries, disposed just now in accordance with the custom of the seaaon, and the gigantic Christmas tree, present a pleasing appearance of winter gaiety, while the exhibition of works of art, the machinery in motion, models and other illustrations of the progress of invention, seem to complete the charm which always attaches to a visit to the Polytechnic. Among the novelties which have been introduced, foremost should be mentioned the magnificent series of dissolving views illustrating American scenery. The Falls of Niagara and surrounding country in its wintry aspect, its caverns and awful precipices clothed with icicles so stupendous in magnitude that were it not for the actual photograph being thrown upon the screen, doubts might be entertained as to the faithfulness of the representation. The instantaneous view taken in the Broadway, New York, is a good illustration of American life and commercialactivity, so also is the railway-station, in which is seen the extraordinary locomotive engine, so different in figure from those used in our own country, on account of wood being there chiefly employed as fuel. The entire series, indeed, reflect great credit upon the energetic exertions of Mr. Eagland, who crossed the Atlantic with the special object of securing these interesting photographic representations of American life and scenery. The optical facilities of the Institution are likewise employed in the illustration of Mr. Pepper's Lecture on the Science of the Armstrong, Whitworth, and other rifled guns; in the description of England's dockyards and iron-clad ships of war; and, with the assistance of Mr. Childe's phantasmagoria apparatus, for the performance of an optical version of Grimaldi's pantomime, entitled, Mother Goose and the Golden Egg, in the course of which the clever shadow effects, introduced so successfully at the Crystal Palace, are exhibited on the large screen. Some of the recent results of experimental science are very ably and popularly treated by Mr. Pepper, in his Lecture on the "Magnificent Field of Discovery Opened out by the Researches of Professors Bunsen and Kirchhoff." With the aid of a Duboscq's lamp, in which the various metallic compounds are submitted to the heat of the electric arc between carbon points, the differently coloured spectral bands are beautifully exhibited upon a large white disc. The opacity of the yellow sodium flame to light of its own colour was that experiment alone which was less conclusively exhibited in this manner, apparently from its requiring extreme delicacy of manipulation. We never remember to have seen so many interesting novelties within the same compass of time and space as that which composes the Christmas entertainment at the Polytechnic Institution.

Royal Institution.—On Tuesday, January 7, at three o'clock, Professor Tyndall will deliver a lecture "On Light" (Juvenile Lectures).

ANSWERS TO CORRESPONDENTS.

*** All *Editorial Communications* are to be addressed to the EDITOR; and *Advertisements* and *Business Communications* to the PUBLISHER at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

*** In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

Vol. IV. of the CHEMICAL NEWS, containing a copious Index, is now ready, price 12s., by post, 12s. 8d., handsomely bound in cloth, gold-lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. A few copies of Vols. I. II. and III. can still be had. Vol. V. commenced on January 4, 1862, and will be complete in 26 numbers.

O. R., Ph.D.—Answered in our last Number.

C. R.—The oxide of iron, not being a conductor of electricity, will not be decomposed.

J. Horsley.—Received. We shall be glad to receive the communication spoken of.

ERRATA.—"Analysis of Yeast," page 338, for *Scheikundize Onderhoekinzer* read *Scheikundige Onderzoekingen.*

THE CHEMICAL NEWS.

VOL. V. No. 110.—January 11, 1862.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Researches on the Relations existing between Atomic Weights, by M. J. S. STAS, *Member of the Royal Academy of Belgium.*

(*Continued from page* 2.)

Analysis of Sulphate of Silver.—I endeavoured to perform the synthesis of sulphate of silver, but was obliged to renounce this project on account of the mechanical carrying off of the silver when I effected the solution of the metal in a platinum vessel, and of the impossibility of finishing the desiccation of the sulphate in a platinum vessel when I effected the solution of the silver in a Bohemian glass flask, as I did in the case of sulphate of lead. I was, therefore, obliged to be satisfied by performing its analysis, which would only determine the amount of silver contained therein. I determined this by reducing the sulphate with hydrogen, as has been already done by M. Struve.

I employed in these experiments three specimens of sulphate of silver. One I obtained by pouring into dilute and boiling sulphuric acid, contained in a platinum vessel, a solution of pure nitrate of silver. The liquid being cooled, the mother-liquor was removed and replaced three separate times by water acidulated with weak sulphuric acid, and the whole brought each time to ebullition and kept at this temperature for half-an-hour. The sulphate of silver deposited, upon cooling, was then washed in cold water until the washing-water came away quite neutral. Before using it in the experiment, I dried it at nearly 350° in a current of dry air, freed from all organic matter by passing through a glass tube filled with oxide of copper heated to redness. With this object I heated it in a Liebig's desiccator, placed in a bath of chloride of zinc.

To procure the second specimen of the salt, I attacked directly in a platinum dish, concentrated and pure sulphuric acid by means of silver prepared with sugar of milk, and in which I had found it impossible to discover any traces of foreign matter. When all the silver was transformed into sulphate I drove off, by heat, as much as possible of the acid in excess.

After cooling the vessel, I washed the salt in water, so as to remove the last trace of sulphuric acid which had escaped the action of heat. Then I heated it in the some platinum vessel much beyond the boiling-point of sulphuric acid. I then introduced it, quite warm, into a flask with a ground stopper previously heated and dried. Before using it in the analysis, I heated it again in the same tube to beyond the boiling-point of sulphuric acid.

Sulphate of silver is of great stability. Although it requires almost a dull red heat to fuse it, it can be liquified without sensible loss of weight, provided the operation takes place away from organic dust in the air and in a platinum vessel; for a glass vessel is attacked by fused sulphate of silver.

250 grammes of sulphate of silver obtained by this latter method were dissolved by ebullition in the water employed to wash the salt, acidulated with sulphuric acid. After the liquid is slowly cooled, the salt separates from it in the form of voluminous prisms, colourless and limpid as water. One of these prisms weighed 11·8 grammes. These prisms, when heated to 300°, decrepitate rather violently. They can then be heated to above the boiling-point of sulphuric acid, and can even be fused, without causing the sulphate sensibly to lose weight.

Sulphate of silver prepared by the action of sulphuric acid on nitrate of silver, and by the action of this metal on concentrated sulphuric acid, appears in the form of a crystalline powder, of dazzling whiteness, even when it has been heated to 350°, provided that during its desiccation a current of air perfectly deprived of organic matter has been made use of. Without this precaution, it assumes in the desiccating apparatus at first a green and then a violet tint. In this case, when dissolved in water, it leaves a blackish residue, which becomes silver-white at a red heat. Its pulverulent state obliged me to weigh it *in vacuo*.

This is the arrangement which I adopted to estimate the silver by means of the action of hydrogen:—I took a Bohemian glass tube, eighteen millimètres in diameter, and seventy-five centimètres long, drawn at one end. After having dried it at a high temperature, I introduced it into a large tube for weighing *in vacuo*, and made a vacuum in the apparatus, which was equipoised with another large tube similar to the first in its exterior volume and the nature of its surface. At the same time, I placed the necessary weights to bring it into equilibrium in the opposite pan of the balance.

I then filled the Bohemian glass tube two-thirds full of dried sulphate of silver, taking care to leave that third of the tube empty which was near to the drawn-out point. After having again heated the sulphate of silver enclosed in the tube in a current of pure, dry air, I immediately introduced it into the vacuum apparatus and removed all the air. When perfectly cool I weighed it.

Finally, I placed the tube containing sulphate of silver in communication with a large apparatus for the disengagement of pure dry hydrogen. To obtain this gas, I took all the precautions recommended by M. Dumas in his Memoir on the Synthesis of Water, only that I used fused caustic potash as the desiccating agent, wishing thus to avoid the presence of traces of sulphurous acid in the hydrogen.

This tube rested in an iron box entirely filled with magnesia, and very slightly inclined to the drawn-out end, which led into a large empty receiver. When the

apparatus was quite full of hydrogen, I heated to the boiling-point of sulphuric acid that part of the box corresponding to the empty portion of the tube. I then gently raised the temperature of that portion of the tube which contained the sulphate of silver. When I saw the reduction of the salt commence, I endeavoured as much as possible to keep the temperature uniform, so as to avoid attacking the glass and volatilising the silver. The decomposition of the sulphate of silver takes place with extreme nicety and regularity; it is a true substitution of hydrogen for silver. Indeed, if the temperature is kept sufficiently low, there is only silver and sulphuric acid produced, which latter runs down, owing to the inclination of the tube; scarcely any traces of sulphurous acid being smelt. In proportion as the sulphate of silver is reduced, the quantity of sulphuric acid produced becomes sufficient to arrive at that part of the tube which is heated to the boiling point of this acid. It is there reduced by the excess of hydrogen disengaged into sulphurous acid and water or into sulphur and water. Sulphuric acid is the only product which I have seen formed when the action of the hydrogen on the sulphate of silver has taken place at a low temperature. As the temperature is more raised, water and sulphide of silver are at first produced, and then metallic silver and hydro-sulphuric acid. In all the determinations of the results which are given below, I have endeavoured as much as possible to avoid the formation of sulphide of silver and the subsequent production of hydro-sulphuric acid. Nevertheless, I have always noticed the presence of hydro-sulphuric acid at the conclusion of all the reductions, when the tube was heated to very dull redness in the current of hydrogen. I have, indeed, only stopped the current when paper impregnated with a salt of lead, placed in front of the hydrogen disengagement tube, did not show the slightest trace of sulphuretted hydrogen. In all these determinations the residue of silver was in a spongy form, and of a dead white appearance. In order to be quite certain of the absence of all traces of sulphide of silver in the metal, I caused to pass through the tube, heated to dull redness, a current of air, in order to burn the sulphur. In only one experiment did I smell a sulphurous odour. When the operation was terminated, the commencement of the empty portion of the tube in which the reduction of the sulphuric acid into sulphurous acid and water took place was covered with a mirror of silver arising from the silver carried by the sulphuric acid in the state of sulphate. But there always remained at least twelve or fifteen centimètres of the tube which did not show the slightest trace of metallic deposit. The acid water condensed in the large receiver, which was in communication with the tube, contained traces of silver, but too small to be estimated.

The silver having been weighed, I always dissolved the metal in dilute nitric acid, to see if any sulphide of silver were present. In no experiment did I find any.

To render the control complete, I perfectly washed and dried the tube in which the reduction took place, and having weighed it again *in vacuo*, I found that it was always of its primitive weight, at least to within 0·002 gramme.

The sulphate of silver employed in the first series was prepared by the action of sulphuric acid on nitrate of silver. The salt employed in the second series was prepared by the direct action of silver on sulphuric acid. Finally, for the third series, I made use of the sulphate obtained by crystallising the preceding salt in water acidulated with sulphuric acid.

Analyses of Sulphate of Silver.

Numerical order.	Apparent weight of the Sulphate of Silver.	Actual weight of the Sulphate of Silver.	Apparent weight of the Silver.	Actual weight of the Silver.	100·000 parts of Silver yield Metallic Silver
	First Series.				
	grammes.	grammes.	grammes.	grammes.	
I.	72·141	72·137	49·922	49·919	69·200
II.	60·2545	60·251	41·6945	41·692	69·197
	Second Series.				
III.	81·028	81·023	56·074	56·071	69·204
IV.	83·120	83·115	57·5265	57·523	69·209
	Third Series.				
V.	55·7195	55·716	38·5615	38·5595	69·207
VI.	63·926	63·922	44·238	44·2355	69·202
				Mean ..	69·203

(To be concluded in our next.)

On the Oxalates of Iron, by EMERSON REYNOLDS, *Assistant Chemist to the Dublin Chemical Society.*

THE attention of chemists has recently been so much directed towards the vast field of organic research, that inorganic chemistry has been comparatively neglected, and errors which in former days would have been soon found out, now remain unnoticed until accident brings them to light. The object of the present paper is twofold,—first, to correct a mistake which has been made in the expression of the formula of the protoxalate of iron; and, secondly, to point out the theoretical considerations involved in certain reactions of the proto- and per-oxalates of iron.

For many reasons, it will be more convenient to consider them under their respective heads.

1. On the Correct Formula for Oxalate of the Protoxide of Iron.—A short extract from the *Comptes-Rendus* appeared in the CHEMICAL NEWS (No. 48, vol. ii.), noticing a paper by Dr. Phipson "On a New Combination of Oxalic Acid with Protoxide of Iron," to which the author assigned the formula $FeO,4C_2O_3$, calling it "quadroxalate of iron." Dr. Croft, of Toronto, it appears, criticised this paper in the *Canadian Journal of Science*, but his comments upon it, as well as the results of his experiments, instituted with a view to disprove some of the statements made by Dr. Phipson, have been reproduced only a few weeks ago in the pages of the CHEMICAL NEWS.[1] In June last an original communication from Dr. Phipson appeared in No. 80, vol. iii. of the CHEMICAL NEWS, in which he gives the results obtained by the combustion of the salt with oxide of copper. From the data thus found he now calculates the following formula, $FeO,3C_2O_3+4HO$, which differs from the one before proposed in representing the salt as being a teroxalate, with four equivalents of water, instead of the anhydrous quadroxalate. I think it will be apparent, from what I have said, that Dr. Phipson did not see the notice of his paper by Dr. Croft, at least before he wrote the second one; and, again, that Dr. Croft had not seen the second paper before writing his own. Dr. Croft, therefore, has shown that Dr. Phipson was not correct in calling the salt "quadroxalate of iron;" but what I wish to prove is, that it is not a teroxalate of iron, with

[1] See CHEMICAL NEWS, No. 106, vol. iv.

four equivalents of water, but a protoxalate, with two equivalents of water, being represented thus:— FeO,C_2O_3+2HO. Now, it is to be remembered that Dr. Phipson must have first estimated the oxalic acid by the loss sustained on igniting a given portion of the salt in air, calculating all the volatile matter as oxalic acid; but, in the second instance, he says, that the oxalic acid was determined by combustion, as carbonic acid and the water was directly estimated. It is evident that we must be very cautious in contradicting his later formula without sufficient evidence to ground our contradiction upon.

About a year and a-half ago, in the course of some experiments, I had obtained a solution of a per-salt of iron in stong alcohol. On adding oxalic acid, no precipitate was observed at first, but after being exposed to ordinary light for a few days, a yellow crystalline deposit was observed; an examination of the minute precipitate was sufficient to convince me that it was a compound of oxalic acid with protoxide of iron. Not finding any information on this subject in the ordinary "Handbooks," I referred to Berzelius' *Traité de Chemie*, and found that Döbereiner had noticed this remarkable reaction, which took place under the influence of the solar rays, many years ago.[2] Following up the subject, without any ulterior object beyond wishing to obtain some information respecting these salts, it occurred to me that the power which the actinic rays possessed of reducing the per-oxalate of iron to the state of protoxalate might be made use of in a photographic point of view. It would be out of place for me to speak now of the *modus operandi* of my process for printing by light based on this reaction, as a notice of it has already appeared in a late Number of this Journal.[3] It is sufficient to say that the results of the experiments which were necessary in working out the process have given me the means of establishing the correct formula for oxalate of the protoxide of iron. In the first place, then, the results of the analyses on which Dr. Phipson bases his original view regarding the composition of the salt are as follows:—

	I.	II.
FeO . . .	19·35	19·44
C_2O_3 . . .	80·65	80·56
	100·00	100·00

As I before remarked, the oxalic acid was calculated from the loss. If, however, we compare the quantity of protoxide of iron found with that obtained by other chemists, we may safely infer that Dr. Phipson has, in every instance, calculated the Fe_2O_3 obtained as FeO instead of Fe_2O_2, and, as a consequence, found only half the real quantity of FeO, as already pointed out by Dr. Croft. I merely draw attention to it here, as it will be necessary to bear it in mind hereafter.

For the determination of the protoxide of iron, and other experiments, three modes of preparing the salt were had recourse to. I. By precipitating ferrous sulphate with oxalate of ammonia. 2. By precipitating ferrous sulphate with oxalic acid (Vogel). 3. By treating solution of ferrous sulphate with normal oxalate of potash (Rammelsberg). Each of the salts produced in the above manner was washed and dried thoroughly, when the following results were obtained on analysis:—

	Theory.	I.	II.	III.
FeO . .	40·00	39·60	39·00	39·03

[2] An excellent *résumé* of the state of our knowledge with regard to the combinations of the oxides of iron with oxalic acid will be found in Gmelin's "Handbook of Chemistry."

[3] Chemical News, No. 105, Vol. iv. p. 304.

The numbers I. II. III. refer to the method adopted for preparing the salt used, as well as to the number of the experiment.

It being necessary for me to ascertain what was the nature of the reaction which takes place between the oxalate and ammonio-nitrate of silver, I found that the method pursued in the following experiments enabled me to determine the amount of oxalic acid contained in the salt, with the requisite accuracy.

22·5 grains of the oxalate of iron (I.) were taken and placed in a glass vessel; a known excess of ammonio-nitrate of silver was then added, upon which the mixture immediately became black, but no gas was evolved; after prolonged digestion it was then filtered, and the insoluble matter collected, dried, and weighed, when its weight was found to be 23·472 grains.

Now, if we consider that 22·5 grains of the oxalate are capable of affording 10·0 grains Fe_2O_3, and that a relative equivalent of silver weighs 13·5 grains, the sum 23·5 grains corresponds very closely to the experimental number 23·472. Therefore, we might conclude that one equivalent of oxide of silver yields up its oxygen to two equivalents of protoxide of iron, which are thereby converted into one atom of sesquioxide. This view is confirmed by actual experiment.

The filtrate from the insoluble residue was now taken, and nitric acid added until the liquid had a very slight but distinct acid reaction. In this way, the oxalate of silver held in solution by the excess of ammonia was thrown down. The precipitate, when collected and dried, was found to weigh 37·88 grains, which corresponds to 39·81 per cent. of oxalic acid contained in the iron salt.

The remaining two samples of the oxalate were treated in exactly a similar manner; the results obtained, which correspond with those of Döbereiner and Rammelsberg, are given in the following tabular form, in which, for the sake of comparison, the formula FeO, C_2O_3+2HO is doubled:—

	Theory.		I.	II.	III.
2 FeO .	72	40	39·60	39·00	39·03
C_4O_6 .	72	40	39·81	39·66	39·47
4 HO .	36	20	20·59	21·34	21·50
	180	100	100·00	100·00	100·00

The water was determined by difference. On carefully considering the conditions under which the above numbers were obtained, I think it is perfectly evident that the true formula for the salt is

$$FeO, C_2O_3+2HO$$

or, viewing oxalic acid as dibasic,

$$Fe_2O_2, C_4O_6+4HO.$$

On referring to Dr. Phipson's last paper, we find the following numbers as the results of two analyses of the yellow salt:—

	I.	II.	Theory.
Protoxide of iron . .	19·44	19·40	20·00
Oxalic acid (by diff.) .	59·56 (by combust.)	59·75	59·99
Water (direct.) . . .	21·00 (by diff.)	20·85	20·00
	100·00	100·00	99·99

This composition corresponds, therefore, with the formula

$$FeO, 3C_2O_3+4HO.$$

These results are obviously wholly irreconcileable with my own conclusions; therefore, it now remains for us to account, if possible, for the discrepancy. The

fact to which I am about to draw attention is, I am inclined to think, quite original, and will go far to explain away the mystery.

If we double the true formula of the oxalate, thereby making its equivalent 180 instead of 90, we see at once that a very curious numerical relation exists between the equivalent numbers of the three bodies engaged.

The following Table will render intelligible what I mean to convey:—

FeO	=	36
FeO	=	36
C_2O_3	=	36
C_2O_3	=	36
$4HO$	=	36
		180

Thus, if one equivalent of protoxide of iron is wanting its place is exactly filled up by an additional equivalent of oxalic acid; so that, supposing this alteration to have been made, we arrive at the formula $FeO, 3C_2O_3 + 4HO$, which is identical with that last proposed by Dr. Phipson. Again, let us take another view of the matter. Dr. Phipson, all through, owing to the miscalculation of his results, has found only half the real quantity of protoxide of iron present. Now, if the salt be analysed by simply igniting it and calculating all the loss as oxalic acid, it is obvious that we must arrive at the formula $FeO,4C_2O_3$, which is that first proposed by Dr. Phipson.

Curious to say, the extraordinary coincidence above pointed out has never yet been mentioned by any of those who have from time to time given their attention to the study of the chemistry of these salts. From what I have said, it will be obvious that, although it is suggested how the errors may have occurred, we are still at a loss to know by what means the gap of 36 parts in the whole equivalent of the salt has been filled up; for we are distinctly told by Dr. Phipson that the oxalic acid was calculated from the amount of carbonic acid produced on the combustion of the salt with oxide of copper. Therefore, an equivalent quantity of carbonic acid ought to have been found corresponding to the 36 parts of oxalic acid. Whether this was the case or not, we have not sufficient evidence to show, beyond the mere statement, indirectly made, in the calculation of the results. I am inclined to think that if Dr. Phipson will re-consider his former calculations, paying due attention to the sources of error which I have mentioned above, he will find that the true formula for the oxalate of iron is, $FeO,C_2O_3 + 2HO$.

2. Certain Reactions of the Oxalates of Iron.—Under this head, I merely wish to draw attention to the theoretical explanation of the change which occurs on exposing oxalate of peroxide of iron to ordinary light; and likewise to demonstrate the nature of the reaction which takes place on treating protoxalate of iron with ammonio-nitrate of silver.

According to Döbereiner, the following equation represents the change which takes place on exposing solution of peroxalate of iron to the light:—

$$C_{12}Fe_4O_{24} = 2(C_4Fe_2O_8) + 4CO_2.$$

The sensitive surface which I use in printing by light is the ammonio-ferric oxalate, which has no solvent action on the deposit of proto-salt formed under the influence of light; whereas, if the plain peroxalate were used, the moment water was applied to the paper it would dissolve and attack the oxalate of the protoxide, over which it exerts considerable solvent power. I only mention this to show my reason for using one salt in preference to another.

The equation which Döbereiner has given for expressing the decomposition under the influence of the solar rays is, doubtless, quite correct when the per-salt is in solution; but when the per-salt is in the solid state and exposed to the rays of the sun, it blackens. If the blackened powder be now immersed in water, the yellow proto-salt is immediately produced. This fact was first noticed by Dr. Phipson, and I have frequently had occasion to corroborate the observation. It appears to me to be very probable that this black substance is the protoxalate in the anhydrous condition, which, on coming in contact with water, assimilates two equivalents, thus becoming the ordinary hydrated oxalate. I have not yet had time to verify this supposition.

With regard to the reaction between ammonio-nitrate of silver and protoxalate of iron, I think the following equation will show more clearly than words can the nature of the decomposition which occurs. The experimental grounds on which it is based have been already given in the first part of this paper[5]:—

$$2(FeO,C_2O_3) + 3(AgO,NO_5 + 3NH_4O) =$$
$$Fe_2O_3 + Ag + 2(AgO,C_2O_3) + 6NH_4O + 3(NH_4O,NO_5.)$$

I need only remark that the excess of ammonia holds the oxalate of silver in solution, from which it is precipitated on neutralising the free alkali.

On the Action of Nitric Acid on Picramic Acid, by M. C. Lea.

On this point very conflicting statements have been made. Girard and Pugh respectively state that picric acid is reproduced by the oxydation of picramic acid by nitric acid. A similar statement is made by Kolbe in his "Lehrb. d. Org. Chemie" (authority not given). In a paper published several years since, on picric acid, I expressed a similar opinion. On the other hand, Wöhler stated that his nitrohæmatic acid (now known to be identical with picramic) was not reconverted to picric acid by the agency of nitric acid. Gerhardt, too, in quoting the first opinion, puts a note of interrogation after it, as if to express a contrary conviction. These differences of opinion have induced me recently to re-examine the subject, and have led to the conclusion that the substance formed is not identical with picric acid. The following were the reactions observed:—

Picramic acid readily dissolves in strong nitric acid to a dark brown solution. By fifteen minutes boiling this becomes clear bright red. If then saturated with potash, quantities of nitrate of potash crystallise out, with much brown varnish, but no trace of picrate. After one hour's boiling the colour of the solution is considerably lighter, the results much the same.

After four hours' boiling the colour of the liquid was bright yellow. It was evaporated in the water bath and gave a crystalline substance mixed with much resinous matter. To remove this it was dissolved in as small a quantity of cold water as possible, filtered and mixed with half its bulk of strong sulphuric acid. On cooling a crystalline reddish yellow substance separated, which might easily be taken for picric acid mixed with resinous impurity. But neutralised by ammonia and heated with sulphydrate of ammonia it gave no indications of the presence of picric acid. Tested with cyanide of potassium the results were the same. By spontaneous evapo-

[5] This is the formula assigned to the compound by H. Rose.

ration of the solution of the substance in ammonia, fan-shaped groups of hair brown needles were obtained. Analysis of these showed conclusively that they consisted of oxalate of ammonia disguised by organic matter.

After eight hours' boiling the liquid was pale straw yellow, and by evaporation on the water bath yielded a substance dissimilar from the former, bright yellow, and coloured intensely deep red by cyanide of potassium after previous supersaturation with ammonia. But treated with sulphydrate of ammonia, it gave no indications of the production of blood red picramate, but became greenish brown with production of a greenish precipitate. The presence of oxalic acid could not be detected.

These experiments appear to me to leave no doubt that picric acid is not formed by the action, either brief or prolonged, of nitric acid on picramic acid, but that resinous substances are produced, accompanied after a time by oxalic acid, which at a later stage suffers decomposition itself. All these substances are, however, produced in very small amount, the greater part of the constituents of the picric acid passing off in volatile decomposition products.—*American Journal of Science*, No. 95.

TECHNICAL CHEMISTRY.

Contributions to the History of Naphthaline, by M. J. Persoz.

After reading M. Roussin's interesting communication, on an artificial coloured product said to be identical with alizarine, I think I ought to make known to the Academy the results I obtained two years ago, while studying, with M. Martel, the derivatives of naphthaline.

Starting from the fact, established by us, that a mixture of commercial nitre and sulphuric acids, even in very variable proportions, will, when heated with naphthaline, readily yield coloured products, we have naturally been led to examine the action of concentrated sulphuric acid on the various nitrogenized compounds of naphthaline.

This is a very difficult study, however simple it may at first sight appear, because the least changes of the condition under which the experiment is performed, exercise a sensible influence on the results. The dye principle formed, possesses the property of madder in dyeing mordants; its colour varies from red to blue, and passes through all the shades of violet.

The blue was only obtained accidentally; and we are unable to state the precise conditions of its formation, though it appears to be due to a molecular change in the nitrogenized naphthaline compound, under the influence of a physical agent.

As the violet-blue tints are the most beautiful, we have devoted most of our attention to them, and have endeavoured to produce them; thus working in an opposite direction to that of M. Roussin, who is chiefly occupied with the reds. We soon found that binitronaphthaline, heated with sulphuric acid only, was best suited to our purpose. In his last communication to the Institute (*Comptes Rendus*, Vol. lii., p. 1033), M. Roussin says:—"By making concentrated sulphuric acid react on binitronaphthaline, no reaction takes place. The binitronaphthaline is completely dissolved, when the mixture is heated to 250°, and the liquid takes hardly an amber colour. After boiling for a long time, the concentrated sulphuric acid began to react on this substance." Binitronaphthaline resists the action of sulphuric acid at a very high temperature, however, at about 300°; the colour of the solution, at first slightly yellow, deepens more and more, becomes cherry-red, and finally brownish-red, beginning at the same time to disengage a small quantity of sulphurous acid. The progress of the operation is easily followed by taking up occasionally a drop of the liquid with a rod, and dropping it into a glass of water. Thus at first a white milky precipitate is obtained, then a light violet; and finally, when the colour is completely developed, a dark violet.

The substance is then taken from the fire, and left to cool, when it is poured into a proper quantity of water and boiled. The liquid, filtered whilst hot, is of a deep red colour, and deposits part of the colouring matter in a flaky state. Alkalies change it to violet red; and even when cold, silk was easily dyed violet by it. After being properly saturated with alkalies, and finally with a little chalk, it dyed mordanted cotton tissues with different shades, varying from lilac to black. The lakes with alum, tin, and lead for a base, are violet; those with iron for a base, were olive, and sometimes reached to black.

In fact, this solution does not seem to alter even during any length of time, in presence of sulphuric acid; though when in contact with air and excess of ammonia, it changes to brown in a few hours, depositing a black powder, which becomes blue when dissolved in alcohol, and red in acids.

The black mass proceeding from the precipitation of the sulphuric solution by water, contains a large quantity of colouring matter, which we were able to separate by means of M. Payen's digesting apparatus. This colouring matter has a beautiful gold reflection, is very soluble in alcohol and pyroligneous acid; but very little soluble in water, ether, benzol, and bisulphide of carbon. As we have before said, it has many chemical analogies with alizarine. In fact, according to whether a bath is slightly acid or alkaline, we can dye the iron mordants to the exclusion of alum mordants, and reciprocally. Moreover, the dyed tissues bear brightening with soap, carefully done, that is to say, progressively. Finally, the colouring matter readily sublimates under the influence of a high temperature.

It is evident, then, that with binitronaphthaline and concentrated sulphuric acid only, without making use of a reducing agent, as M. Roussin has done, a colouring matter may be obtained with marked analogies to alizarine in its chemical properties; however, the observations I have made during my operations, have led me to doubt whether it is possible, even while obtaining perfect red tints, to prepare in this way a colouring matter identical with that of the madder.—*Comptes Rendus.*

PHARMACY, TOXICOLOGY, &c.

On Some Applications of Carbolic Acid, or Hydrate of Oxide of Phenyle, by Dr. F. Crace Calvert, *F.R.S.*

Although carbolic acid has long been known to possess powerful antiseptic properties, its use has been delayed in medicine owing to the difficulty experienced in obtaining it in considerable quantities and in a state of purity,[1] as well as to the caution required in intro-

[1] This acid is now prepared for medical purposes by the Tower Chemical Works Company, near Manchester.

ducing new substances in that branch of science. The success, however, which has lately attended its application, will tend greatly to increase its importance as a therapeutic agent. It has been used with marked advantage in the Manchester Royal Infirmary by several of its distinguished physicians and surgeons. Thus, Dr. Henry Browne has given it in solution in water in cases of chronic diarrhœa, with very satisfactory results. Dr. Roberts has applied it with very great success in the dose of one drop, in cases of vomiting, even after creosote had failed; he has also found it beneficial in cases of vomiting from dyspepsia, which disease is especially marked by pain after food. Mr. J. A. Ransome has used it for ulcers and other offensive discharges. Mr. Thomas Turner, in a note which he has communicated to me, speaks of carbolic acid in the following terms:—

"It may be advantageously used as a solution of one part of acid in seven parts of water, in fœtid ill-conditioned ulcers. It alters the action of the blood-vessels, causing a purulent instead of a sanious discharge, and destroys almost immediately the offensive smell of the secretion. The ulcers having a communication with carious bone, or even necrosis (where the bone is dead), it has in its diluted state a good effect when injected into the sinuses leading to the diseased bones. When there is mere caries or ulceration of the bone, it effects the healing process; and in necrosis it promotes the exfoliation of the dead portion. . . . In gangrenous and all offensive sores it removes all disagreeable smell and putrescency, and may render the discharge innocuous to the contiguous living and unaffected tissues. In its diluted state, therefore, it is a great boon to patients labouring under that class of disease."

Mr. Heath, house-surgeon of the Infirmary, has used it with two parts of water as a lotion in sloughing wounds, and has found that in a short time after its application, it entirely arrests the sloughing process, and produces a healthy appearance.

Dr. Whitehead has used with advantage Dr. Robert Angus Smith's solution of sulphites and carbonates of lime and magnesia.

In July, 1859, M. Velpeau drew the attention of the French Academy of Sciences to the value of the mixture of coal-tar and sulphate of lime, of MM. Corne and Demeaux, in the healing of ulcers and other offensive wounds; and it may be added, that this mixture was used with great advantage in the French army, after the great battles of Magenta and Solferino.

In the following month I forwarded a note to the French Academy, pointing out that from experiments I had made with the various substances existing in coal-tar, it was highly probable that carbolic acid was the active agent of the coal-tar used by MM. Corne and Demeaux; and that much more certainty might be expected if that acid were substituted in their mixture; for the composition of coal-tar varies according to the nature of the coal, and the temperature employed in its preparation. I also suggested that it was probable that the powerful antiseptic properties of carbolic acid prevented the decomposition of the adjacent parts, and thus tended to restore the wounds to a healthy state, and to remove the cause of infection. Before quitting this part of the subject, I beg again to call attention to a fact which I have already published in one of my papers, namely, that the addition of two or three drops of this acid to a pint of freshly made urine, will preserve it from fermentation or any marked chemical change for several weeks.

I have also applied it lately to foot-rot, which annually carries off large numbers of sheep; and I have been given to understand that the remedies hitherto adopted in this disease have been only partially successful. I think that if my experiments are further confirmed, it will prove a great boon to the farmers of this country.

This acid has also been applied by me, during the last twelve months, to the preservation of gelatine solutions and preparations, of size made with starch, flour, and similar substances, and of skins, hides, and other animal substances. In fact, its antiseptic powers are so great, that it is the most powerful preventive of putrefaction with which I am acquainted. It appears also to act strongly as an antiferment; for I have proved on an extensive commercial scale, that it prevents (as stated by me in a paper published in 1855) the conversion of tannin into gallic acid and sugar. It also arrests lactic fermentation. I am now engaged in a series of experiments to discover if that power extends to alcoholic, butyric, and acetic fermentations. I hope also to communicate to you shortly the results of my experiments on the protection of timber from dry rot.—*Pharmaceutical Journal.*

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Six Lectures on Light[1] *(adapted to a Juvenile Auditory), by* JOHN TYNDALL, *Esq., F.R.S., Professor of Natural Philosophy in the Royal Institution.*

LECTURE I. (Dec. 26, 1861.)

NOTES TO THE LECTURE:—

Light moves through space at a speed of 192,000 miles in a second—Light travels from the sun to us in 7½ minutes; it would take a cannon ball 15 years to perform the journey—An express train travelling night and day would require 3 weeks to go round the earth; light would perform the same journey in the interval between two puffs of the engine—If the sun were blotted out, we should continue to see it for 7½ minutes after its extinction—If the nearest of the fixed stars were blotted out, we should continue to see it for 5 years after its extinction.

Light moves in straight lines, and therefore opaque bodies cast shadows—If the opaque body be a sphere, and if the light emanate from a mere point, the shadow of the sphere is a sharply defined *divergent* cone—If the luminous sphere be of any sensible magnitude, but less than the opaque one, the perfect shadow will be still a divergent cone, but it will be surrounded by an imperfect shadow called a *penumbra*—If the luminous body be a sphere equal in size to the opaque body, the perfect shadow will be a cylinder, and it will be surrounded by a penumbra—If the luminous body be a sphere larger than the opaque one, the shadow will be a *convergent* cone, surrounded by a penumbra—In consequence of the great size of the sun, this last is the character of the shadows of the earth and moon in space—The shadow of a body placed in the sunlight is never sharp, but fades gradually away at the edges.

When light strikes an unpolished surface, it is scattered irregularly in all directions—When light strikes a polished surface it is regularly reflected—If it strike the surface along the perpendicular, it is reflected along the perpendicular—If it strike the surface obliquely, the direct and reflected rays are equally inclined to the perpendicular—This is expressed by the law, that *the angle of incidence is equal to the angle of reflection*—(*Note.* The angle of incidence is that enclosed between the direct ray and the perpendicular; the angle of reflection is that enclosed between the reflected ray and the perpendicular)—If a mirror from which a ray is reflected be caused to rotate, the ray will also rotate, but it will turn round with exactly twice the speed of the mirror.

In the case of a plane mirror, if the rays before reflection be convergent, parallel, or divergent, they will be convergent, parallel, or divergent after reflection. The *relative directions* of the rays are unchanged by reflection from a plane mirror—In the case of a concave mirror, if the rays before reflection are divergent, they are less divergent after reflection; if the rays before reflection are parallel, they are convergent after reflection; if they are convergent before reflection, they are more convergent afterwards—The point to which parallel rays converge after reflection is called the *principal focus* of the mirror—In

[1] Reported verbatim by special permission.

the case of a convex mirror, convergent rays are rendered less convergent, parallel rays are rendered divergent, and divergent rays are rendered more divergent by reflection—If the rays of a parallel beam reflected from a convex mirror be prolonged backwards, they will cut in a point behind the mirror, which point is called the *principal focus* of the mirror—In a concave mirror, then the principal focus is a real point in front of the mirror where the reflected rays actually cross each other; whereas in a convex mirror the reflected rays do not actually intersect; hence the focus of the convex mirror is called an *imaginary* focus, in opposition to the *real* focus of the concave mirror —The image in a plane mirror is as far behind it as the object is in front of it—The image in a plane mirror is a lateral inversion of the object. A compositor's type, or a sentence written backwards, may be read as common print or writing when reflected from a piece of looking-glass—You can see your whole person in a looking-glass of half your height—If two plane mirrors be parallel, a luminous object placed between them gives an infinite series of images on each side—Two plane mirrors, so placed as to inclose an angle, give a definite number of images of an object placed between them—this is the principle of the kaleidoscope.

The principal focus of a spherical mirror lies midway between the centre of the sphere of which the mirror is a portion and the surface of the mirror—The image of an object placed between the principal focus and the surface of a concave spherical mirror is *erect* and *magnified*, and is behind the mirror—The image of an object placed between the focus and the centre of the sphere of which the mirror is a part, is formed in front of the mirror and beyond the centre, it is *inverted* and *magnified*—The image of an object placed beyond the centre of the sphere to which the mirror belongs, is also formed in front of the mirror, between the focus and the centre; but here it is *inverted* and *diminished*. This is the case observed on looking at the concave surface of a polished silver spoon—The image observed in a convex mirror is upright and diminished: this is the case observed on looking *at the back* of a polished silver spoon.

I take some pleasure, ladies and gentlemen, in going to the Alps every summer. I like climbing the mountains partly on scientific accounts, and partly for the simple love of climbing; but, usually, before I try any dangerous point—any high and giddy peak—I require three or four days' discipline. I dare not trust myself, fresh from London, upon a very high and precipitous peak. Unfortunately for me, morally speaking, I am at present placed upon what to me is a dangerous moral peak, without those few days discipline which I find of so much importance to me in the Alps. I find myself placed in a very peculiar position. However, I will shorten my preface, and make it very brief in reference to this point. The leader—the noble leader who has hitherto led us up the mountain of knowledge, and before whom I have been as a boy and a listener for the last eight years, has thought proper to lay aside his alpenstock and to rest himself for a time. I hope sincerely, for your sake and for mine, that he will soon again resume it. (Cheers.) I say it falls on me to stand in his shoes; and, believe me, ladies and gentlemen, it is no pleasure to me to do so; but I feel that I have nothing to do with pleasure under present circumstances, for I, and every boy who hears me, ought to place his duty before his pleasure; and my duty I shall seek to discharge with your assistance—with your help—throughout the course of these lectures, to the best of my ability, throwing myself upon you for the reception that they are to receive.

Just one word I will repeat of what Mr. Faraday has often told you at the commencement of his Juvenile Lectures —that they are really to be juvenile lectures, and although people of mature years, who know more, perhaps, of these matters than I myself do, come here, they will kindly be content to stand spectators while my young friends and myself converse together upon the subject that forms the object of our present consideration.

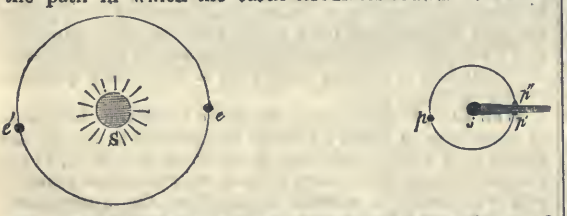
That subject is *light*; and in order to make plain to you from the outset the means we shall make use of in these lectures, I must commence by saying to you that I must have a source of light. It would not answer for my purpose to take a candle, to take the light of a lamp, or to take the light of these gas jets that you see around you. In order to make these phenomena visible to you all I must have a very intense source of light—a source of light almost as intense as the sun itself. And here I have two conductors of most mysterious power about which you, my young friends, know not quite so much, but almost as much as I do. These two wires which are covered with gutta-percha, are the channels of a power which is now latent in the yard down-stairs. That power will rush through these wires; (we have nothing to do with it farther.) It will give us the means of producing this miniature sun which we shall use in these experiments. Here at the end of these wires we have two pieces of coal, and if I now bring these two pieces of coal skilfully together, you see that I produce a very intense light. The electricity is flying across from point to point producing this intense light. Now in our experiments we shall have to make use of the instrument that you see here. The bits of coal attached to these wires are fastened to this instrument and thus I am saved the trouble of holding them in my hand, and am enabled to keep them at a regular distance asunder, for it is necessary that they should be kept at a certain distance asunder in order to produce this glorious sun-like light. Here again you see coal points exactly the same as those that I made use of in the former experiments; and by means of a screw I can bring them together, separate them, and there you have the light. Now this is the glorious light by which we are going to operate at present, by which we must endeavour to track the phenomena of light, and by which we shall, I trust, learn something more regarding this wonderful agency which we have all passed by perhaps unheedingly from our infancy. We shall, all of us, I hope, learn something more of this agency than we know now. For we are all learners here—all students, all boys in fact, at the feet of Nature, endeavouring to learn—endeavouring to decipher the vast and glorious book which she lays open before us; and I say that the wisest philosopher among us, Mr. Faraday included, is nothing more than a boy in the presence of that glorious book.

Well then, in order to enable me to show you the course of these rays of light, I have to be grateful to Nature for giving us a hazy, foggy day in London, and here, at the present time, in this room, I have no doubt that many of you will notice a certain dimness of the atmosphere. That dimness I hail as a boon, because it will enable me, I trust, to show you the track of the rays of light through the air; for if there were no obstacle to the rays of light in the air—no dust suspended, or nothing upon which the light could fall,—the light would be absolutely darkness: in order to make itself evident to our sense it must fall upon something. I have here a lamp, precisely the same as the one you saw previously, and I have placed it inside a little box in order to shade it, and I will cause the rays of light to issue from the lamp, and will show you the track of light through the air of the room. I first bring my coal points close together just as in the former instance, and now you see we have the track of our beam of light. I do not know whether all of you see what I mean, but if you look you will see what I want you to pay attention to—the track of the beams—the straight line which they describe through space (for light is propagated in straight lines), and in this way I trust to be able to make the presence of the rays visible to you, and to enable you to track them through the room, and to develope their laws in the most beautiful and interesting manner. So much, then, for the instrument which I intend to use in this Lecture.

You have thus seen that these beams of light going through the room pass in straight lines; and I have taken the liberty of placing a few notes on this subject before you, for the use of those who will be pleased to be philosophers enough to try to repeat some of those experiments afterwards—who will entertain a sufficient love of philosophy to try and imprint what they have here heard upon their minds,— and I have not a doubt (I speak confidently, not on my own account, but in virtue of the very importance of the subject) that although five-sixths of the boys present may perhaps forget all that I say, some of you will remember all your lives long what is said here; and I do not know but that in speaking in

my own humble way at this table, I am scattering seed which may blossom when you become men;—you may become philosophers like Sir Isaac Newton; you may become great men; you may be able to lay hold of these treasures of Nature, and convert them into stores of power for the good of your country. Unfortunately, we cannot do this at present, but I trust that the boys of this generation, who will be the men of another generation, will be able to do it.

Well, then, with regard to this agency that we call light, I must direct your attention to one of the first statements in the notes that I have caused to be placed upon the seats of the Institution. I speak there of the enormous velocity of light. You see we can measure that velocity. Light runs over a distance of twenty miles in 1-10,000th part of a second. You see we measure a thing that travels so quickly as that. I will tell you how it was measured in the first instance by a very able man. You all know the glorious orb that shines so redly through the London smoke at the present time, our sun. Well, at a vast distance from the sun there is a great planet called Jupiter, which planet is weightier—is heavier—than all the rest of the planets put together. This ball (*s*) I take to represent our sun. This ball (*j*), the planet Jupiter. As you know very well, the earth travels round about the sun, describing what is called the earth's orbit. That dark ball (*e*), I will ask you to imagine as representing the earth. I have drawn a broad line round the sun; this line is to represent the path in which the earth circulates round. Now we



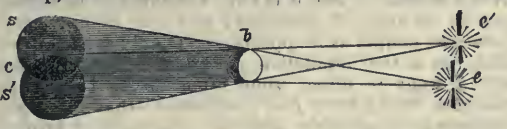
will suppose that the earth travels round and round the sun incessantly through ages and ages. Here upon this little ball we live, and there are astronomers with their telescopes pointed to this great planet Jupiter. They can with their little bits of telescopes accurately tell you the size of the planet, tell you the weight of it in pounds, how much it weighs. But more than that, they have discovered that round about this large planet four beautiful little moons circulate. Now I have taken a white ivory ball (*p*) to represent the first moon of Jupiter, —the one which is nearest to Jupiter.

I do not want you to remember the other three moons at all at present, only that which is nearest to Jupiter, and to fancy that little moon going round and round the planet incessantly. Well now, we will suppose the earth is here (at *e*) an observer on the earth's surface looks at Jupiter through his telescope, and sees this little moon (*p*); he observes it travelling along, crossing the face of Jupiter, going round until finally it comes here (to *p'*) upon the edge of the vast shadow that this huge planet casts through space. It is like a little lamp shining before the astronomer up to a certain point, suddenly it plunges into that shadow and is no more seen; it crosses the shadow, and by-and-bye as the astronomer still gazes, he sees the little lamp emerging from the shadow of Jupiter here (at *p''*) as a bright light; and thus it goes round and round, and the astronomer is enabled to calculate with the greatest certainty the precise moment when this little lamp ought to appear. It can be calculated to the fraction of a second when it ought to dip into Jupiter's shadow and quench itself, and when it ought to emerge from Jupiter's shadow, and re-light itself. Now imagine six months have passed by, and the earth has gone, gone, gone away to here (*e'*); our astronomer, still keeping watch on Jupiter's moon—and this is a beautiful discovery—now finds that this little lamp *appears to be about fifteen minutes later* in emerging from the shadow than when he was there, (at *e*.) Why? Because it requires fifteen minutes to travel across the earth's orbit. It requires a time of fifteen minutes—that is, seven minutes and a-half, as I have stated in the list of memoranda, to travel from the sun to the earth. If the velocity of the light travelling from this point (*e*) of the orbit to the opposite point (*e'*) of the earth's orbit requires fifteen minutes, it will require simply half that, that is, seven and a-half minutes, to travel from the sun to the earth. And this is the velocity of light; it requires seven and a-half minutes to travel from the sun to the earth; its velocity being in round numbers 200,000 miles a second, exactly, it is 192,000. I will not repeat all the statements here made to you in the memoranda. The human mind cannot conceive of this velocity: and, in order to enable you to form some vague notion of the speed, I have compared it with the velocity of an express train travelling night and day at the velocity of forty or fifty miles an hour, which would require three weeks to get round the earth; whereas, in the interval between two puffs of the engine, light would travel over the same space. So much then for the velocity of light.

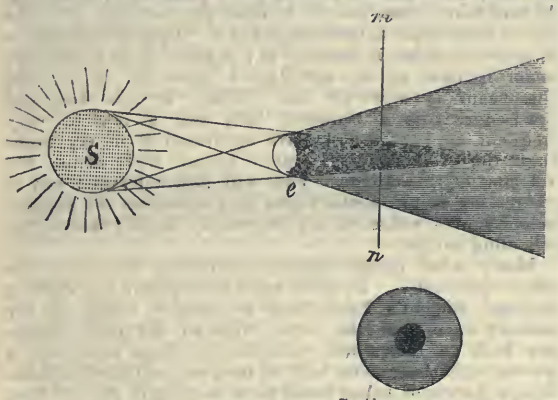
I now pass on to the consideration of another portion of the subject, and that is, inasmuch as light travels in straight lines, (as we have already seen when passing through the room,) if you place an object in the path of a ray of light, the light will graze past the sides of the object and will cause the space behind it to be shady. We have thus shadows produced. And now let me speak to you in reference to these shadows. I have here a lamp on the table; I trust you have cast your eyes upon the memoranda where I speak of the influence of the size of a luminous body upon the shadow that it casts; for if light move in straight lines—the light from this lamp, for instance—if it start here from a point, the rays of light will go diverging from that point, and grazing the two sides of that weight, it will go on in a straight line, and all the space behind the weight will be, as I have said, a divergent cone of shade. But if it emanate, not from a point, but from an object of sensible magnitude, such, for instance, as the flame of a lamp, as I have here, then, I say, that the light does not cast what we call a sharp shadow. It casts a shadow, but that shadow is surrounded at its edges by a fringe, which is called a penumbra. Now, when I hold that stick pretty close to the screen there is a very slight penumbra,—you perhaps see none; but I see a very little fringe, and as I draw it towards the light you will see the fringe expand. There it is pretty clear. You see in the centre it is a perfect shadow, but round about that you see a dim washy rim, which is the penumbra of the shadow; and that is because the light has a certain magnitude—that what is shadow to one part of the light is not shadow to another; and there is only one space in the centre that is really perfect shadow, shaded from all portions of the light. This central portion is what we call the perfect shadow, and this fringe surrounding it is what we call the penumbra. I will now lower that light, and give you an idea of the sharpness that we have with this splendid electric light, in which, to some extent, the light, is emanating from a point, or very nearly a point, at least; the electric light having scarcely any magnitude at all compared with the light of the lamp. [Holding a stick between the electric light and the screen.] Now you see no penumbra; you see perfectly clear and sharply defined edges, and if we bring it near it is the same—perfectly clear and sharp. Those who look at it closely may perhaps see that there is now a little fringe, because the light does not emanate absolutely from a point; but if it had no sensible magnitude, then it would be a perfectly clear and sharply-defined shadow in all positions of the stick.

Now, in order to make this plainer to you, I will make

use of two lamps, and let light from both fall upon an object such as this ball (*b*) which I have suspended here. It will show you the difference between a perfect shadow and an imperfect one, or half shadow, very clearly. I have now divided into two equal parts the battery which I have hitherto made use of. In the experiments that we have hitherto made we have used sixty cells. I have now separated this ir to two batteries, each possessing thirty cells, and each of these batteries I have connected with one of these electric lamps. Now you will see the principle which I am endeavouring to make clear to you when I cause these two lights to operate. Here (at *l*) we have one light which will strike that ball and cast a shadow of it upon the screen (at *s*). There is the shadow of the ball upon the screen : and now I have another light (*l'*) at some distance from the former, connected with its own battery, which casts another shadow of the ball on the screen (at *s'*). These two shadows overlap each other, so that you see there is a certain portion which is undoubtedly shadow to one lamp, but not shadow to the other. And here in the



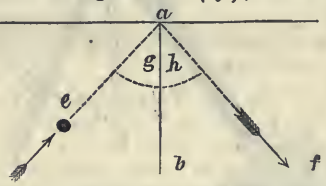
centre (*c*) we have a region of perfect shadow. This (*s* and *s'*) being the penumbra. On each side we have the penumbra, and if you suppose the light to come from a ring of lamps, this penumbra will extend all round, and we shall have a circular space shaded. This is exactly what takes place with the sun in regard to the shadows that the earth and moon cast on space. I have drawn them here, this (*e*) being the earth, and this (*s*) the sun. Owing to the greater magnitude of the sun, the region of real shadow is represented by this dark cone. It is, as I



Section across *m n*.

have said, a convergent cone. It comes to a point, and all round that region of perfect shadow you have the penumbra, the section being as seen across *m n*. I cannot here enter into all the minutiæ of these things, but I have written down in the notes sufficient to engage your attention, and I trust you will take each statement that I have made, dwell upon it when you go home, and make yourselves perfectly clear about it. What I want you to remember is this,—that when the luminous body is less than the illuminated body, you have a divergent cone, surrounded by a penumbra, and when you have a luminous body, such as the sun, larger than the body illuminated,—larger than the opaque one,—larger than the body which casts the shadow—then I trust you will make it perfectly clear to your minds that you must have a shadow of the shape of a convergent cone surrounded by a penumbra ; and astronomers sometimes see these vast conical shadows moving through space during eclipses. So much for shadows, which are consequent on the passage of light in straight lines through space.

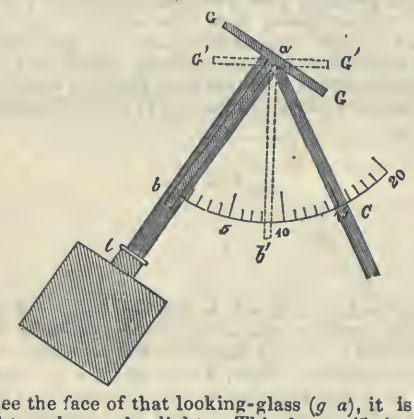
I pass now to another very important portion of our subject, and that is to the subject of reflection, and I want to make this subject of reflection perfectly clear to everybody present. Philosophers, the wisest and the greatest, when they think, and when they reason upon these things that we call light and magnetism and electricity, are actually forced to imagine these things to be like something tangible. Mr. Faraday, I have not a doubt of it, imagines that magnetism is something moving about in lines—straight lines and curved lines. He imagines this, but it is all imagination, for human eye has never seen it. And so with regard to light ; we are obliged with the eye of our mind, with the eye of our souls, so to speak, to imagine that we see these thing, and to look at those things which are imperceptible to the eyes of our bodies. Now, it is of infinite importance, I think, that boys particularly, and if you will allow me to say so, girls too, should have clear and distinct ideas of how this thing that we call light is reflected. Here is a bagatelle board, known to all of you ; if I take one of these balls and drive it plumb against the other side, what occurs? It rebounds. That is a case of reflection, true reflection, so that you see when I strike this perpendicularly,—to use a very learned word which of course all of you will know by-and-by,—when I strike this straight against the side, the ball rebounds along the same line, it returns along the line on which it struck the surface. Light does exactly the same. If a beam of light strikes plumb against the surface of a plane mirror, it rebounds ; it is reflected back exactly as that ball is reflected back ; and suppose a billiard player wishes the ball to rebound in a sloping direction, after striking the side of the billiard board, what does he do? He does not strike it plumb ; no, he wisely strikes so that the ball shall hit the board obliquely ; and now when I do so, if you observe the path of the ball, you see it will strike here, and be reflected back on a line obliquely to the surface against which it strikes. Now, what I want to imprint upon your minds, and never to forget is this :—Supposing you have a line (*a b*) drawn perfectly plumb and perpendicular to another one (*c d*), and supposing here to be a ball (*e*), and that I drive that ball right against that point (*a*) where this line is drawn ; if that ball were perfectly elastic, and if the surface against which it strikes were perfectly elastic, then it would rebound along this line (*a f*) ; it would be exactly



equally inclined on each side to this line (*a b*), and would fall as much upon one side of the perpendicular as it did upon the other ; the two angles (*g* and *h*) being exactly equal to one another. Now, I hope you see this clearly ; it is very gross and very simple, but it is absolutely essential to the understanding of the law with regard to light, for light does just the same. Light, when it strikes obliquely against a reflecting surface, is driven back ; so that the reflected ray, as it is called, makes the same angle with the perpendicular as the direct ray. To make this plainer, I will try, in the first place, to show you the general fact of the reflection of light. I will take, in order to show you the effect of this reflection, a beam from a lamp, as I have already done. Now, how is this reflection accomplished? You see this beam passing straight from the lamp. Here I have a piece of silvered looking-glass ; I throw that into the path of the beam ; and look what takes place. There you have that splendid cylinder of light passing through the room. You see as I twist the

mirror the reflected beam passes over the gallery, and thus it is evident I can make the beam to course along and be reflected as I please. The great Sir Isaac Newton imagined that light was due to small particles projected away from the luminous body with intense velocity, and that these particles were reflected in precisely the same manner as we have seen the billiard ball to be reflected. I should not like you to dwell upon this as a truth at the present time. We have reason to believe that this is not the case, but it is too early to think about that at present. Now, I want to examine this matter of reflection more fully, and before I do so, let me just draw your attention to that splendid beam of light. The slightest motion of the glass causes it to move through a very vast space. All these things ought to be taken, and are taken, advantage of by philosophers. You saw that beam of light moving about. It has no weight. If you wish to enable yourselves to see the motions of the wheels of your clocks and your watches you put hands to them and make them traverse a large circle, in order to be able to divide that circle into hours and minutes, and you can see the hands travelling over those hours, over those minutes, and the larger your circle the more minutely can you subdivide it. There are friends of my own here who know how very necessary it is in the large instruments—large theodolites—to have a large circle in order that that circle may be divided into very small parts; but weighty indices like these hands would be utterly useless in any scientific experiments, and philosophers, therefore, make use of an index without weight—a glorious beam, such as you saw there going over the room. They attach a bit of looking glass to their little magnets, and the slightest motion of the magnet causes this long index—and you can make it as long as you like—to move through a large space, and in this way we employ an index of light which possesses no weight.

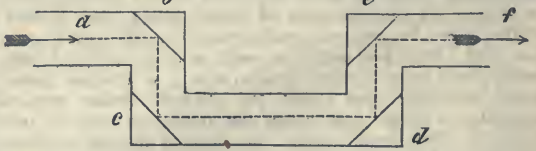
Now I want to develope further, if I can, this doctrine of the reflection of light; and in order to make you understand it thoroughly and perfectly, I have placed this lamp (*l*) here, and have placed a piece of common looking-glass (*g a*) in front of the lamp. Now what am I doing here?



You see the face of that looking-glass (*g a*), it is exactly at right angles to the light. This beam (*l*) is perfectly plumb, is perfectly perpendicular to the surface of this looking-glass. Very well now, but you see that in this case, when the beam strikes against the surface of the looking-glass, it is reflected directly back again and strikes the lamp, and a portion of it is reflected back upon the audience in the line in which it fell upon the looking-glass. Now I have here drawn the sweep of a large circle; this arc I have divided into exactly twenty equal parts. You see the index (*a b*) that I have here. When I turn it round it causes the looking-glass to move. This index is perfectly perpendicular to the surface of the looking-glass. Well, I will place it here at 8 (*a b'*). Now you remember what I have said about the billiard ball; if what I have said be true, inasmuch as that index (*a b'*) is plumb to the surface of the mirror (*g' a*), and inasmuch as the direct ray of light will, as I have said, fall as much on this side (*c*) of the index as the reflected ray falls upon the other side (*b*), then the ray of light will strike the youth who is in the direction of this line (*a c*). The apparatus is placed with perfect accuracy, so that you see this direct ray of light (*a l*), and that reflected ray of light (*a c*) make equal angles with this perpendicular (*a b'*).

Now, I have endeavoured to define a term which is used incessantly in writings on this subject, and which must be perfectly remembered. The angle you see here (*l a b'*) between the direct ray and this index, is called the angle of *incidence;* and this angle between the reflected ray and the perpendicular (*b' a c*), is called the angle of *reflection.* I am satisfied every boy could express this law. Now I will ask you a question,—Is the angle of incidence unequal to the angle of reflection? Is it, or is it not? [Voices: "No, it is not."] No, it is not; the angle of incidence is equal to the angle of reflection; and thus we have arrived at the law, simple as it appears, and simply as it is proved, which lies at the foundation of optical science. Another point I want to direct your attention to is this (and remember what I have just established—that the angle of incidence is equal to the angle of reflection): I bring the index back to my former position (*a b*), and I will allow the beam to fall plumb upon that mirror (*a g*). Here it is. You see the beam returns exactly along the same line, as I have already proved. Now watch me: I will cause the index to move; I will push it on. Now both the index and the reflected beam start from one and the same point; they are like two boys setting out to run a race, for they start fairly; but watch their progress. I push the index to No. 2. Where is the beam? The beam has gone farther; the beam is now at 4. I push the index to 4. Where is the beam now? It does not coincide with the index; the beam is now exactly at 8. I push the index to 6, and the beam is now at 12; I push it to 8, and the beam is now at 16, exactly; I push it to 10, and the beam is now at 20. What is this beautiful law?—for it is a beautiful law, and a law of very frequent application. I have expressed it in my notes that the reflected ray turns round with twice the velocity of the mirror. You will find a learned way of expressing that in books where they state that the angular velocity of a reflected ray is exactly double that of the reflecting mirror.

Many other things equally interesting flow from the same principle. I have met sometimes, in walking through Regent's Park, and I daresay many of my hearers have done the same, with an old man standing with an instrument mounted on three legs, like a photographer's camera, and he had, supported upon these legs, a system of tubes much more cunningly devised than this [exhibiting a square mahogany tube with four rectangular elbows], but still exactly the same in principle.



Well, this is supported upon the legs and he desires people to look through his tube; and he puts, cunningly enough, opaque things between, for he has these tubes prolonged a little so as to make it appear as if you looked straight through, as his object is to make you fancy that you actually look straight through these tubes, and in consequence he prolongs this and leaves a space between. He puts his hand in and wants you to believe you can see

through his hand. He puts a piece of board in there, and says that you can see through that piece of board. I remember once going up to this old man, and I said "May I ask what you have got at this corner, and what you have got at that corner, and at that other corner?" and he looked very much puzzled. I daresay many boys present can solve the problem at the present moment; but there was a man and his wife standing beside this old man, and they became extremely offended at me. (For ignorant people are the most easily offended in the world, and the more ignorant people are, the more they recoil from having their ignorance enlightened.) As I was leaving the place, not desiring anything that might be intended as an insult, —for it was thought that I was doing the man altogether wrong by putting him into such a dilemma,—the gentleman said, "It is quite right, Sir, you *can* see through your hand; you *can* see through the board; it is perfectly manifest, I can see through the board at the present moment; it is a wonderful thing, Sir, no matter what the gentleman says." At the present time you understand the philosophy of this thing, no doubt. Here I have a bent tube [again referring to the apparatus just described] and here at the corner there is a piece of looking-glass—any boy could construct it of paste-board himself. The beam comes in at *a*, strikes upon that looking-glass (*b*); but it is reflected,—because the angle of incidence is equal to the angle of reflection— it is reflected to the opposite corner (*c*) where there is another piece of looking-glass. At that corner the beam strikes upon that piece of looking-glass and is reflected along to (*d*). There there is another piece of looking-glass; it strikes against that, and from thence is received upon a fourth piece of looking-glass (*e*) and is there again reflected; so that you see I shall be able to send the beam right through this, and you will see that the apparatus will allow the light to go round the corners as I have described. [The apparatus was held before the electric lamp, and a beam was passed through it and received upon a white screen.] Here again we have a tube at which you can all look, and it is worth saying a word about it. At the time of the Crimean war it was found that the sailors were very fond, when they fired a shot, of popping their heads over the parapet in order to see what execution the shot had done, and they were very often picked off while in that position by the Russian riflemen. The Rev. Mr. Taylor, a member of this Institution, devised this simple little apparatus, and this is one illustration of what I have said—that in natural science there is a reservoir of power that, if men only used it aright, might be converted into a potent agency in the service of our country. It is simply upon the principle of this tube which has been devised for years and years—before I was born. Here I have a tube with a bit of looking-glass placed across one end, so; and when the beam of light comes in it is reflected down along this tube, and here at the other end is another bit of looking-glass, and the light is reflected out here, so that if I wished to observe you, and supposing you to be a rifleman with deadly intent upon my life, I should not, if I could, pop my head over the parapet, but I should quietly drop my head behind it. [The Lecturer showed the experiment, making his observations from beneath the lecture table.] I can see you perfectly well. In such cases this might be turned to very great and useful account, and many valuable lives might thus be placed out of peril by giving them the means which will enable them to observe without exposing themselves to personal danger. I will pass the electric light through this tube; I can only send it in a rough manner, but there is the beam of light, and you can trace the beam of light through the room, if you watch it, along the dust; and here you see that the beam which enters from the bottom issues at the top.

I have now to beg your attention to another effect of reflection, and it is very instructive indeed. I have here a pair of mirrors. If I place that candle between those two mirrors, set together at an angle, you see more than one image of it, you see a series of images; and the larger I make that angle enclosed between the mirrors the fewer the images you see; and the smaller the angle the larger the number of images. You see the images of the candle ranged symmetrically round about, they are all in a circle, and the centre of this circle is the point where the mirrors meet. If these two mirrors were placed perfectly parallel, you would find the series of images diminish in brightness to an indefinite length, reflected between the two parallel mirrors; and this is the reason why in some shops, where there is looking-glass on each side, the shops appear to have no end. These angular mirrors give you a series of images (and, remember, this is an experiment in the power of all of you to make). You will always find this—that the number of the images is perfectly definite: at the present time I have set them at an angle of 90 degrees—a right angle—that is to say, one of those sides is exactly plumb to the other; and in that case you have four objects—that is, three images and the light itself. Many of my hearers do not understand what degrees are, I have no doubt; but I have also little doubt that the older ones will teach the younger ones when they go home, and tell them what it means. If you suppose a circle, to contain 360 degrees, the right angle contains 90 degrees, that is, the fourth part of the number of degrees in a circle. Divide the number of degrees in a circle by 90, and what is the result? it leaves the quotient of 4, and that always gives you the number of images plus the object. If you divide 360 by the number of degrees in the angle at which the mirrors stand, you then get the number of images, together with the object. Supposing I make it an angle of 45; then 45 will go in 360, how many times? Eight times. Then you would have 7 images plus the object; because the number of images plus the object is the quotient that I have referred to. So that you can always determine from the number of images you get, the angle that these mirrors make with each other. Upon this principle depends the toy known to you all—the kaleidoscope, in which you do not see a single image, but a series of images refracted symmetrically, producing a regular figure. The kaleidoscope was invented by Sir David Brewster, but it is now very much used in commerce, and sold at a very cheap rate: here I have a fourpenny instrument, which will show it very beautifully. In it I have these reflectors, and here is the object—pieces of coloured glass; and when I look through at these pieces of coloured glass, I see, not simply what the eye looks at, but I see also the reflections of the object, and these are arranged in beautiful symmetrical figures. I will try to cast these kaleidoscopic figures on to the screen for each to see, if I can. I send a beam of light through, so that it shall strike first the coloured objects and then the reflectors within, and then I trust we shall see on the screen the image of those bright pieces of glass. Here you have the bits of coloured glass—scraps of useless glass, which look very shabby if looked at directly, but when they are reflected in this way they give you these beautiful figures.

There is now another point I think I ought to direct your attention to, and that is this, that when you look into a mirror your face is laterally inverted; for instance, I believe that my hair is really parted to my left hand, but if I look here I see a gentleman in this looking-glass with his hair parted to the right hand, and thus we are laterally inverted when we look into a looking-glass; and photographers often have people complaining when they find themselves represented exactly as they are, because they quite forget that they do not know themselves really as they are; and when a photographer sets the parting of the hair on the proper side— the side which other people see, they are sometimes very angry with the photographer. They say he has misrepresented them, forgetting, in fact, that they have been using an instrument all their lives which inverts them

in this lateral way. I have written a word here backwards, but if I look at it in this looking-glass it is perfectly legible, as you will see it to be when I cast it on this screen. Thus when compositors set up their type, it has to be printed upon a sheet, and therefore the type is always the reverse of the printed sheet. Here I will cause this backward word to be reflected upon the screen, and you will then see what it is. You have it now plain enough, D—O—G dog, it being inverted by the reflection.

I have now to pass on from inversion by plane surfaces —by such things as looking-glasses—to reflection by curved surfaces. We have here a splendid mirror; here is one less splendid, but equally good. I do not know how you could learn any philosophical matter without practical instructions. The most learned man may discourse upon natural philosophy, but he can never get the thing into his hearers; you can never learn natural philosophy by hearing it, or by reading about it. Reading and hearing it are both very good; but let each boy present, when he goes home, who feels an ambition to become a natural philosopher,— and indeed it is a glorious vocation,— try to repeat the experiments for himself. Let him, if he has sufficient pocket-money, buy a little convex mirror, or a little concave mirror, as I have said, and try to repeat the experiments for himself, and not give way if he finds it difficult. Let him persist, and repeat his experiments again and again, and with a kind of moral responsibility, and he will then, and in that way only, become a natural philosopher. That is the only way to master the truth of the thing.

Now, supposing I have a ray of light, and that ray of light falls at an angle upon a plane reflector, you know how it will be reflected, making the same angle to the perpendicular as did the direct ray. Here, again, let us have another piece of reflecting surface inclined to the first one in that direction; and let us suppose a ray of light parallel to the first ray to strike on this plumb, it will be reflected back again to where it started from. And now we have another piece of looking-glass inclined in the opposite direction to the first; and let us have a third ray of parallel light falling upon that; it will be reflected as the others have been, and thrown back. Thus you see that, by this arrangement of sloping the mirror, we shall get all the rays thrown back so as to cut one another in the same point here. Now, if you had a great number of small bits of mirror, you might so arrange them that the reflection from each should come towards that same point, and in a concave mirror, as it is called, we do that. In a mirror of this sort, formed by the union of surfaces of that kind, all the rays that fall upon it on reflection are forced to come back and unite at a single point crossing each other. I will now make an experiment with this mirror so that you may see the track of the beam in the manner I have already indicated. [The diverging rays from the electric lamp were then allowed to fall, at an angle on the concave mirror, which reflected them back again to a focus, the conical form of incident and reflected rays being clearly visible in hazy atmosphere.]

How splendidly these beams are reflected! How they are squeezed, and converged and brought together to a "focus," as we call it. There you see the focus; how bright it is! And if that mirror were perfect, you would find that the carbon points of the lamp, that are intensely illuminated, would rebuild themselves up there, and you would have them here depicted in the focus with the utmost accuracy; even now I can see a blurred image of the coal points on this screen when I interpose it, so that the rays of light that issue from that lamp are made use of by this mirror to build up the image again; they are the bricks, so to say, by which the coal points are here built up in the focus of the mirror, and as I cause the mirror to move—if I tip it up—look at that splendid cone of light produced by the convergence of the rays after reflection. This then will illustrate the law of reflection from this concave mirror.

I have now a convex mirror, and when I set it down here for a moment we shall see the difference between this and a concave one. This convex mirror bulges out, you know, in the centre, while, the concave is hollow in the centre. Now, what takes place with a convex mirror is this. Supposing the rays of light fall upon it, they are reflected back again, but spreading out instead of being squeezed together to a point, and if you look at the mirror you will see the picture of the rays, as it were, going right through it and intersecting at a point *behind* the surface. With the concave mirror you remember that we had the rays converged and caused to cut in a point in space actually *before* the mirror, but here in this convex mirror you see there is no real cutting of the rays; they are all reflected and caused to diverge. The intersection behind the mirror is merely an appearance, and hence this cutting point, which is the focus of the mirror, is called an imaginary focus, in opposition to the real focus, which as I have said in the Notes, is actually formed in space in front of the concave mirror.

MANCHESTER LITERARY AND SCIENTIFIC SOCIETY.

Ordinary Meeting, December 24, 1861.

J. P. Joule, *LL.D., President, in the Chair.*

Mr. Brockbank exhibited some samples of steel manufactured by Mr. Bessemer's process. These specimens had been bent and twisted cold, and showed a remarkable degree of ductility. He stated that the Bessemer steel was one of the most plastic and manageable of metals—more so even than copper. It could be bent, flanged, or twisted, either hot or cold, without annealing, and over a considerable range of temperature—which is not the case with ordinary steel or copper.

A plate of 18 inches diameter had been forced through a series of dies until it formed a tube 13 feet long and 1⅞ inches diameter, without any crack or flaw.

A ring of metal could, at one heat, be hammered into a die to form a locomotive chimney top.

In drilling a circular hole into a plate, continuous shavings are formed—whereas, in copper, or Low Moor plates, or any other metal, the shavings break into pieces ⅛in. long.

Thin sheets of the Bessemer soft steel can be bent backwards and forwards hundreds of times without a fracture, and are almost as flexible as paper.

MICROSCOPICAL SECTION.

Meeting, December 16, 1861.

E. W. Binney, *F.R.S., F.G.S., in the Chair.*

Dr. Edward Morgan was elected a member of the Section.

Dr. Wallich kindly presented to the Section for mounting several specimens of material, from his private collection, containing Biddulphia of various kinds, and other diatomaceæ, from Guernsey, St. Helena, &c.

Mr. Thomas H. Nevill presented to the Section eight slides, mounted from the specimens of Soundings, No. 131, taken in Lat. 51° 48′ N., Long. 7° 8′ W., off the south coast of Ireland, in forty fathoms, presented by Captain Moodie, of the R.M.S.S. *Canada*. Mr. Nevill reported that the specimen contained Entosolenia Marginata, Entosolenia Squamosa, Lagena Vulgaris, Textularia, Rotolina, Miliolina. Numerous spines and plates of Echina; calcareous prisms from shells, &c., &c., all water-worn. The sand is composed of about half calcareous and half silicious material.

Mr. Latham proposed that the subject for discussion at

the next meeting should be "On the Cause of the Metallic Lustre on the Wings of the Lepidoptera, both Diurnal and Nocturnal," which was agreed to. Mr. Latham also reported upon the Ovum presented at the last meeting by Mr. Leigh. Mr. Latham presented to the Section a slide, mounted with a portion of the elytra of the Platyomus subcostatus, from Venezuela; also, an oak spangle with stellate hairs.

Mr. Binney exhibited mounted specimens of Fossil wood, from Standish, near Wigan; Trigonocarpon oliviforme, from the lower Lancashire coal bed; and the palate of the Psammodus porosus, from the mountain limestone, County Armagh.

Mr. Joy presented mounted sections of coal from Bohemia, showing woody fibre.

Mr. Whalley exhibited living ova of the Trout, one month old.

Mr. Brothers exhibited a section of Agate from Siberia; Stentor Müller, &c.

NOTICES OF PATENTS.

792. *Preserving Fermented Liquors.* Henry Medlock, Great Marlborough Street, London. Dated March 30, 1861.

This improvement consists in adding sulphurous acid or a soluble sulphite to beer and similar liquors after fermentation, or introducing these agents into the bottles, casks, or other vessels about to receive the same. Such addition has the effect of preventing acetous fermentation, or the tendency to become sour on keeping.

794. *Lubricating Compound.* O. Earle, Liverpool. Dated March 30, 1861.

For the preparation of a cheap and efficient lubricating agent, the patentee recommends the employment of an oil combined or incorporated with lime water.

The addition of lime water is not calculated to aid in an economical point of view; and wherever such a material is applicable it is more than probable that soap water would answer the purpose equally well.

Grants of Provisional Protection for Six Months.

2658. George Davies, Serle Street, Lincoln's Inn, London, "Improvements in lamps for burning coal oil and similar fluids."—A communication from Joseph Thomas, New York, and Joseph Tindall Van Kirk, Philadelphia, U.S.—Petition recorded October 24, 1861.

2726. Eugene de Bassano and Adolphe Brudenne, Brussels, "Improvements in the manufacture of stearine."—Petition recorded October 30, 1861.

2815. François Henry Marie Come Damiens Chevalier Finis de Lacombe, Paris, "Improvements in generating hydrogen gas for illuminating or other purposes, and in apparatus used therein."—Petition recorded November 9, 1861.

2861. Henry Bird, Liverpool, "Improvements in the construction of bottles and other vessels, and in stoppers for the same to indicate that they contain poison."—Petition recorded November 13, 1861.

2906. Simon Dede, Rue Duvivier, Paris, "A new process of discolouring, purifying, and improving varnish, oil, resin, gum, ether, wines, spirits and other matters through the application of compressed air."—Petition recorded November 19, 1861.

2924. George Henry Polyblank, Gracechurch Street, London, "A new or improved method of protecting and preserving photographic and other prints, water-colour drawings and other works of art from injury and decay."

2961. Alfred Vincent Newton, Chancery Lane, London, "An improved method of removing and preventing the formation of calcareous and saline deposits in steam boilers."—A communication from Lewis Baird, Cambridge, Massachusetts, U.S.

2991. William Clark, Chancery Lane, London, "Improvements in the construction of parts of electric telegraph belt apparatus, and in apparatus used in making the same."—A communication from Pierre Desire Prud'homme, Boulevart St. Martin, Paris.

2997. Henry Wilde, Manchester, "Improvements in magneto-electric telegraphs, and in apparatus connected therewith."

3002. Peter Spence, Newtown Heath, near Manchester, "Improvements in the treatment of ores for the manufacture of sulphuric acid, and in apparatus connected therewith, which apparatus is also applicable to the treatment of ores for separating metals therefrom."

3044. Richard Archibald Brooman, Fleet Street, London, "Improvements in albums or books for containing and showing photographic and other pictures, and in slides for the same."—A communication from Henry Strauss, Paris.

Inventions protected for Six Months by the Deposit of Complete Specifications.

3112. Marc Antoine François Mennons, Furnival's Inn, London, "An improved means of defecating and purifying cane and other saccharine juices."—A communication from Louis Marie Amand Achille de Courson de la Villeneuve, Caen, Normandy, France.—Deposited and recorded December 12, 1861.

Notices to Proceed.

1958. Peter Spence, Newtown Heath, near Manchester, and James Mellor, Manchester, "Improvements in separating copper from its ores."

2023. Richard Archibald Brooman, Fleet Street, London, "Improvements in coating wire with copper, silver, gold, or other metal or alloy, in order to prevent oxidation."—A communication from Martin Miller, Vienna.

2067. Richard Archibald Brooman, Fleet Street, London, "An improvement in preserving meat and other animal substances."—A communication from Jean Pierre Liès-Bodard, Strasbourg, France.

2347. Rene Prudent Patrice Dagron, Paris, "An improved microscope to be used for exhibiting photographic views and productions."—Petition recorded September 19, 1861.

2441. Pierre Alexis Francisse Bobœuf, Paris, "The preparation and application of certain new hemostatic and antiseptic agents."—Petitions recorded September 30, 1861.

3005. Jules d'Adhemar de Labaume, Dorset Terrace, Clapham Road, Surrey, "Improvements in machinery for cooling and freezing water and other fluids."—A communication from Edouard Blée, Rue d'Amboise, Paris.—Petitions recorded November 28, 1861.

CORRESPONDENCE.

Chemical Nomenclature.

To the Editor of the Chemical News.

Sir,—In the Chemical News of the 7th December, you inserted a question that I wished to ask Mr. Newlands, to ascertain whether his new system of nomenclature would be of any service to chemists: my question was, "What name would he give to albumen that should express its composition?" I might have chosen substances with even more complicated formulæ than this; but could Mr. Newlands have given me a short and easily to be remembered term for albumen, and which should, as he says, "express its composition," I should have been quite content, and almost ready to become one of his disciples.

Instead, however, of attempting to answer my question,

he quietly puts it on one side, by saying, "that when the formula of albumen is established beyond doubt, it will be time enough to think of constructing a name expressive of its composition; but whether a new appellation be devised or not, I would always retain the name Albumen, my desire being to retain, as far as possible, names which are associated with the labours of illustrious men," &c. Mr. Newlands calls the above a reply to my question, but most certainly it is nothing of the kind: to me it appears an attempt, and a very poor one too, to get out of a difficulty.

If all the bodies, for which Mr. Newlands cannot build up one of his extraordinary names, which it is scarcely possible to pronounce, much less to remember, are to be considered of uncertain composition, and retain their original names; and every substance whose name is associated with its discoverer is to be in the same predicament, we shall have to reconcile ourselves to having two different systems of nomenclature: one, the original, which I imagine will contain the larger proportion of substances; and the other, that euphonious one of Mr. Newlands. Mr. Newlands will thus, instead of simplifying the complicated state of the vast subject he has taken in hand, have succeeded in making confusion more confounded.

Until he (Mr. Newland) can construct a new system, not so difficult to remember as the present one, but which, however, must include all bodies, not excepting Albumen, and bodies of such uncertain composition, I think it would be better for him not to grapple with such an extensive subject.—I am, &c. INQUIRER.

On the Estimation of Nitrogen.

To the Editor of the CHEMICAL NEWS.

SIR,—In the Number of the CHEMICAL NEWS for November 2, Mr. Walker takes exception to some remarks of mine, upon a process proposed by him for the Estimation of Nitrogen. I regret very much that he should conceive that I have done him injustice; certainly nothing was further from my intention. But I believe it to be the duty (although often an unpleasant one) of all chemists to point out such errors as they may notice in new processes proposed for analysis.

As to the point in question, I can only say that it does not admit of an argument. No number of empirical results can avail to support a method which can be demonstrated to be essentially erroneous in principle.

The accuracy originally claimed for the process, seems now to be virtually abandoned, and it is claimed that it gives good approximate results.[1] Even supposing this to be true, there can be no advantage in bringing forward a plan based on a radical error, when very good approximate results can be obtained for technical purposes, and with much more rapidity, by volumetric analysis as proposed by Péligot.

An opinion has been expressed on this subject, by an adequate and wholly independent authority, and to that I desire, in conclusion, to refer. Messrs. Kopp and Will notice Mr. Walker's process, and not favourably. In their *Jahresbericht* for 1860, p. 630, they remark:—

"J. Walker proposes a method for the estimation of nitrogen, depending upon a complete ignorance of the behaviour of ammonia to a zinc solution. He conducts the ammonia disengaged by treating nitrogenous substances with soda-lime, into a solution of chloride of zinc, and calculates the quantity of nitrogen from the weight of the zinc oxide precipitated. M. Carey Lea also points out the faultiness of the proposed process."

I write these lines with considerable reluctance, as I especially dislike controversy, nor shall I again return to the subject. I am, &c.

M. CAREY LEA.

Philadelphia, December 17.

P.S.—An article of mine on the subject of the "Action of Reducing Agents on Nitrite of Ethyl," is reprinted in the same Number above referred to, and by an error of the press, the word "nitrite" has been made "nitrate," both in the heading of the article and in the 2nd paragraph. Elsewhere it is correctly given. Also in the 3rd paragraph, "heated with $PtCl_2$" should read "treated with $PtCl_2$."

[1] The mode adopted by Mr. Walker for determining how much oxide of zinc sal-ammoniac is capable of holding in solution, I consider to be altogether fallacious. To test the dissolving power of a very weak solvent, it must always be placed in contact with a large excess of the matter to be acted upon, (especially in the present case, as that is the position which the sal-ammoniac occupies in his mode of analysis), and the amount dissolved be determined. By taking a reverse course, and adding the weak solvent until the whole of the solid matter dissolves, the quantity of solvent necessary is greatly overestimated, and the most discordant results may be obtained, all incorrect. This is well exemplified in Mr. Walker's own determinations, in one of which 6·15 grs. oxide of zinc required 450 grs. of sal-ammoniac to dissolve it, and in the other the same quantity of oxide of zinc required 560 grs. of sal-ammoniac. The inaccuracy here involved invalidates Mr. Walker's subsequent arguments.

MISCELLANEOUS.

Royal Institution.—On Friday, January 17, at eight o'clock, Professor Tyndall will deliver a lecture on "The Transmission of Heat through Gases."

Chemical Society.—The next meeting of this Society will take place on Thursday, the 16th inst., when the following papers will be read:—"On the Simultaneous Variations of Hippuric and Uric Acids in Healthy Urine," by Dr. Bence Jones; "On the Solubility of Sulphate of Lead in Hydrochloric and Nitric Acids," by Mr. G. F. Rodwell; "On a New Mode of Effecting Chlorine Substitutions," by Dr. H. Müller.

Tobacco Poison.—It is considered by the most reliable authorities that the tobacco crop of the whole world amounts to 250,000,000 kilogrammes per annum. Now, Schlosing has found that, taking one kind with another, there is an average of five per cent. of nicotine in the leaves of the plant; it is clear, therefore, that about twelve millions and a-half of kilogrammes of this poison are annually produced. As the specific gravity of nicotine very slightly exceeds that of water, this quantity would fill nearly 100,000 wine barrels, and would give twelve and a-half grammes (293 grains) to every man, woman, and child on the globe. As a few drops will produce death, it is probably much within the mark to say that the nicotine from one year's crop of tobacco would destroy every living creature on the face of the globe, if its proportion were administered in a single dose.

ANSWERS TO CORRESPONDENTS.

*** All *Editorial Communications* are to be addressed to the EDITOR; and *Advertisements* and *Business Communications* to the PUBLISHER at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

*** In publishing letters from our Correspondents we do not thereb adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

Vol. IV. of the CHEMICAL NEWS, containing a copious Index, is now ready, price 12s., by post, 12s. 8d., handsomely bound in cloth, gold-lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. A few copies of Vols. I. II. and III. can still be had. Vol. V. commenced on January 4, 1862, and will be complete in 26 numbers.

R. A. P. S.—We are unable to furnish the information required.

J. A. Horsley.—We think the discussion had better end.

J. A. Davies.—Declined with thanks.

Dr. Ballard's communication is unavoidably postponed till the next Number.

J. M. C.—You can purchase pure permanganate of potash in beautiful crystals, at a manufacturing Chemist's. Its preparation is attended with some difficulty unless you have a good laboratory.

THE CHEMICAL NEWS.

VOL. V. No. 111.—January 18, 1862.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Researches on the Relations existing between Atomic Weights, by M. J. S. STAS, Member of the Royal Academy of Belgium.

(*Concluded from page* 16.)

Conclusions.—According to Prout's law, we have the following equivalents for the seven simple bodies which I have submitted to examination :—

Chlorine . . .	35·5
Sulphur	16·0
Nitrogen . . .	14·0
Silver	108·0
Potassium . . .	39·0
Sodium . . .	23·0
Lead	103·5

In order to enable every one to judge for themselves whether the hypothesis of Dr. Prout is conformable or not to experiment, I put below, in parallel columns, the proportional relations resulting from my determinations, together with those which are derived by calculation according to this hypothesis :—

According to Experiment, the proportional relation	*According to Prout's hypothesis, the proportional relation*

Between Silver and Chlorine

Is, minimum	= 100·000 : 32·841	is, as 100 000 : 32·870
maximum	= 100 000 : 32·850	
mean	= 100·000 : 32·8445	

Between Silver and Sulphur

minimum	= 100 000 : 14·849	100·000 : 14·814
maximum	= 100 000 : 14 854	
mean	= 100·000 : 14·852	

Between Silver and Nitrate of Silver

minimum	= 100·000 : 157·463	100·000 : 157·404
maximum	= 100·000 : 157·481	
mean	= 100·000 : 157·473	

Between Silver and Chloride of Potassium

minimum	= 100·000 : 69·099	100·000 : 68·981
maximum	= 100·000 : 69·107	
mean	= 100·000 : 69·103	

Between Silver and Chloride of Sodium

minimum	= 100·000 : 54·206	100·000 : 54·166
maximum	= 100·000 : 54·209	
mean	= 100·000 : 54·2078	

Between Silver and Chloride of Ammonium

minimum	= 100·000 : 49·590	100·000 : 49·537
maximum	= 100·000 : 49·600	
mean	= 100·000 : 49·5944	

Between Nitrate of Silver and Chloride of Potassium

minimum	= 100·000 : 43 864	100·000 : 43·823
maximum	= 100·000 : 43·885	
mean	= 100·000 : 43 878	

Between Nitrate of Silver and Chloride of Ammonium

minimum	= 100·000 : 31·486	100·000 : 31·470
maximum	= 100·000 : 31·490	
mean	= 100·000 : 31·488	

Between Lead and Nitrate of Lead

Is, minimum	= 100·000 : 159·959	is, as 100 000 : 159·903
maximum	= 100·000 : 159·982	
mean	= 100·000 : 159·969	

Between Lead and Sulphate of Lead

minimum	= 100·000 : 146·419	100·000 : 146·376
maximum	= 100·000 : 146·443	
mean	= 100·000 : 146·427	

Between Chlorate of Potash and Chloride of Potassium

minimum	= 100·000 : 60 838	100·000 : 60·8163
maximum	= 100·000 : 60·853	
mean	= 100 000 : 60·846	

Between Sulphate of Silver and Silver

minimum	= 100·000 : 69·197	100·000 : 69·230
maximum	= 100·000 : 69·209	
mean	= 100·000 : 69·203	

We perceive, then, that with the exception of the proportional relations between silver and chlorine, and silver and sulphate of silver, there is throughout a difference in excess between the figures obtained by experiment and those obtained by calculation according to Prout's hypothesis.

This difference is, for the mean :—

In the case of sulphide of silver $\frac{38}{14814}$, or eight times larger than that existing between the minimum and the maximum.

In the case of nitrate of silver $\frac{74}{157404}$, or four times greater than that existing between the minimum and the maximum.

In the case of silver and chloride of potassium $\frac{127}{68081}$, or thirteen and a-half times greater than that between the minimum and the maximum.

In the case of silver and chloride of sodium $\frac{42}{54166}$, or fourteen times greater than that between the minimum and the maximum.

In the case of silver and chloride of ammonium $\frac{57}{49537}$, or six times greater than that between the minimum and the maximum.

In the case of nitrate of silver and chloride of potassium $\frac{55}{43523}$, or three times greater than that between the minimum and the maximum.

In the case of nitrate of silver and chloride of ammonium $\frac{18}{31470}$, or six times greater than that between the minimum and the maximum.

In the case of nitrate of lead $\frac{66}{159903}$, or three times greater than that between the minimum and the maximum.

In the case of sulphate of lead $\frac{51}{146376}$, or twice as great as that between the minimum and the maximum.

It is impossible in this *résumé* to apportion the true value to all the differences, which would be considerable in some cases, but appear small in others.

I will discuss the results of the determinations of five substances. Indeed, it would suffice to demonstrate that Prout's law is inapplicable to two elements in order to upset the hypothesis. These five bodies are silver, nitrate of silver, chloride of potassium, chloride of sodium, and chloride of ammonium.

According to Prout's hypothesis, the proportional relation between silver and chloride of potassium is as

100·000 : 68·981.

I have found that it is as

100·000 : 69·103.

M. de Marignac has made this determination previous to me.[1] Upon reducing his results to a vacuum he found that

100·000 of silver	=	69·105	of chloride of	potassium.
,, ,,	=	69.077	,,	,,
,, ,,	=	69·0995	,,	,,
,, ,,	=	69·1027	,,	,,
,, ,,	=	69·0995	,,	,,
Mean	=	69·098	of chloride of	potassium.

Rejecting the second experiment, which is evidently affected by accidental error, we have for a mean of the five experiments 69·102, which only differs by a hundred-thousandth part from the result of my twenty-five determinations.

Let us now examine what causes may vitiate the accuracy of this result. On the one hand there is the means of effecting the determination; and on the other hand there are the impurities in the materials made use of. Gay Lussac's method may introduce a constant cause of error; but, properly used, it allows of the detection in a litre of liquid of the twentieth of a milligramme of silver, which is but the eighty-thousandth part of the weight of the silver used by me in one experiment. After the use which the Mint Laboratories have made of this method for more than a quarter of a century, we may, I consider, be perfectly satisfied as to the results which it furnishes. If, therefore, there is a cause of error, it ought to be sought for in the uniform presence of impurity in the materials. The details into which I have entered to describe the processes by means of which I prepared the materials, will enable any one to judge of the considerable efforts which I have made to guarantee their purity. To obtain chloride of potassium, I put into operation all the powers which the actual state of science permitted me to apply to the preparation of substances in their greatest state of purity.

The very extraordinary concordance between the results furnished by chlorides of very different origins and produced by diametrically opposite means—this concordance, I say, is a better testimony than any possible argument to their purity which is otherwise so difficult to guarantee. Moreover, the agreement which is observed between my determinations and those of M. de Marignac is an additional proof of purity.

What I have just said with respect to chloride of potassium is applicable to chloride of sodium.

The impurities which, against all probability, might be discovered in these two chlorides, would increase their proportional relations. But without denying the possibility of the existence in these bodies of an unknown foreign substance, there is no chemist who, after having attentively examined all the means which I have employed to procure them, would venture to pretend that the enormous and uniform difference observed by M. de Marignac and myself between the calculated and the experimental numbers, can be attributed to the presence of a known foreign body.

Bearing in mind the processes employed by me to procure chloride of ammonium, it is impossible that there could be found in certain specimens of the salt made use of, bodies foreign to the composition of sal-ammoniac itself. Although I have done all in my power to avoid an excess of hydrochloric acid or of ammonia, it may happen that certain specimens contained a very slight excess of acid; but the presence of this latter body would tend to diminish its proportional relations, and all my experiments lead to a more elevated proportional number for chloride of ammonium.

I may add that, if the existence of unknown foreign bodies should raise the proportional number of the chlorides of potassium and sodium, the possible, and even probable, presence of foreign bodies in the silver would diminish this proportion; the effects produced would be in opposite directions, and the chance of augmentation would, therefore, be diminished by one half.

Let us now see if it is possible to reconcile my syntheses of nitrate of silver with Prout's law. I have demonstrated that 100·000 of the metal produced as a mean 157·473 parts of this salt instead of 157·404; there is, therefore, $\frac{1}{2281}$ths of excess over that required by Prout's law. It may be pretended that this excess of weight is due to nitric acid or to water retained by the fused nitrate. I will first recall that I have shown the neutrality of the salt obtained in my experiment, and the absence of tension when it was fused *in vacuo*. Moreover, this objection falls to the ground in the presence of the direct control to which I submitted the nitrate of silver itself by determining its proportional relation in respect to chloride of potassium.

According to Prout's law this relation is as 100·000 : 43·823. But I have found, as a mean of three series of experiments, that this relation is as 100·000 : 43·878. M. de Marignac[2] has already obtained the same relation as a mean of two series of determinations. But by combining the result of my synthesis of nitrate of silver with the result of my determinations of the proportional relation between silver and chloride of potassium, I arrive at this conclusion, that 100·000 of nitrate of silver are equivalent to 43·883 of alkaline chloride figures, which are almost identical with those found by experiment. There is, therefore, a perfect agreement between the syntheses of nitrate of silver and the determinations of the proportional relation between this salt and chloride of potassium, and a complete disagreement with the numbers calculated for the three bodies according to Prout's principle. The composition of nitrate of silver, whether determined by synthesis or established from its proportional relation in reference to a known chloride, is, therefore, incompatible with Prout's law. It is, therefore, a necessary result that, amongst the constituent elements of this body, two at least, nitrogen and silver, do not obey this principle.

If it is radically impossible to reconcile Prout's hypothesis with the relations which the atomic weights of nitrogen, silver, and the chlorides of potassium, sodium, and ammonium, present one to the other, there is no reason to admit it in respect to chlorine, sulphur, and lead, if even the numerous determinations given in this memoir did not prove in an evident and certain manner that these bodies do not obey Prout's law.

To conclude this long account, it remains for me to state the value of the atomic weights as derived from my researches.

By the calcination of chlorate of potash I found that 100·000 of this salt furnished on an average oxygen	39·157
By the action of hydrochloric acid I found it to contain an average of	39·151
The mean of these two results is	39·154

[1] *Bibliothèque Universelle de Genève*, vol. xlvi. p. 352.

[2] *Bibliothèque Universelle de Genève*, vol. xlvi., p. 365.

Berzelius found a mean of	39·150
M. Pelouze obtained a mean of	39·156
M. de Marignac obtained a mean of	39·161
Oxygen being represented by	8
Chloride of potassium becomes	74·59
And the atomic weight of silver	107·943
,, ,, chlorine, according to my experiments	35·46
,, ,, chlorine, according to M. de Marignac	35·468
,, ,, potassium	39·13
,, ,, sodium	23·05
,, ,, ammonium	18·06
,, ,, nitrogen (deduced from the synthesis of nitrate of silver)	14·041
,, ,, of sulphur	16·0371
,, ,, lead (synthesis of sulphate of lead)	103·453
,, ,, lead (synthesis of the nitrate dried *in vacuo* at 155°)	103·460

On glancing at the atomic weights of ammonium and of nitrogen, it will be perceived that they differ by 4·02 instead of by 4·00. It necessarily follows that either my syntheses of nitrate of silver are inexact, and that the quantity of this salt produced from metallic silver is a little greater than I have found it (which removes me still further from Prout's law), or else that the atomic weight of hydrogen itself is wrong by $\frac{5}{1000}$ths or $\frac{1}{200}$ths of its value.

The results of my researches lead me to think that the error exists rather in the atomic weight of hydrogen than in that of nitrogen.

If this statement, to which I draw with diffidence the attention of chemists, is correct—and this I am about to investigate by performing the synthesis of water by a new method—it results that the basis upon which Dr. Prout established his law has itself no foundation.

Whatever may be the doubt which I hazard in respect to the accuracy of the atomic weight of hydrogen represented by 1, oxygen being 8, there can be no doubt concerning the principle itself. I therefore conclude by saying, so long as recourse must be had to experiment to establish the laws which govern matter, we must consider Prout's law as a pure illusion, and must regard the undecomposed bodies of our globe as distinct entities having no simple relation of weight among themselves. The undoubted analogy of properties which may be observed between certain elements must be sought for amongst other causes than those which are derived from the relation between the weights of their acting masses.

TECHNICAL CHEMISTRY.

On the Production of Nitrate of Soda at Iquique (Peru).

As Iquique is the centre of this trade, and to it its present importance is wholly to be attributed, it is thought to be advisable to convey in this report as much information as can be procured as to this article, and in order that such information shall be truthful, the writer has availed himself of the views of several Englishmen at present engaged in the trade.

About from six to fourteen leagues from the coast, and running parallel with it through the province, at an elevation of 3300 feet or thereabout, is the Pampa of Taramugal. This plain or pampa was a sea lake, and the greater part is covered with salt along the western border; and generally not extending eastwards more than 500 yards from the verge of the old lake is found the "caleche" or "terra salitrosa," rough nitrate. Between the pampa and the coasts exist other old sea lakes, on the borders of which "caleche" is also found; but these deposits are of secondary import. The "caleche" is generally found in insulated masses, irregular in shape and thickness, which adds greatly to the expense of working. It is sometimes found with only a few inches of sand over it, but more frequently covered with a hard stone, consisting of sand indurated with salt; this is called "costra," the thickness of which varies from one to ten feet, but averages three feet. The "caleche" varies in thickness from one to nine feet, but in general runs from three to four feet; below this exists a soft sand, containing an abundance of crystals of glauberite and small quantities of borates of lime and soda. The strata consist of—

1. Loose sand, a few inches thick.
2. Hard sand, indurated with salt, from one to ten feet thick.
3. "Caleche," from one to nine feet thick.
4. Soft sand, or cora.

The "caleche" varies in quality from nearly pure salt to 50 and 60 per cent. of nitrate, generally containing the following substances:—

Earthy matter.
Nitrate of Soda.
Chloride of Sodium.
Sulphate of Soda.
,, Lime.
And traces of Chloride of Magnesium, and Iodides and Bromides.

It is impossible to state the respective proportions, as they vary with every different sample. The method of extracting and refining nitrate of soda is as follows:—

When "caleche" is required, the barretero (miner) makes holes in the ground where he expects to find it. If successful, he fills up the holes with coarse gunpowder made on the spot (costing three and a-half dollars per quintal), regulating the charge in proportion to the thickness and hardness of the "costra" and the thickness of the "caleche;" the charge varies from one to eight quintals, and occasionally as much as fourteen quintals; when blasted the whole mass is turned over and mixed. He then proceeds to separate the "costra" and "cora" from the "caleche," throwing aside all the latter that he does not believe to contain more than ten or twelve per cent. of nitrate; it is then broken into smaller lumps, to be conveyed to the "paradas." A refinery of nitrate is called an "Officina," and is generally placed in the centre of the calecheros or nitrate grounds, and consists of one or more paradas; a paradas is a pair of round iron boilers, each holding from 70 to 300 gallons; these are placed together, in rough stonework, with a fire-place between them. At the parada, the acendrador breaks the lumps into pieces about the size of a fist, rejects the inferior pieces, so as to bring the whole to about 25 to 34 per cent. of nitrate. It is now thrown into the boilers with a quantity of water; after boiling some two or three hours, the fondeador (boiler) continually stirring the mass, supposing that the caleche is by that time exhausted, throws out the ripio (refuse), adds more caleche and mother water; and, after boiling some two or three hours, a well saturated solution is obtained; it is then by hand baled into a deposit, from whence, as soon as the mud and salts are deposited, it is baled into shallow coolers, where it crystallizes. The mother water is then drawn off, and the nitrate thrown out to

dry. The paradas are charged twice a-day, and the daily product is from fifteen to twenty quintals of nitrate, containing about 3 per cent. of impurities, chiefly common salt. The average cost of a quintal of nitrate is :—

Barretero, breaking out	12½ cents.
Acendrador, assorting	6¼ ,,
Fondeador, boiling	12½ ,,
Powder for blasting	6¼ ,,
Asses bringing the caleche to the paradas	3 ,,
20 lbs. coals at 1·50 dol. per quintal	30 ,,
Wear and tear of parada, reparations, and depreciations	29½ ,,
	1·00 dol.

This system of making nitrate is the same as was first adopted at the commencement of the trade, and unquestionably well adapted for that early period, having the advantage of being simple, easily understood and worked; yet it is still continued, and the whole system of labour arranged to it. It is almost impossible to conceive a system more rude and more wasteful; and although many exertions have been made during the past ten years, without success, to improve it, yet that want of success has been caused chiefly by the lack of skilled labour in the province; still there is no doubt that it will be superseded, in the course of a few years, by the more refined and complicated apparatus now being introduced. The theory of the process of refining nitrate is this :—"Caleche" consists of nitrate of soda, chloride of sodium (common salt), and earthy matter (the other substances present exist in such small quantities that they are overlooked), and as chloride of sodium is very little more soluble in boiling than in cold water, whilst nitrate of soda is comparatively insoluble in cold, but very soluble in hot, water, it is very evident that it is only required to add such quantities of "caleche" to boiling water to procure a strongly saturated solution; the earthy matter, being insoluble, is left with the excess of common salt in the boiler, or the deposit, before it is discharged into the coolers, where, as the liquid cools, it deposits the excess of nitrate of soda, the mother liquor retaining all the salts in solution. Reverting to the customary process of refining, two systems are now being tried, which use steam; in the one (Gamboni's patent) the "caleche" is placed in an inverted semi-cone, with a perforated cover and bottom; through the side a jet of steam is introduced, mother water is thrown on the cover, and the refined nitrate falls through the bottom, and is at once conveyed to the coolers; in the other, steam is introduced to boil the solution; but both promise the same advantages—economy in the make, and a superior article.

No sketch of the nitrate trade would be complete without some reference to the abuses. In the first place, it is badly based. The merchant makes advances to the saliteros, officineros (makers), of money and goods, on the promise of receiving in return the product of the officina. This advance frequently is used in paying off old debts, or in advances to the labourers. The merchant must still keep advancing barley for the troops, coals and provisions for the labourer, &c., or there will be no nitrate forthcoming. This system trenches heavily upon the merchant's resources, and occasionally leads to losses. The officineros, as a body (with some exceptions), are a reckless set of men, wasteful in their expenditure and careless of their promises. Their arrangements with their labourers are also bad; their principal ones, the barretero, acendrador, and fondeador, being paid according to the product of the parada, recriminations are ever recurring, and not unfrequently leading to a closing of the works. Another thing must also be noticed—the great amount of adulteration that has taken place within the three past years. Rarely a cargo leaves that is better than 5 per cent., some even 7 to 10, and some samples assayed have shown as much as 30 to 50 per cent. of foreign matter. The adulteration is effected in two ways; in one, white "caleche" is ground and mixed with the refined nitrate,—this is called green nitrate: the other, the powdered "caleche, is mixed into the solution, and at once put into the coolers,—this is dirty nitrate. This is in some measure protected by the present state of the trade. Merchants in England purchase from the importer, and get a deduction from him corresponding to the amount of foreign matter in the article; but as the general sales are made without any deduction, then the worst cargoes are the most profitable to the merchants.

The province has not been thoroughly surveyed; but enough "caleche" has been discovered to yield an increased supply for ages. In May, 1856, there were about 100 officinas at work, with about 250 paradas; but the work is not constant, 240 days is a good year's work. The principal sales of this article are made in Valparaiso, on the usual terms, viz., ore well sacked, not to contain less than 95 per cent. of nitrate placed in the ship's launch outside the surf. The price has been very fluctuating, commencing at 18 reals, rising to 20 reals, falling to 16 reals, and then in four months rising to 23 reals, but taking an average price of 19 reals; 936,719 quintals, with the exchange at 46 dollars, would give 426,402*l.* 5s. 10¾d. The other salts found in the province are chloride of sodium, biborates of lime and soda, sulphates of lime and soda, magnesian alum, &c. Iodine exists with the nitrate, and throughout the calecheros traces of boracic acid have been found in the water.—*From the Report of H.B.M. Consul.*

PHYSICAL SCIENCE.

The Divisibility of Matter.

It is curious to observe how purely conventional are the terms *large* and *small, great* and *little*, and how meaningless they are when they cease to have relation to matters of every-day experience to our senses. They are simply relative, and depend for their significance upon some artificial comparison instituted in our own minds, which has no connection whatever with the absolute amount of substance regarded in the abstract. The same quantity may thus appear very large or very small, according to the way in which it is viewed, and the size of the bodies with which it is mentally associated; thus, a pound of any mineral may seem an insignificantly small quantity to the miner accustomed to deal with hundreds and thousands of tons; the same lump may appear a large cabinet specimen to the mineralogist, whilst, on the other hand, to the natural philosopher engaged in the detection of its various constituents by means of spectrum analysis, and working on thousandths of a grain, this weight becomes an enormous quantity. The same weight of different bodies likewise appears large or small according to the scarcity or commonness of the material; thus, an ounce of diamonds would fall under the first denomination, whilst an equal weight of coal would be looked upon as a very small quantity. If, however, we cease to associate our ideas of any substance with the purely conventional qualities which it acquires by comparison

with other bodies, the terms large and small will be found to be capable of infinite extension either way, until it becomes evident that in the physical universe the properties which they are supposed to represent have no existence, but are merely artificial distinctions called into being by the imperfections of our senses.

We have been led into these reflections by some theoretical and experimental inquiries which have come under our notice respecting the constitution of matter; and some of the results obtained in the endeavour to ascertain the limits of mechanical divisibility of bodies are so curious, and illustrate so forcibly the above remarks, that we are induced to place them at once before our readers.

The divisibility of matter is a subject which has been frequently treated of and reasoned upon; indeed, in most works on physics or chemistry, illustrations are given of the extraordinary degree of subdivision to which bodies may be brought by mechanical means. One of the best illustrations of this is to be found in Miller's "Elements of Chemistry," part I., page 4, where the author shows to what a minute state of division it is possible to bring the metal gold. All these illustrations have, however, stopped far short of the limit attainable by mechanical means, and indeed have been merely given to illustrate extreme subdivision without pushing the subject to its legitimate extent. The following experimental illustration shows what infinitesimally minute quantities the natural philosopher is capable of working with and rendering evident to the senses (or rather sense, for sight alone can appreciate them), and will also show how conventional are the ordinary ideas of magnitude.

We will start with a sheet of gold leaf. This consists of metallic gold beaten out into a film of about the 1-282,000th of an inch in thickness, measuring 3·375 inches square and weighing about the 1-5th of a grain. A single square inch therefore weighs 1-57th of a grain. Now, Faraday in his beautiful researches on the relations of gold to light[1] has shown that it is possible by chemical means to reduce this thickness very considerably, still preserving the metallic continuity of the film. This is readily effected by breathing on a clean plate of glass and then gently placing it on a piece of gold leaf; the latter will adhere to it, and if distilled water be immediately applied at the edge of the leaf, it will pass between the glass and gold, and the latter will be perfectly stretched: upon now draining the water out, the gold-leaf will be left well extended, smooth, and adhering to the glass. If, after the water is poured off, a weak solution of cyanide of potassium be introduced beneath the gold, the latter will be gradually dissolved away, becoming thinner and thinner; but at any moment the process may be stopped, the cyanide washed away by water, and the attenuated gold film left on the glass. If towards the end, a washing be made with alcohol, and then with alcohol containing a little varnish, the gold film will be left cemented to the glass. By this means the thickness will have become reduced to about one-twelfth part of what it was originally, weighing (in round numbers) about the 1-6ooth of a grain to the square inch, and being only about 1-3,000,000th of an inch in thickness. The film in this condition, although consisting of pure gold, presents none of the ordinary appearances of the metal, being perfectly transparent, and resembling a delicate film of pale green varnish more than a dense metallic body.

[1] "Experimental Researches in Chemistry and Physics," p. 394.

Having now obtained a continuous metallic film of gold upon a plate of glass (and that it is continuous and metallic has been amply proved by Faraday), let us see how far it is possible to subdivide it by mechanical means. From an examination of Nobert's test plate, it is seen that it is possible to rule lines with a diamond point on glass so close together, that upwards of 90,000 of them are comprised in the space of one inch. The apparent limit of vision in the best microscopes, as tested by De la Rue, Quekett, and Ross, does not, however, reach beyond lines separated the 1-80,000th of an inch. Let us, therefore, cut our square inch of gold on the glass plate, with lines this distance apart, and crossing each other at right angles. The whole inch will, therefore, be divided into 6,400,000,000 squares, each of which is capable of being distinctly seen under adequate microscopic power. What now is the weight of each piece? The square inch of gold weighed at the commencement the 1-57th of a grain. By the action of cyanide of potassium, it was diminished in thickness until it only weighed the 1-6ooth of a grain. This has now been cut up into 6,400,000,000 separate pieces, each of which therefore weighs no more than the 1-3,840,000,000,000th of a grain; or, in other words, a single grain of gold—a fragment about as large as a good-sized pin's head—has been divided into three billion, eight hundred and forty thousand million separate pieces, each distinctly visible to the eye!

The mind is quite unable to attach any definite significance to these figures without artificial assistance, but it may, perhaps, enable our readers to form some faint idea of the minuteness of the subdivision, when we state that each square bears about the same proportion to the original grain of gold that a thimbleful of water does to a building five times the size of St. Paul's. How insignificant do our ideas of great and small appear in the contemplation of such overwhelming figures as these! In the eloquent words of Dr. Nicoll, "Great and little, in truth, seem in creation alike terms expressing merely relation to us, and vanish in the universe of the Infinite God."

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Six Lectures on Light[1] *(adapted to a Juvenile Auditory), by* John Tyndall, *Esq., F.R.S., Professor of Natural Philosophy in the Royal Institution.*

Lecture II. (Dec. 28, 1861.)

NOTES TO THE LECTURE:—

A ray of light falling through a vacuum as a perpendicular on the surface of any transparent solid or liquid, goes *straight* into the solid or liquid—But if the ray strike the surface obliquely, it is bent on entering the solid or liquid; and it is so bent as *to approach the perpendicular* — Conversely, a ray of light issuing from the solid or liquid would go straight forward if it struck the surface perpendicularly; but if it strikes it obliquely it is bent *from* the perpendicular—This bending of the ray on passing from one medium into another is called *Refraction*—Gases, liquids, and solids all refract light. The quivering observed over a heated surface is due to refraction by the air—By the refraction of our atmosphere we see the sun before he actually rises; and refraction causes his disk to be slightly *flattened* when near the horizon.

The Eye sees an object from which a ray issues in the direction of the ray as it enters the eye. In consequence of this, and of refraction, the bottom of a lake or river appears lifted up, the water seeming to be shallower than it really is—Hence, if we thrust a straight stick obliquely into water, its immersed end will appear higher than it really is, the stick being apparently bent upward where it quits the water—The bending of the stick must be carefully distinguished from

[1] Reported verbatim by special permission.

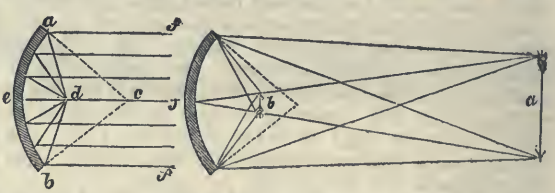
the bending of the *rays*; they are in opposite directions—Bodies refract light in very different degrees; the refraction produced by water is much greater than that produced by air, while the refraction by glass or turpentine is greater than that produced by water. With one exception the diamond is the most powerful refractor known, and to its power in this respect its brilliancy is due—The angle inclosed by the direct ray and the perpendicular is called the angle of incidence; the angle inclosed between the refracted ray and the perpendicular is called the angle of refraction. However these angles may vary in size, there is for each individual substance a fixed relationship between them—[The older boys will understand this relation. It is this: *the sine of the angle of incidence, divided by the sine of the angle of refraction, is a constant quantity for each particular medium.* In glass this quotient is about 1½. No matter, then, how the obliquity of a ray falling from a vacuum upon glass may vary, the sine of the angle of incidence is always as long and a-half as the sine of the angle of refraction. This is a celebrated law, and ought, if possible, to be understood. The constant quotient is called *the index of refraction.* In glass, as just stated, the index is about 1½, in water it is about 1⅓, while in diamond it amounts to 2½.]

If two bodies refract a ray of light equally, one of them cannot be seen *within* the other. The eye-ball of an ox, for example, vanishes if plunged into water, the refraction produced by both substances being the same. In this case *there is no reflection* at the common surface of the two media—Reflection always accompanies refraction, and the greater the refraction the more copious is the reflection; hence the wonderful power of the diamond as a reflector of light—Hence, when it is stated in the first note of this day's Lecture, that a ray of light goes straight into a transparent solid or liquid, the portion of the ray which has escaped reflection is meant—If a ray of light pass obliquely through a transparent plate with parallel surfaces, the refraction suffered on entering is exactly undone by the refraction suffered on quitting the plate, and the ray afterwards pursues a direction parallel to its original one—If the refracting surfaces be not parallel, the original direction is not restored. It is not restored, for example, in the case of prisms or lenses.

Lenses are divided into two great classes, called convergent and divergent lenses—A double convex lens is an example of the former class; divergent rays have their divergency diminished, parallel rays are rendered convergent, and convergent rays have their convergency augmented by passing through a double convex lens. In lenses, as in mirrors, the focus of *parallel rays* is the *principal focus*—A double concave lens is an example of the divergent class: convergent rays are rendered less convergent, parallel rays are rendered divergent, and divergent rays have their divergency augmented by a double concave lens—If the divergent rays produced by the refraction of a parallel beam by a double concave lens be produced backward, they will cut in a point on that side of the lens from which the parallel beam comes. This point is the principal focus of the concave lens—Thus, in the case of the double convex lens, the rays actually cut each other on the side opposite to that from which the parallel beam comes;—the focus is therefore *real.* In the case of the double concave lens the rays do not actually cut;—the focus is *imaginary.*

In a spherical lens the refraction near its edge is greater than at its centre; the central rays and the circumferential rays do not therefore converge to one and the same point—This difference between the refraction of the central and edge rays is called the *spherical aberration* of the lens—Convergent lenses produce real images which may be either larger or smaller than the object. Divergent lenses produce no real images—If a luminous object be placed at the principal focus of a convergent lens, its rays after passing through the lens do not intersect; they are parallel—If the object be placed between the focus and the lens the rays do not intersect; they are still divergent, though less so than before they entered the lens—If the object be placed beyond the focus, the rays, after passing through the lens, intersect; and an inverted image of the luminous object is formed where they cut—The images of the magic-lantern and camera-obscura are thus produced.

We concluded our last Lecture by some experiments showing the action of two different kinds of mirrors upon light. We allowed light to fall, first upon what we called a convergent mirror—the concave mirror—and we found that when the light fell upon the face of it, it was gathered up and projected and converged into a cone. I then interposed a screen where the cone came to a point, and there we found an image of the light from which the rays which fell upon the mirror emanated. We found on the other hand, in the case of the convex mirror, that when the beam of light fell upon it, the rays, instead of being converged to a point, were scattered widely in all directions and appeared as if they came from a point behind the mirror. I will just make one or two experiments with the concave mirror to-day in order to finish what I have to say upon the subject of the last lecture. Here is the same mirror we had on that occasion. You must figure to yourselves that concave mirror as a slice of a hollow sphere. If you supposed this mirror continued round, it would make a very large sphere of glass; but that small slice is, as it were, taken off that large sphere, and forms our concave mirror. Now with regard to the manner in which images are formed by this mirror. I will draw the outline of the mirror (*a e b*), in this way (see Fig.); and suppose the curvature of this mirror completed, so as to form a sphere; then when I speak of the centre of the sphere of which the mirror is a part, I mean a point (*c*) about this distance from the surface of the mirror. You must bear in mind that there is a particular distinction and emphasis laid upon rays of light which are called parallel rays which run side by side perfectly parallel without crossing each other—such rays for example as we get from the sun and the stars. Now supposing a bundle (*f f*) of these parallel rays to fall upon the mirror in this way, they will be reflected to a point (*d*), midway between the centre (*c*) of the sphere, and the surface of the mirror. This point midway between the centre of the sphere and the surface of the mirror is therefore called the principal focus—it is the focus for parallel rays. But if I bring the light nearer—here, for instance,—and allow the rays to fall upon the mirror, they will also be reflected, but will meet at some point between the centre and the focus. And if instead of having a point of light there, I had an object—an arrow for instance, that arrow would be so reflected as to form a little arrow at the focus, and if I draw this arrow (*a*) with the point bending downwards and the

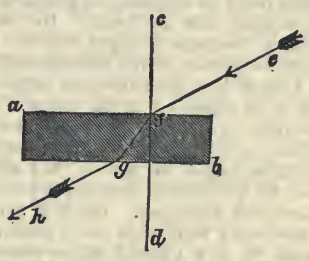


feather end up, we should on examination find the image of it with the point bent upwards and the feather end downwards (*b*) the arrow being inverted and diminished. Now, what I want you to bear in mind is this:—if you put a little arrow here (*b*) making that the object, then this arrow (*a*) becomes its image. This (*a*) being the object, that (*b*) is its image, and that (*b*) being the object, this (*a*) is the image; one being changed for the other; the image becoming the object, and the object the image. You also see now that if you put your luminous point in front of the mirror (*d*), the rays, reflected from that point, and striking upon the mirror, will go from it parallel, without intersecting at all; and if the rays come from a point between this principal focus and the surface of the mirror, when the rays strike the mirror from that point they are caused to diverge,—they are reflected outwards.

Now, with regard to an object; you have seen that if I place an arrow there (at *a*) the image of the head of the arrow would be upwards and the feather end of the arrow downwards, there (at *b*); and I find this to be precisely the case if I go near to this mirror myself. When I stand near it, between the centre and the mirror, I see an upright and enlarged image of myself. I am quite a giant, looking into that mirror. But if I go beyond the focus, what do I see? I see an inverted image of myself in the air:—there I stand with my head downwards; the hand of the image moves with mine like a spectre or a ghost—for there it is as plainly as possible in the air in front of me. It does exactly the same to me as I do to it, and when I point a stick at it it is so like a man driving a stick into my face that I can hardly strike out without winking my eyes. Our fingers are actually mingled; I am now shaking hands with the fingers of the spectre. There, it is grasping mine, as plainly as I grasp it. And just in accordance with what I have been saying to you, if I could stand upon my head there in the air—which, however, I cannot do, and which none pretend to do but those people who call themselves spiritualists, who regard the laws of this glorious

nature by which we are surrounded as nothing at all (for those people who absolutely have never taught us anything about these things, who are altogether ignorant of the laws and constitution of nature, pretend to be able to do things that we confess we cannot do)—however, if I could stand there with my feet in the air, resting upon nothing, then what would take place? Why, you would see upon the screen a magnified and upright image of myself. Now, as I profess my inability to stand with my feet in the air, resting upon nothing, I must ask you to accept a substitute. I will place a person just there, where the spectre stands who is shaking his stick in my face, and I will illuminate his face, and will try if I cannot actually cast an image of his face upon the screen. And will this image be erect or inverted? will it be upright or downward? It will be inverted, no doubt of it, let us try it now. We have our light simply for the purpose of illuminating this man's face. [The experiment was then performed, and the face of the assistant illuminated by the electric light, being placed between the principal focus and the centre of the mirror, an enlarged and inverted image of the face was projected upon the screen.] You may make all these experiments for yourselves with a much smaller apparatus. Here is a very small concave mirror, and when I look close into it, I see myself upright, and when I hold it at a distance I see myself inverted; and I will try and project an image with it to show that it may be made use of in that way also. Here are two medals which I will illuminate by means of the electric light, and will then throw an image of them by means of this concave mirror upon the screen. Taking the copper one first, I hold it, as you see, with the head downwards, but by virtue of the action of the mirror it is inverted, and you see we get an upright image. Here I have a silver medal, the image of which you also see on the screen. You can take a candle and try these experiments by means of it, and you will find the candle will be turned upside down by a little mirror of this kind—the flame will go downwards when the image is thrown clearly upon the screen.

I have now to go on to another and very distinct portion of the subject, and I must try and make this perfectly clear to you by a somewhat gross image. I ask you to imagine the particles of light being shot off, so to say, from a luminous body: when those particles of light strike against a rough surface they are like elastic balls shot against a rough wall, such as this unpolished surface. Anybody who has ever played in a racket court will know that there is no dependence to be placed upon a ball that strikes a rough wall; it is reflected back, but it rebounds in any direction, and does not obey the law of the angle of incidence being equal to the angle of reflection; so that when the particles of light strike a rough wall, they are, as I have said, scattered in all directions. If on the contrary the elastic balls strike a perfectly polished or smooth wall, they are reflected according to the same law as light. Well now, suppose you drop a body into the water—a heavy body—a stone if you will; it strikes the water, and goes right down through it without turning to the right or to the left. But cast it from the hand thus [obliquely], every boy present ought to know that the earth pulls that stone downwards, and that its path is therefore not straight, but bent; (for even a cannon ball fired upwards is pulled down by the earth.) Well now, figure to yourselves—and this is merely an image that I put before you—figure to yourselves the little particles of light—(though I do not believe they are particles at all, and I shall tell you my reasons for not believing this, in a future lecture)—fix your thoughts upon the particles of light falling obliquely upon a surface of glass. When they fall obliquely upon the surface, the piece of glass appears to exert an attractive force and pull them down, just like the influence which is exerted by the earth when a stone is thrown from the hand. This is exactly the way the great Sir Isaac Newton imagined that when light struck obliquely, it was pulled down in its course. What is the consequence of that? the consequence is this—that if you take a ray of light and allow it to fall exactly plumb upon a surface of that kind, it goes straight through it without deviation either to the right or to the left; but if you allow it to fall obliquely, when it comes nearly to the surface—a little before it enters the medium, it begins to be bent—instantly bent, at the point where the ray enters the glass. Here, then, I have a piece of



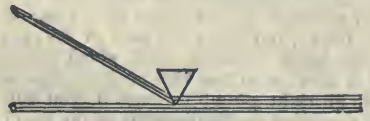
glass *(a b)* with two parallel sides; if, as I have said, the ray of light falls upon this perpendicularly, it goes straight through; but if it strikes obliquely *(c f)* the consequence is that when it comes very close to the surface of the medium, it is pulled down towards it and goes through in that direction *(f g)*; but when it wants to get out again, however, the attraction of the lower surface tends to pull it back again and it goes off in that direction *(g h)*: so that in a case of this kind, there are two bendings. There is a bending (at *f*) where the ray enters the glass from the air: it is there bent towards this perpendicular *(c d)* and when it quits the glass *(at g)* and gets into the air again, it is bent away from the perpendicular.

The original direction of the ray is thus perfectly restored, and it goes along exactly in the same direction as that in which it entered the medium; but instead of going straight along in a line with the original ray, which it would do if the glass were not there, it takes a course parallel to the first ray. In order that you may satisfy yourselves of this fact, take a piece of glass, the thicker the better, and make two dots with ink upon paper close together. Upon looking straight down at one of those dots through the glass, it does not alter in position at all with regard to the other; but when looked at obliquely, the moment you bring the glass between your eye and the dot, it immediately appears to have shifted its position. They are no longer in the same straight line.

I will now show you the *refraction* of light—for that is the term that is used. This bending of the ray downwards and upwards by the glass plate is called refraction. I will refract a beam of light by means of this piece of glass, which you see is very thick and has perfectly parallel surfaces. I have here the electric lamp; I will first get a beam of light quite horizontal and will interpose a slit in front of the lamp. Here you see is the exact position of the image of the slit marked on the screen by a pointer. I beg your attention to this beam, look along it, not only upon that image, but along the path of the rays of light. You see the beam tracking itself along the dust of the room. If I now interpose this piece of glass so that the beam shall go right plumb through it, you will find that there is no deviation in the direction of the beam upon the screen; but if I now twist the glass on one side so as to cause the beam to pass through it obliquely, and then you will find an immediate shifting of the image. There, you see, it shifts in one direction away from the pointer, and then as I twist the glass in the other direction, the beam shifts accordingly. This is caused by the bending of the rays on entering the glass and the restoration of them to a straight line parallel to their first position when they quit the glass. I will now take instead of this thick slab of

glass, a bar of glass such as I have here in my hand. It is simply a rod of glass with two plane polished surfaces. I will interpose this bar across the slit, and when I allow the light from the slit to pass through the bar at right angles, you will find there is no deviation,—the light will pass straight through from beginning to end; but when I hold it so that the beam shall fall on the glass obliquely, you then find that a portion of it is deflected, either to the right when I hold it in this way, or to the left when I hold it in the opposite direction. Here is our bar of glass; you see there is no change produced as long as I hold it fair, plumb, over the beam. But as I now twist it so as to cause the beam of light to strike it obliquely, you see the middle portion of the beam is deflected on one side; and if I allow it to pass through in the other direction you see it is deflected to the other side. This is caused by the refraction by the piece of glass. Instead of that, I can take an object such as I have here, an arrow, and project its image upon the screen. If I allow my bar or plate, of glass to go straight across the path of the light there is no deviation whatever. But if I cause it to twist aside you know very well what takes place: a portion of the arrow must be now twisted on one side owing to the refraction. If I bring it straight again so as to allow the light to pass through perpendicularly, there is no deviation no shifting at all of the arrow. This is an example of what we call *refraction*—the bending of the rays of light when they pass through a transparent substance obliquely.

We have hitherto been dealing with a body possessing parallel surfaces; and I hope you all remember what I have said regarding a body of this kind: when the light enters obliquely there is a bending of it *towards* the perpendicular, and when the light quits the medium it is bent *from* the perpendicular. But, instead of taking a piece of glass with two parallel surfaces, suppose I now take a piece of glass with unparallel surfaces. In the case of a piece of glass with two parallel surfaces, the bending produced upon entering the glass was immediately undone upon the ray quitting the glass. This is no longer the case when we take a piece of glass with the two surfaces not parallel. Here I have a little bit of glass shaped in the form of a wedge with the two surfaces not parallel; and you will find that the refraction is greater than in the case of the parallel surfaces; and here also I have two pieces of window glass put together so as to form a wedge-shaped figure, and if I have a little water poured into it, you see I have a wedge of water—what is called a prism, in fact, and now you will see what the effect of that wedge of water will be upon a beam of light issuing from our lamp. Unfortunately, this is not so nice and foggy a day as the day when we last met; and, therefore, the beam is not so clearly defined: still I would ask your attention to the beam, such as it is, marked along the dust of the room. It is perfectly clear to many of you, I dare say—a beautiful slice of light, so to say, falling thus upon the screen. Now I take the prism of water, and when I introduce it (*a*) you will see what takes place. If you watch the path



of the light (*b c*) you will see how the beam behaves—how it jerks up, down, up, down, up, down, up [inserting the wedge in and out of the path of light]—this being produced by the refraction or the bending of the light by the prism of water. The track of the beam through the room is, perhaps, more instructive than the image itself. You see how the beam jumps up the moment the wedge of water is interposed. Instead of a wedge of water I will now take a wedge of glass—one of these small prisms, and I will make a beautiful sun-like spot of light on the screen. Now, if you observe the track of the beam, you will find that it is jerked up also. There it goes—up, up, up,—and when I turn the wedge in this way the image goes down, down, down. If I turn it in this way, the image goes to the left, and in this other way to the right. Thus you see the ray is bent as it enters and quits this prism.

Now, having learnt generally the nature of what we always call *refraction*—if it were not called refraction I would not use such a difficult word, I want to use the most simple words possible for my present audience; I would not therefore use an unnecessarily learned word, but this word refraction must be remembered; it is the word always used to express this bending of the ray of light,—having learnt generally the nature of refraction when a ray of light falls upon glass out of air. I want you to remember that the same would occur if the ray of light fell into water out of air. Supposing this to be a vessel of water, and suppose an object, say a white pebble, to be at the bottom there, a ray of light quitting that pebble would go in a certain direction, but on quitting the water and getting into the air it does not go straightforward, but is bent away from the perpendicular. What is the consequence of this? Supposing the eye to be placed at a point above the water; it sees the light along this line; instead of seeing the pebble in its real place, it sees it higher up; in fact the whole bottom of the basin is apparently raised up by the water and is rendered thereby in appearance much shallower than it really is. I have often found this to my great discomfort when I was learning to swim. Many a time I have imagined that I should be only up to my breast, and have really found myself up to the chin. I will take a basin of this kind and put a penny-piece there at the bottom; if I place my eye here, this edge of the basin when it is empty cuts off the penny piece altogether from my eye. You know, in point of fact, that this quite intercepts the light coming from the penny-piece. Supposing this basin to be now filled with water; the bottom appears to be raised up, and in that position I can see the penny-piece very clearly, it being brought within the range of my eye. This is an experiment which I dare say some of you have made already, but which I want to make for the benefit of all. I dare say the smallest

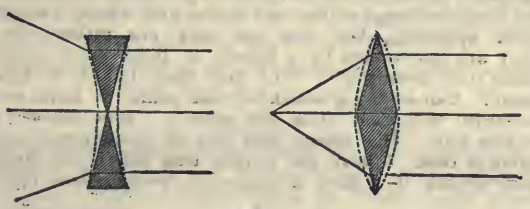
of my hearers will testify that he cannot see the penny-piece at the bottom of this basin. Evidently he would have to stand up to see it. Now I ask you to notice the effect produced by the water; it will be apparently to lift up the bottom of the vessel and bring it in view. [Water was then poured into the basin.] Here you see the bottom is raised up; and thus this action of the water, which has often disappointed me when a boy, is perfectly explained by the refraction of light. I will now take that penny-piece away, and I dip this rod straight down into the water, and there is no effect produced upon it; it does not appear to be bent at all. But look what takes place when I put it in obliquely. The bottom is raised up, and thus that end of the rod which dips into the water appears higher than it ought to be, and as a consequence the rod will actually appear bent up at the point where it enters the water.

During the last few years I have been very much in the habit of examining people, not boys, exactly, but young

men, and I have very often asked them to explain this effect. I have asked them to draw how the stick would be bent on being thrust obliquely into water; and I believe in nine cases out of ten they have drawn first of all a line through the water and then they have drawn the stick going off in the opposite direction to what actually takes place. They have mistaken the bending of the rays for the bending of the stick. They have drawn the way the ray of light issuing from the vessel is bent; the ray of light is so bent that the bottom seems to be lifted up, and because the bottom is lifted up, the stick is bent altogether in the opposite direction to the ray; so that you see they were quite wrong in ascribing that to the stick which belonged to the ray. This is an experiment well worthy of your attention, and it may be made with instruction when you go home.

Well now, I have introduced to you these little bits of glass prisms as they are sometimes called—cut into the shape of wedges. I have here two of these wedges. If a beam of light falls upon one of these prisms, it strikes the surface a little obliquely and is bent towards the perpendicular on entering the surface and from the perpendicular on quitting it and goes on in an oblique direction.

I will now take both these wedges and hold them edge to edge. Now, a ray of light striking upon these will on



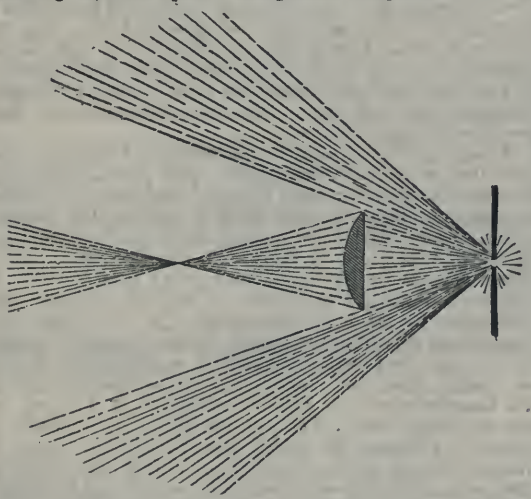
entering be bent towards the perpendicular, and on quitting it and going into the air again it will be again bent. So that you see all the light falling on the prisms when in this position is scattered outwards on quitting them. We now pass on in the easiest manner possible from these two surfaces with straight lines to surfaces with curved lines, and you see at once that that would make what we call a lens, or an eye glass. I do not know whether any boy here present requires an eye-glass; but boys are often short-sighted and they find articles of this kind necessary. I will explain the reason of that afterwards; but if you suppose that eye-glass to be sliced in two, and then look at the edge you will have a lens concave on both sides, and hence it is called a double concave lens. Suppose a parallel beam of light falling upon the concave lens, it is scattered in the way just shown. It is caused to diverge by passing through the lens. Hence these double concave lenses are double diverging lenses.

I pass on from this to a very interesting and important subject. It is a very difficult subject, but it is well worthy of all the care which you can bestow upon it. Let me reverse the position of my prisms. Suppose I put them with their bases touching (see Fig.); then if a ray of light falls upon them it will be bent on entering the prisms and on leaving, and the rays of light will be converged to a point instead of being scattered abroad as they were when we placed our prisms edge to edge. I then come to a figure similar to this, but bounded by a curved line in that way, and this is exactly the shape of a section of those spectacle glasses used by old people. We shall see the reason why by-and-by. Here is such a double convex lens; it is bulged out on both sides, and when we cause the rays of light to fall upon it, it will squeeze them together and bring them to a point behind the lens which is called the principal focus of the lens. I have here two such spectacles—a pair of boy's spectacles for short sight, and an old man's spectacles which might suit myself, for instance—for long sight. I will pass these in front of the lamp and allow the light to fall through both before falling on the screen, and I think you will find that one will squeeze the light together and produce a spot of light in the middle; whereas the other will scatter the light abroad and leave the lens in a state of shade, and cause a rim of light to fall around the circumference of the spectacle. When I put these before the lamp I will ask you which is convex and which is concave, and if you cannot answer me correctly, it must be because I have not made my subject sufficiently clear. I hold these glasses before the beam of light without actually knowing to which class each belongs; but now tell me, are they convex or concave? Why, they are convex, for concave lenses diverge the light, and here we have a squeezing together of it. You see there the difference between these two classes of lenses. I have shown you the boy's lens, and there as I have said we have the old man's lens. You see the squeezing of the light in the middle; and here on the other hand you see the divergence of the light—the scattering of the light forming a luminous rim round about the lens. This is concave, and the first one was of course convex.

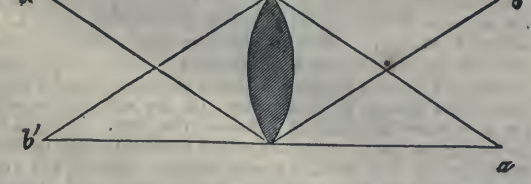
I have stated in the list of memoranda of to-day's Lecture that all bodies refract light—that gases, solids, and liquids refract light; but I have deferred showing you the refraction of light by gases until I reached the present point, for you will be able to understand it now better than if I had introduced it earlier in the Lecture. I have stated that the quivering observed over a heated surface is due to the refraction of the air. I will try and show you this refraction and this quivering. [A gas-burner was lighted and placed in the path of the rays from the electric lamp.] At the present time there is a column of heated air coming from that burner, and I will cause the light of the lamp to cross this column. Nobody at present can see anything there; I cannot see it, but still there is a column of heated air which has a different degree of refrangibility from the air round about, it being rarified by the heat; and although you cannot see the air, it will act as if I had interposed a series of glass lenses. [The electric light was then turned on.] There you see the quivering of the shadow on the screen. How beautiful that is! It is something analogous to the quivering that you see when you look over a heated surface. These little jets of gas are like so many concave lenses, and the spaces between them act like convex lenses, and so you have the light squeezed in the one lens and thrown out in the others, and thus you have this beautiful effect produced. It is not, however, necessary to use a jet of gas to produce this effect. If I heat the air simply with a hot iron, I produce the same thing. [A red-hot poker was introduced into the path of the rays.] There you have it by warming the air; the current of heated air, being lighter, goes up, and thus you see we have this beautiful effect produced. But it is not actually necessary to make use of heat at all. I have here two vessels; one contains hydrogen gas, and the other another gas which will burn like coal gas—olefiant gas. I will cause a quantity of hydrogen to pass up into this bottle in front of the screen. Nobody here present can see it; you cannot see a trace of the hydrogen, but being very light it will ascend and fill this bottle, and you will see it by-and-bye escaping from the bottom. It is perfectly transparent, but you see there the effect produced by the escaping hydrogen. This is due to the fact that the hydrogen has a different refracting power from the air. It is capable of bending the rays of light in a different degree. Here is another gas—olefiant gas—which is capable of bending the rays of light more than the air. I will let those two jets of gas issue side by side, and if you look at their shadows on the screen you will see a marked distinction between the two. The jet of hydrogen gas as it escapes is rather shaded in the centre, while that of the olefiant gas is bright. Now, I ask you all to attend to this remark,—that if we could trace it exactly, you would find that each line of light produced

by the hydrogen corresponds to a line of shadow or darkness produced by the olefiant gas, the one having more refracting power than the air, and the other being less refracting than the air. Those ar· points that I trust you will follow out in your own thoughts afterwards. We cannot well do it now.

I want now to prove to you that what I have said with regard to lenses is a matter of ocular demonstration, and I have here a large converging lens, called a plano convex lense; one side is bulged out and the other is perfectly flat. If we cause the rays of light to fall upon it, they are converged or squeezed together. I will try and show you this convergence of the rays from the electric light by allowing them to pass through this large lens. Now

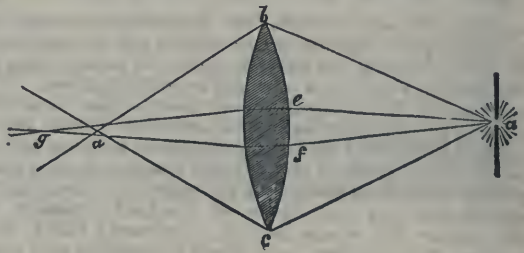


those who are looking in a certain direction,—and I am sorry to say this room is not so foggy as I should like it to be—will see a fine cone of rays issuing from that lamp and after passing through the lens coming to a point; and if I cause the lens to turn round, the cone will travel round. There is the fine convergent cone produced by this plano-convex lens. You see we get this splendid cone produced by the converging power of the lens, and those who are sharp-sighted enough will observe even in this atmosphere the light diverging from the coal points. Beyond the lens, outside the cone, there is a space of darkness, and the rays are actually squeezed away from the space of darkness so as to form this cone; and if I interpose a screen in the path of the beam, I get a bright disc of light, and at the focus I have actually a blurred image of these coal points from which the light emanates. The blurring will be explained afterwards, but here you see an image of the coal points produced by the collection of all the rays by the lens. This lens, like a skilful architect, has built up these particles of light, like so many bricks, into an edifice of exactly the same kind as that from which they issue. I want now to show you some of the images produced by lenses of this kind. Supposing I have a convex lens, and I allow the beam of light to fall upon it



obliquely. On quitting the lens the light will be converged to a focus and come to a point. Now, suppose these beams are not parallel, but that they issue from a point like our coal points yonder; I will call that the point *a*; there would be an image formed of it at the point which I call *a'*. Supposing I take another point here which I call *b*, and allow a conical beam of light to fall from it on the lens. On quitting the lens it will be refracted, converged, and brought to a point at *b'*, and there we should have an image of the point *b*. Supposing there were a man at *a b*, and that mark *a* represented his feet and *b* his head, and the intermediate space the body, what would take place here with regard to the image? You see this point *b*—the head—would be downwards in the image, and the feet *a* up, so that the image produced by this double convex lens would be that of the object inverted. And thus if I take any object—supposing a candle—and throw upon it a beam of light from the lamp, and place it behind the lens, you have the candle thrown on the screen inverted, just as I said the image would be inverted.

Now, I wish to show you the image of these coal points with which we have been hitherto operating, and for that purpose I will interpose a lens in front of this lamp, and I will throw my beam through that lens, and we shall see immediately whether we cannot by means of the lens make an image of the coal points upon the screen. Here we have the image of the coal points with which we have been operating, but you see they are rather dim; they are surrounded by a blotch of light which rather dims their definition. Why does that blotch of light exist? Can we bring out those coal points in strict and beautiful definition upon the screen? We can do it in this way. The reason that you see that blurred light there is this. I have the convex lens before the lamp and behind it I have a source of very intense light



When the rays of light fall from that point (*a*) on to the edge of that lens (*b c*), they are bent, and pass on in that direction (*b d*, *c d*). Now, let me take a portion of the centre of the lens, and let the beam fall upon the part *e f*. When it enters it will be refracted, and when it quits the lens it will also be refracted, but it will not be refracted so much as the rays that have entered at the edge are refracted, so that the focus of the edge and the focus of the centre rays are not the same. The focus of the centre rays (at *g*) not being the same, you get a quantity of light round it which blurs its definition, but if you cut off the circumferential rays, and use merely the rays near the centre, you get a beautiful and clear image of the coal-points. I will endeavour to do that in this way. You see I have a piece of metal with a hole in it of not more than about an inch and a-quarter in diameter, and if I interpose this so as to allow the rays from the middle portion of the lens only to pass through, then we shall obtain a clearer image of the coal-points than we have had hitherto. Now, I ask you to observe what a beautiful image we have obtained of these coal-points, without any blur or indistinctness. This image is produced by these central rays, and here the inversion I have already spoken of takes place. This point that you see move up and down is really the

topmost point of the two, though it is inverted on the screen, and when it appears to be moving up, as it is now, it is really going down. Notice what takes place when we bring these points near together. See how the one appears to devour the other,—to eat it up,—the one consuming the other. You find the substance of the one is becoming heaped up on the other, and so in one we have a point, and in the other a crater, or hollow form.

Allow me now, in conclusion, to show that we can make with these lenses the same experiments that we made with a concave mirror. I have here a double convex lens, and we will place our lamp so as to illuminate these medals, and I think I can show you some beautiful images upon the screen by means of the lens—as beautiful, at least, as those that we obtained by means of the concave mirror. Now, for that purpose we must have our lamp pointed to you, in order to illuminate the objects; but, boys, do not fear a gush of light. I will take first of all this copper medal, turn it upside down, place the lens in front of it, and try to throw its reflection upon the screen. Having shown you the copper medal, here you have an inverted image of the silver medal, brighter but still not more beautiful than in the other case.

In our next Lecture we shall deal with that most wonderful of all optical instruments—that most wonderful collection of lenses in Nature—the human eye; and I trust we shall not part before we have made ourselves perfectly well acquainted with its construction.

NOTICES OF PATENTS.

796. *Artificial Substances to be used as a Covering for Stone, Bricks, Wood, &c.* J. Briggs, Bridge Street, Blackfriars, London. Dated March 30, 1861. (Not proceeded with.)

For these improvements the inventor uses coal-tar-pitch, lime, and gravel, the latter of such size as will pass through the meshes of a one inch seive. The relative proportions in which these ingredients are mixed may be varied according to the nature of the application: for general purposes he takes 10 parts of pitch, 1 of lime, and 50 parts of gravel. The pitch is first melted in an open cauldron, the lime added, and lastly the gravel, which must previously have been heated to about the same temperature as the melted pitch. The materials are well incorporated and pressed into molds to form flags or blocks. In proposing to use the same as an upper coating for bricks or stone, the inventor recommends the employment of sharp river sand or grit in the place of gravel.

Mixtures of coal-tar-pitch and gravel or broken stone have frequently been employed for the purposes herein specified, and are commonly laid down as cheap substitutes for asphalte.

797. *A New Method of Colouring, as a Substitute for Saffron, in the Manufacture of Cheese, &c.* G. Russe, Genoa. Dated April 1, 1861. (Not proceeded with.)

This invention consists in the substitution of curcuma root (turmeric) for saffron in all the various purposes to which the latter is usually applied, especially in the tinting of cheese, pastes, vermicelli, etc.

The employment of turmeric for the colouring of pastry and articles of confectionery was pretty generally understood and practised in England long previously to the date of this specification, April 1.

842. *Shoes for Horses.* W. Edwards, Wolverhampton. Dated April 5, 1861. (Not proceeded with.)

In making the aforesaid shoes the inventor employs a compound metal obtained by rolling and welding together iron and steel, so that the wearing parts or surfaces shall consist of hard steel, and the remainder of iron. Very light and durable horse-shoes are said to be thus obtained.

853. *Preparing, Applying, and Adapting certain Vegetable Productions, to further the purposes of Manufacture.* T. G. Ghislin, Hatton Garden, London. Date April 6, 1861. (Not proceeded with.)

This invention relates to the manipulating and manufacturing, by chemical and other processes, certain vegetable productions found in, and indigenous to, South Africa. The first article, a marine plant or fungus, known to botanists under the name of Eiklonia buccinalis, the inventor applies to veneering and inlaying purposes, or, after being stamped and embossed, as a substitute for leather, shagreen, etc., or, instead of horn, for knife-handles. The second article, known as the Juncus trista, is suitable for basket-work, instead of reeds or cane. Other vegetable productions, Juncus serratus (an aquatic), and certain members of the armyllideæ, are applied by the inventor, in the form of fibres (alone, or mixed with other materials), for the purpose of coating submarine cables, and for insulating electric wires in general.

877. *Improvements in the Manufacture of Artificial Stone and Cement, or Plaster, and in Treating Timber, for the purpose of Preserving the same.* F. Ransome, Ipswich. Dated April 9, 1861.

For the purpose of manufacturing artificial stone, the patentee mixes powdered chalk with the solution of an alkaline silicate, and moulds the compound into blocks or shapes. Afterwards, when the blocks are sufficiently dry, he washes over the surface with a solution of chloride of calcium, or other soluble salt of an alkaline earth, or with a solution of chloride of aluminium or iron, for the purpose of converting the alkaline silicate into an insoluble silicate of lime, alumina, or iron. In the treatment of wood for the purpose of preserving the same, he forces into its pores a solution of silicate of soda, and afterwards applies the chloride of calcium, or other earthy salt, as before.

The material produced, according to the terms of this specification, was submitted by Mr. Ransome to the Parliamentary Committee on the Decay of Stone, in May last, and the character of the silicated blocks has already been fully described in our notices of the Committee's Report.

892. *Drying and Preparing Grain, Seeds, or Berries for Food.* T. Don, Poultry. London; T. Smith, Tenter Lane, Leeds; and L. Horsfield, Bank Foundry, Leeds. Dated April 11, 1861.

The patentees, in carrying out these improvements, avail themselves of the use of heated air, steam, super-heated or otherwise, which they cause to pass through suitable pipes, arranged in a horizontal position, and enclosed within an upright case. The grain, seeds or berries to be desiccated are allowed to descend among the heating surfaces, and the moisture so evaporated drawn off by means of an exhausting fan, or other equivalent mechanical contrivance.

Grants of Provisional Protection for Six Months.

2495. William Clark, Chancery Lane, London, "An improved gas regulator and purifier."—A communication from Jean Baptiste Frangeis Maliquet-Allegret and Lucien Victor Teste, Rue Ferrandière, Lyons, France.—Petition recorded October 5, 1861.

2976. John Henry Johnson, Lincoln's Inn Fields, London, "A new or improved apparatus for supporting the womb in cases of prolapsus uteri."—A communication from Doctor Otto Langgaard, Berlin."—Petition recorded November 26, 1861.

2999. Charles Stevens, Charing Cross, London, "Im-

provements in furnaces for working iron ore."—A communication from Joseph Bonne, Paris.

3054. Charles Davis, Bancroft Place, Mile End, Middlesex, "An improved composition for coating metal and wood to preserve them from decay, applicable as a substitute for copper and other sheathing or other compositions now in use for coating ships' bottoms to protect them from the injurious action of water." — Petition recorded December 5, 1861.

3066. John James Russell and Burdett Lambton Brown, Crown Tube Works, Wednesbury, Staffordshire, "Improvements in apparatus used in the manufacture of paper tubes."—Petitions recorded December 6, 1861.

3082. John Foldred, Brighton, Sussex, "Improvements in treating linseed oil."

3100. John Washington Agnew, Windsor Chambers, Great St. Helen's, London, "A new and improved electro-voltaic pocket battery."—A communication from Henry Palmer, London, Canada West. — Petitions recorded December 10, 1861.

3108. William Henry Tooth, Rhodeswell Road, London, and William Yates, jun., Parliament Street, Westminster, "Improvements in the manufacture of iron and steel, and in the machinery, apparatus, or furnaces used therein, and for the production of gas to be employed in such manufacture."

CORRESPONDENCE.

On the Specific Gravity of Liquid and Solid Substances.

To the Editor of the Chemical News.

Sir,—In the Chemical News of December 28, 1861, is an article by O. Richter, Ph.D., on the Specific Gravity of Substances in the Liquid and Solid State, which brings forward a want that I for one have long felt, viz., full and trustworthy tables of specific gravities; and I am persuaded that there are many who will be truly thankful if the Chemical News or Dr. Richter will supply this desideratum. Mr. Richter makes some valuable remarks upon the philosophy of the subject, which are certainly well worthy of consideration. He reminds us also of the deeply interesting experiments of Playfair, now many years ago, which it is certainly the unanimous desire of those who devote themselves to the philosophy of Chemistry, that the gifted Professor of Chemistry in the University of Edinburgh would resume. These experiments, Dr. Richter says, "appear to conceal within them a profound philosophical truth, viz., that the eighth part of the volume of ice is really the standard whereby the volume equivalents of the various chemical elements will one day be correctly measured and calculated." Generalising this statement, I am fully persuaded, after much inquiry, that it will be found to be perfectly true. At all events the theory of specific gravities having been triumphantly established in reference to one class of substances (aëriforms), that theory surely ought not to be abandoned in the further pursuit of the subject, until it has been shown to be inadequate. It has been established that the specific gravities of all aëriforms may be derived from their atomic weights, on the theory that their atomic or molecular volumes are either the same or in simple ratio with those of the ambient medium (N = 14, O = 16), and therefore with one another, to maintain which law and harmony we know that they are capable of wonderful compression or diminution of some kind. Thus the aëriform molecules by which the vegetable kingdom is rendered fragrant, whose formula is never simpler than $C_{20}H_{16}$ have each a volume no larger than that of a particle of atmospherical nitrogen and oxygen taken as one, and so in other cases, according to a theory which is well entitled to all regard.

Now, why not attempt to carry out this theory in reference to dense molecules; the molecules of bodies in the liquid and solid state? Of course we cannot look to the atmosphere for an unit volume. But we are not without a great cosmical element, and as we might say an ambient medium, here as well as in reference to aëriforms. The sea viewed as resting in its own basin, and in its all-pervading arms and products, lakes, rivers, springs, and percolating waters, and with the abundant evidence that there is of its prevalence during all geological epochs, suggests itself in reference to dense bodies as holding the same relation to their molecules that the atmosphere does in reference to aëriform molecules. And hence the question—Ought we not, in extending the theory of specific gravities when we arrive at dense substances, to view them in reference to an unit volume of water, as we view aëriforms in reference to an unit volume of air? May we not legitimately conjecture and enquire, unless the contrary appear, that just as there seems to be a law in virtue of which the particles of bodies, when forming into aëriform molecules, shall conform in volume or be in harmony as to volume with that of the particles of the ambient atmosphere, so the same law may require with regard to dense molecules, or the molecules of liquid and solid bodies, that the elementary particles which go to constitute them shall group together in such numbers as shall give molecules conformable in volume as nearly as possible to the all-pervading dense element of the terraqueous globe, viz., a particle of water or of ice? Or more generally, are we not to expect, in the absence of evidence to the contrary and of adequate enquiry, that just as we find that Nature aims at a standard volume with respect to aëriform molecules of which an element of the atmosphere may be taken as the type, so we may enquire with advantage, whether she does not the same with respect to liquid and solid molecules of which a molecule of water or of ice may be taken as the type? I have pursued the enquiry on this hypothesis to a considerable extent, and hence obtained results which are equally unexpected and beautiful, and which bring the inorganic world, as it is commonly called, into exquisite harmony with the organic, which latter is now best known as to the molecular constitution of its elements. These results I shall be very happy to communicate to you, duly Macadamised for a weekly periodical, should your insertion of this lead me to infer that they would be acceptable.

I am, &c. T. M. S.

Spectrum Analysis.

To the Editor of the Chemical News.

Sir,—As your valuable Journal is generally recognised as containing the most complete account of the progress of "Spectrum Analysis," I venture to trouble you with the following.

One of the most beautiful and instructive experiments that can be performed with the "Spectroscope" is the exhibition of the two spectra in the field of view at the same time, for the purpose of comparing the various lines with each other. This is generally done by refraction, while I have advantageously employed reflection by a prism. The following simple and inexpensive method will, however, be found very efficient:—

Take a piece of white card or polished tin about four inches square and fix it on a support in a vertical position in front of the instrument, first cutting it in such a manner that the edge may be in close contact with the adjusting slit and exactly bisecting it.

If the two burners be now placed one on each side of the tin or card, and as near to it as possible, the two spectra will be at once visible.

As I know many of your readers have a "Spectroscope," the hint may prove serviceable to them, but this plan cannot of course be adopted with any other form of Spectrum Apparatus, as the slit in the whole of them is vertical, which precludes the possibility of employing any such arrangement.

The copper spectrum being one of great beauty, may be easily obtained in a very fine manner by dipping a piece of copper wire about a quarter of an inch in diameter in hydrochloric acid and then bringing it to a dull red heat in a Bunsen's burner.—I am, &c. John Browning.

111, Minories, E. C., January 13.

Preparation of Pure Hydrochloric Acid.

To the Editor of the Chemical News.

Sir,—It may be useful to some of your readers to know that pure hydrochloric acid for toxicological purposes may be prepared from the distillation of chloride of potassium or sodium and oxalic acid in equivalent proportions. I have found the acid so produced to be pure. The residue in the retort dissolves in water, furnishing the salt. $NaO_2 (C_2 O_3) + 2$ Aq.

Should you think this worth notice, by placing it in your Journal you will oblige me.—I am, &c.

Thomas Bloxam, F.C.S.,
Lecturer on Chemistry, Grosvenor-place School of Medicine.

28, Duke Street, Grosvenor Square, W.

Chemical Nomenclature.

To the Editor of the Chemical News.

Sir,—I have already given a list of numerals sufficiently extensive to provide names for the great bulk of organic bodies, but they may also be formed even for substances containing several hundred equivalents of carbon, hydrogen, &c.

For this purpose the names are constructed in two parts, separated by a hyphen, the letters and syllables composing the first part being understood as representing ten times the value of those composing the second part. Thus, b, d, f, &c., if commencing the second part of a name, signify C, C_2, and C_3 respectively, whereas in the first part they severally represent C_{10}, C_{20}, and C_{30}.

The same numerals that are used for oxygen may also be employed to denote the amount of nitrogen when present in large quantity, the second syllable of any name giving the oxygen and the third syllable the nitrogen.

For example, let us take one of the formulæ[1] which have been assigned to the albumen of blood—viz., $C_{108} H_{169} O_{34} N_{27} S$, all of which may be remembered by the name pedfa-mangates albumen, or pedfa-mangatesone. In the first part of this name p signifies C_{100}, ed H_{160}, and fa O_{30}, and in the second part m is C, an H_9, ga O_4, te N_{27}, and s S, making together $C_{108} H_{169} O_{34} N_{27} S$.

It may be said that this name does not indicate the amount of phosphates in albumen, but neither does the formula; and surely it cannot be expected that a name shall be both short and at the same time actually give more information than the formula from which it is derived.

We thus see that with a list of numerals only half as extensive as that contained in my previous paper, it is possible to construct a name expressing the composition assigned to albumen itself, and one which, though inferior in point of sound, is not longer than those usually given to the simplest of inorganic substances.

Perhaps when the formulæ of these highly complex bodies have been thoroughly investigated, they may be resolved into something simpler, in which case the corresponding names will be improved in sound, and rendered more in accordance with those now in use.

I will only add that this system of nomenclature may be applied to other sciences, and that the same names which represent the quantities of carbon, hydrogen, &c., in organic substances may also serve to denote the species, genera, &c., to which particular plants or animals belong.

With many apologies for again trespassing on your valuable space, I am, &c.

John A. R. Newlands, F.C.S.

[1] Gregory's Handbook, fourth edition, p. 496, using Gerhardt's equivalents.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Nitrogen in Meteoric Iron.—The constant presence of nitrogen in ordinary iron led M. Boussingault to look for it in meteoric iron. The specimen he examined (*Annales de Chimie et de Physique*, T. lxiii., p. 336), fell at Lénarto, in Hungary, and besides the iron and nitrogen, contained the mixture of metals usually found in meteoric stones. M. Boussingault made three very careful analyses, the results of which gave 0·000102 of a gramme of nitrogen in each gramme of the aërolite.

Dianium or Niobium.—Two years ago (Chemical News, Vol. ii., p. 143), Von Kobell announced the discovery of a new metal to which he gave the name Dianium. He found the metal, or rather an acid oxide of it, in minerals up to that time supposed to be mainly composed of hyponiobic acid. MM. Deville and Damour have examined some of the same minerals (*Comptes-Rendus*, T. liii. p. 1044) and have come to the conclusion that what Von Kobell called dianic acid is only a modification of one of the acids of niobium. This opinion is shared by Hermann. Von Kobell replies, but we fear he must give up dianium.

Simultaneous Action of Air and Ammonia on Copper.—Peligot is well known by his earlier experiments on the ammoniacal salts of copper. Anticipated in some of his results by Schoenbein, he has continued his experiments, and now (*Annales de Chimie et de Physique*, T. lxiii., p. 343), gives the latest conclusions he has arrived at. He distributes finely-divided copper (obtained by reducing a salt by iron or zinc) about the sides of a large flask, into which he poured a small quantity of very strong ammonia. The vessel soon became warm, and white vapours were seen, which Peligot found to be composed of nitrite of ammonia. On repeating this experiment several times, taking care to refill the flask with air, a blue liquid is obtained. (It is unnecessary to add more copper, as very little is acted on in one experiment; but the points of contact must be changed.) This blue liquid the author found to contain a double nitrite of copper and ammonia. Crystallized and dried in the air, it had the formula $NO_3, CuO\ NH_4O, HO$. When boiled, this salt became green, lost its ammonia and water, and there remained anhydrous nitrite of copper, NO_3CuO. The double salt, wrapped in paper, placed on an anvil and struck with a hammer, detonated.

When the solution of the double salt is added to water, a turquoise blue precipitate of hydrated oxide of copper is obtained, which enjoys the remarkable property of preserving its colour in the air. It slowly absorbs carbonic acid, and becomes carbonate of copper without changing colour. M. Peligot thinks the same oxide may be cheaply prepared and become an important article in industrial art. The ammoniacal solution of the double salt above mentioned, is said, by the author, to be the best agent for dissolving cellulose, for that substance is precipitated again without alteration on the addition of an acid.

Preparation of Chlorosulphide of Phosphorus.—M. Baudrimont observed that chlorosulphide of phosphorus was formed whenever the pentachloride came in contact with sulphur, whether free or in the state of sulphide; and after many attempts he succeeded in obtaining it easily and abundantly by the reaction of PCl_5 on SbS_3, conducted in the following way (*Comptes Rendus*, T. liii., p. 468):—In a very large flask he placed 15 grammes of well-dried phosphorus, and having removed the atmospheric air by a current of carbonic acid, he passed dry chlorine until the whole of the phosphorus was converted into perchloride. He then removed the flask into a large yard (to avoid the disagreeable effects of the chlorosulphide on the eyes and air passages) opened it, and dropped in by degrees 115 grammes of sulphide of antimony. The first portions took some time to act, but presently the flask heated, and

white vapours were disengaged, and the portion of the sulphide was then added which acted quicker; and so on until the whole of the sulphide had been used. It is necessary to shake the flask well often to break the crystalline crusts of the pentachloride which form on the sides of the flask and then fall on the sulphide. The neck of the flask should be surrounded with a wet cloth to absorb the vapours of PCl_3S_2 which tend to escape. As soon as a slight excess of sulphide of antimony is seen, the flask is connected, and the chlorosulphide carefully distilled between 125° and 135°. Any chlorides of antimony or arsenic which may distil can be removed by shaking the chlorosulphide with a dilute solution of sulphide of sodium, the two liquids being separated by means of a separating funnel. The chlorosulphide is then treated with chloride of calcium and rectified. All these processes must be conducted in well-closed vessels, in consequence of the irritating effects of the chlorosulphide, which should afterwards be kept in a well-stoppered bottle under a bell glass with some quicklime. The author thinks that this body will be a powerful agent in modifying (probably sulphurizing) many organic bodies, and so give rise to a great number of new products.

II. ORGANIC CHEMISTRY.

Compound of Nicotine with Chlorobenzoyl.—Will obtained (*Annal. der Chem. und Pharm.* Bd. cxiii. s. 206), by adding a solution of nicotine in anhydrous ether to chlorobenzoyl, a precipitate of a glutinous liquid which, on standing for some hours under the layer of ether, deposited white prismatic crystals. Removed from the ether, and left in moist air, these crystals soon deliquesced and formed a yellow syrupy liquid. They had the composition $C_{24}H_{12}NO_2Cl$, and may be regarded either as *chloride of benzoyl nicotine*

$$\left.\begin{matrix}C_{10}H_8\\C_{14}H_5O_2\end{matrix}\right\}N, ClH,$$

or as *chloro-benzoyl-nicotyl-ammonium*,

$$\left.\begin{matrix}C_{10}H_7\\C_{14}H_5O_2\end{matrix}\right\}NCl.$$

Aribin.—Martius has discovered a new vegetable organic base, to which the name *Aribin* has been given by Wöhler (*Chem. Centralblatt*, December 4, 1861). It has the composition $C_{46}H_{20}N_4$, and is the first example of a non-oxygenated organic base of a stable crystalline form, the other non-oxygenated bases (*e.g.* Coneine nicotine) being fluid. The aribin was obtained from a tree growing in Brazil and known as the *Arariba rubra*. The author gives a long account of its preparation, reaction, and compound, to which we may return when space permits.

Leucic Acid and its Salts.—P. Waage has a paper on the above (*Annalen der Chem. und Pharm.* Bd. cxviii., s. 295), which, however, does not add much to the information we gain from Dr. Thudichum's paper just published in the *Quarterly Journal of the Chemical Society*, Vol. xvi., p. 307. When heated to 100° C., says Waage, leucic acid volatilizes. The sides of a watch glass heated on a water bath becomes covered with a net work of crystals of the sublimed acid. These crystals easily dissolve in water, always leaving some flocculi which are probably the anhydride of leucic acid. The syrupy mass left on the watch-glass only dissolves in water after long boiling, but is easily soluble in alcohol and ether. This mass Waage thinks is anhydrous leucic acid: a direct experiment showed that the acid lost water when heated. For a further account of the acid and its salts, we recommend our readers to go to Dr. Thudichum's paper.

III. TECHNICAL CHEMISTRY.

Resistance of Starch on Cotton Tissue to Solvents.—Chevreul (*Comptes Rendus*, T. liii. p. 984) boiled a cotton fabric impregnated with starch in distilled water for two hours, then soaked it in water and hydrochloric acid for eighteen hours, afterwards washed it with common water and then in distilled water; and after all this, the cotton retained enough starch to be coloured blue with iodine.

Coal Tar to Prevent the Potato Disease.—M. Lemaire (*Comptes Rendus*, T. liii. p. 1074) mixed two per cent. of coal tar with earth, scattered the mixture over his ground, dug it in eight inches deep, and then planted his potatoes. None of those protected by tar showed any sign of the disease, while more than half of some planted at a short distance on the same day, and left unprotected, were found to be diseased.

Use of Baryta Salts in Dyeing and Printing.—Frightened at the prospect of manufacturers being some day hard up for potash, M. Kuhlmann proposes (*Comptes-Rendus*, T. liii. p. 1047,) to economise its use immediately by substituting baryta salts for the corresponding potash salts employed in dyeing and printing, *e.g.*, the tartrate, chromate, and ferrocyanide. So far he seems to have tried only the tartrate of baryta, which appears to replace tartrate of potash successfully; but M. Kuhlmann is always a little mysterious.

MISCELLANEOUS.

Wood for Shipbuilding.—Professor Crace Calvert is now making an investigation for the Admiralty of different kinds of wood used in shipbuilding. It appears that the Professor is at no loss to explain why so many of the fleet of recently-built gunboats became rotton and others escaped untouched. He finds the goodness of teak to consist in the fact that it is highly charged with caoutchouc; and that, if the tannin be soaked out of a block of oak, it may then be interpenetrated by a solution of caoutchouc, and thereby rendered as lasting as teak. A few years ago an enterprising individual spent 30,000*l.*, in trying to introduce a new wood for shipbuilding purposes from South America, where it is known by the name of Santa Maria; but the dockyard authorities could not be persuaded to take it into use, and the imports were entirely neglected. This is one of the specimens investigated by the Manchester professor; and he finds it to be sound and resinous, and but little inferior to teak. Of the durability of teak there can be no question.

ANSWERS TO CORRESPONDENTS.

*** All *Editorial Communications* are to be addressed to the Editor; and *Advertisements* and *Business Communications* to the Publisher at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

*** In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

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Valuation of Indigo.—F. D.—This may be effected by stirring up a known weight (1 gramme) of the indigo with 8 or 10 parts of oil of vitriol. In about six hours add 50 grammes of hydrochloric acid and 1½ pint of water, and boil. Now add a test solution, made by dissolving 2½ grammes of dry chlorate of potash in 1 litre of water. Between each addition the liquid should be boiled. The amount of test solution delivered from a bourette required to decolourise the liquid will show the value of the indigo. This test is only relative.

A. T. Brown's communication has been auswered by the publisher.

Index of Refraction.—W. C.—According to Mr. Delffs, the Index of Refraction of compound ethers augments with their equivalents. Isomeric ethers possess the same index. Thus formic ether and acetato of methyl have each a refracting index of 1·357; acetate of ethyl, 1·3672; butyrate of ethyl, 1·3778; and valerate of ethyl and acetate of amyl, each, 1·3904.

THE CHEMICAL NEWS.

VOL. V. No. 112.—January 25, 1862.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Adulteration of Butter with Animal Fats, by EDWARD BALLARD, *M.D. Lond., M.R.C.P., Medical Officer of Health for Islington, and Parochial Analyst under the Adulteration of Food Act.*

[THIRD COMMUNICATION.]

I FIND from my friend Mr. Horsley's last letter that I must recur to the question of the solubility of butter (pure and adulterated) in ether. He very courteously gives me credit for "meaning well," but I claim something more than this. I claim to be *right*. Nor can I endorse his ultimatum. I think that there is yet more to be said upon the subject, and this I propose partly to say in the present paper. It seems that it must partake also somewhat of the form of a reply. And my reply is this:—

1. In Mr. Horsley's first communication he writes: "If a piece of this prepared butter be introduced into a wide-mouthed stoppered bottle and surrounded with ether at the temperature of 65° Faht., it ought to entirely dissolve, forming a clear, lemon-coloured liquid." He now says that he dissolves the butter in a phial "held in the warm hand for a minute or so." This is not dissolving it at 65°. That, when thus dissolved at a higher temperature, it should still be held in solution at 65° is altogether a different statement. That all his experiments were performed in a similar manner is a fact which, to my mind, vitiates them.

2. I must decline to bow to Mr. Horsley's criticism, on the ground that in his first paper he mentions no proportions of butter and ether, and, therefore, ought not to blame me for using those which are incorrect in my trial of his test. Doubtless, if an excess of ether be used, or the temperature be sufficiently raised, the solution might, as he describes, be perfect. But I also decline to submit to his criticism, because in the experiments he relates he uses the same "immoderate" quantities that he accuses me of experimenting on. If he reads my last paper, he will find experiments where the proportion of 10 grs. to ʒj. of ether were those employed; still with a residue.

3. I assert that these last described experiments of his are not fairly conducted, even allowing the question of temperature to stand aside. He did not use adulterated butter where the ingredients had been fairly mixed by melting together, but the butter and the adulterant separate from each other. This is no trifling error, especially when lard was used; inasmuch as the physical conditions of the latter causes it, when unmixed with butter, to resist the action of the ether. Again, the appearance of the deposit is no criterion of its absolute quantity; a loose deposit, though bulky, may weigh wonderfully little, and *vice versa*. I know by observation that this is so in the instance in hand.

And now, before passing to observations of a different character, one word in favour of Mr. Horsley. I think it probable that there *may* be butters which do dissolve in ether at 65°, in the proportion even of 20 grs. to ʒj., although I hold that he has not proved it, nor have I yet met with such a sample. And the reason of my opinion is, that I have found butters which I have every reason to believe pure (barring the salt and water which all shop butters contain) leaving different amounts of residue when acted upon in my cylinder. I append the result of experiment in three of these, 20 grs being used with ʒj. of ether and left in the water-bath at 65° for one hour:—

1. "Dorset" butter, sold at 1s. 3d. per lb., melted with boiling water in a beaker formed a layer nearly uniform, the cellulations being so fine as to require a lens to distinguish them, and breaking down when cold and dried into a very fine mealiness, tasting only of butter, residue 3·3 grs. of the form and appearance of that from pure butter.

2. "Fresh" butter, sold at 1s. 5d. per lb., professedly "unadulterated," by the agent of a farmer in the country, melted with boiling water, presented in the melted form, and when cold, the same physical characters as above; *i.e.*, those of pure butter, residue 1·8 grs. of same form and appearance.

Here there are two samples which leave residues, the one greater the other less than in the pure sample I described in my last paper. The third sample stands alone.

3. Butter *imperfectly churned*, very soft and creamy, obtained from a friend, who prepares it for his own table. When melted and cold, physical characters those of pure butter as described above, residue 5·7 grs. When dry, bulky, loose, and honey-combed; deeply fissured in several places, and not aggregated like other samples of pure butter into the typical form. The cow from whom this butter was prepared was stall-fed, and the butter was white. Here there are all three grades of residue. It is possible to imagine butter with even less residue, till it amounts to *nil*. But I say this has not yet been fairly shown.

And now for another branch of the subject. I think it is Dr. Hassall who has somewhere suggested that the consistence of butter at various temperatures may possibly furnish a test of their adulteration with foreign fats. It appeared to me that this suggestion was worth carrying out. But the consistence of a substance which may present every degree between complete fluidity and absolute hardness, passing through every stage of soft solidity, seemed at first so difficult to represent that for a long time I despaired of a method of testing it. And the difficulty was increased by the irregularity with which caloric diffused through a hard mass of butter or fat exposed to increased temperature. It also occurred to me that possibly the appearance of the butter, its clearness and limpidity at different temperatures, might tell us something; but here I found that the rapidity or slowness with which the heating was effected modified

the result, as did also every irregularity in the rate of warming by introducing a disturbance either from convection or gravitation. The only satisfactory mode of carrying on such an inquiry thus turned out to be by at once raising the substance to a high temperature and allowing it to cool slowly, gradually, and uniformly. But then, how to measure and represent the consistency? I experimented thus:—

My apparatus consisted of a test tube, length 4½ in.; diameter, ⅞ in.; an ordinary chemical thermometer, diameter of containing tube, $\frac{3}{10}$ in.; diameter of bulb, ⅕ in.; length of bulb, ⅗ in.; and a half-pint beaker, to serve as a water bath. I used throughout the same tube, thermometer, and beaker, so that the comparison of the experiments might be fair and incontestible. The weight of the tube was 175 grains, and the quantity of butter, &c., acted on was in each instance the same, viz., 155 grains, making a total weight of 330 grains. Having weighed the butter, &c., in the tube, I pressed the thermometer to the very bottom of its centre and supported the whole upright in the beaker, which I filled to the brim with boiling water. I noted the point to which the thermometer rose, and then allowed the whole to cool spontaneously, noting also the time it took to attain various temperatures; and, lastly, I noted the time and temperatures at which various changes in appearance of the liquid took place, and at which it had acquired sufficient solidity to enable me to raise the loaded tube by the thermometer, which now became fixed in the fat or butter, first an inch or so above the water in the beaker, and next completely out of it, so as to allow of its being oscillated without permitting the thermometer to slip. I experimented upon pure butter,—the sample I mentioned in my last paper,—upon beef and mutton dripping, upon lard (home made), and finally on the several adulterated butters. I will endeavour to compress as much as possible the results of my experiments.

First Experiment.—Pure Butter—Carefully dried, raised to

169°. Liquid, cloudy from minute points. Thermometer visible, but not the markings on it. After a few minutes the points became more distinct, and after

20 minutes and at 122° they had aggregated into an universally diffused fine flocculence, and the mercurial column had become visible. After

35 minutes, and at 108°, the first evidence of gravitation of the flocculi became apparent by the formation of a line of clear fluid at the top, which now gradually increased, until after

78 minutes, and at 82°, the clear layer, through which the numbers on the thermometer were quite legible, had attained the depth of half an inch (the entire of the butter occupying about an inch and three-quarters); the bulb had become now obscure by gravitation of the flocculi, but both this and the lower part of the stem were still visible through them. In

110 minutes and at 75° the clear layer had attained about one inch in depth, and the bulb, which at 76° was still obscurely visible, was now quite lost in the deposit. This total obscurity of the bulb was synchronous with a change in the clear layer (a change which occasioned the total obscurity by filling in the interstices of the deposited flocculi), viz. the appearance of minute points in the clear top layer. In

118 minutes and at 74° no further gravitation of flocculi had occurred, but the points in "clear" layer had become more distinct, and the deposit was to the eye denser and more opaque, obscuring totally all that part of the thermometer it surrounded. In

127 minutes and at 73° the readings through "clear" layer slightly obscured; and in

145 minutes and at 71° the numbers on thermometer in "clear" layer illegible, and scarcely even visible. A slight secondary gravitation apparent from the greater obscurity of the markings through the upper than through the lower part of this layer. The stem of thermometer now gradually became lost to view, the opacity proceeding from below upwards, so that after

165 minutes and at 69° none of the stem of the thermometer was visible, the upper part of the "clear" layer, however, remaining more translucent than the lower. The punctiform appearance of the opacity still remained.

178 minutes and at 68°. The loaded tube could be raised one inch out of the water, and then the thermometer began to slip. In

222 minutes and at 66° it could be raised completely out of the water, but, after a few seconds of supporting, it began to slip.

On repeating this experiment the same results were obtained, but the cooling was still further continued, viz. for 505 minutes and to a temperature of 60°, at which the thermometer still slid out. It was now left for the night, and in the morning, at a temperature of 55° the loaded tube could be supported out of the water and oscillated.

Second Experiment.—Beef Dripping—At

170° was liquid and almost clear, presenting a very trifling milkiness, the readings being clearly legible through it. By the lapse of

25 minutes and at 120° the milkiness had assumed the form of very fine points. After

60 minutes and at 90° these points had become more distinct, but not flocculent, and at the upper part the readings were slightly clearer than at the lower, showing a trifling gravitation. After

80 minutes and at 80° the opaque points were very distinct, and either larger or more numerous, for they were so diffused throughout the liquid as to render the readings everywhere more obscure, but still legible. After

85 minutes and at 79° the numbers on the thermometer were illegible; the bulb and lower part of the stem were slightly more obscure than the rest. In

88 minutes and at 78° the numbers and mercurial column were invisible. In

96 minutes and at 76° the thermometer was wholly invisible. In

107 minutes and at 74° the loaded tube could be raised a quarter of an inch out of the water. In

111 minutes and at 73° it could be raised nearly out of the water; and after

115 minutes and at 71° it could be raised and supported out of the water, and oscillated without slipping.

Third Experiment.—Mutton Dripping—At

177° was liquid and colourless, presenting a very slight milkiness, apparently due to very fine points; numbers on the thermometer were all very legible and distinct. After

64 minutes and at 91° points had gradually become larger and more distinct; the readings were equally legible, but rather less so at the lower than the upper part. In

71 minutes and at 88° points much larger, but no flocculence; numbers less legible. In

80 minutes and at 87° numbers illegible, but still visible. In

84 minutes and at 86° bulb and lower part of stem

lost to view, and within one minute the whole of the stem was invisible. At 85° the thermometer was not yet fixed. In

95 minutes and at 84° the loaded tube could be supported out of the water and oscillated without slipping.

Fourth Experiment.—Lard (home made)—At

168° liquid and as clear as water, and thus without any remarkable change it continued for about an hour. After

59 minutes and at 88° a diminution of clearness became evident. After

66 minutes and at 85° obscurity had increased, numbers everywhere still legible, but less distinctly at lower than at upper part, and bulb not quite so distinctly visible as the stem. After

68 minutes and at 84½° numbers everywhere illegible. After

71 minutes and at 84° numbers and mercurial column only obscurely visible. Opacity distinctly punctiform. In

75 minutes and at 83° stem of thermometer only obscurely visible.

And now occurred a remarkable phenomenon which did not occur with butter, or beef, or mutton dripping, and which was apparently due to rapid solidification. The temperature fell about a quarter of a degree, and then began to rise until it reached 83¾°. Thus, after

82 minutes and at 83¾° the temperature became stationary for a minute or two, and then began again to fall. During this time the stem of the thermometer was quite invisible from opacity and solidity of mass. It attained after

86 minutes 83° again. After

94 minutes and at 81° the loaded tube could be raised in the water, but the thermometer was not yet solidly fixed. After

104 minutes and at 79° it could be supported out of the water, and oscillated without slipping.

The differences in the phenomena of cooling of these four substances may be summed up as follows:—1. The formation of flocculi in the cooling liquid is a peculiarity distinctive of butter. In the sample described the first opacity observed was in points. In another sample of pure butter—that which gave 1·8 grains residue with ether—mentioned in an earlier part of this paper, the flocculi were visible from the first, viz. at 171°. 2. Butter at all temperatures above 55° is less solid and consistent than beef or mutton dripping or lard. It is still quite liquid when mutton fat and lard have solidified, and is only on the eve of solidifying when beef fat is solid. 3. The order of solidification of the four fats on cooling is mutton, lard, beef, butter. 4. Butter is quite solid only at a temperature of about 55° (in an experiment with the other sample of butter mentioned above it was solid at 59°). Perhaps it may be said that its solidifying point is at least from 12 to 16 degrees below that of beef dripping. 5. This difference is observable throughout the cooling. Thus complete opacity was observed in the mutton dripping at 84°, in lard at 83° or thereabouts, in beef dripping at 76°, and in butter at 69°. 6. There exist in butter two substances at least which are insoluble in the olein and other conjoined liquid elements of butter. One of these is insoluble at temperatures between 74° and the highest temperature I have applied, viz. 212°. It is this which forms the gravitating flocculi. The other is insoluble only below 74° or 75°. It is this which forms the points during solidification in the "clear" layer. 7. In mutton fat, beef fat, and lard there was no substance which corresponded with that forming the flocculent deposit in butter, but the solid material began to be deposited in points at a much higher temperature, viz. in mutton and beef dripping considerably above 90° and in lard about 88°. The difference is probably due less to the difference in the melting point of stearine and margarine than to the amount of the liquid elements in these several fats. 8. The time occupied in acquiring the same degree of solidity was for these four fats least in mutton fat and longest by far in butter, lard occupying a shorter time than beef fat. 9. Lard presents in the act of solidifying a remarkable rise in temperature—about 83°. I found a similar phenomenon occur with a sample of lard which was purchased.

Islington Parochial Laboratory, January 1, 1862.

On the Estimation of Sulphur in Iron and Copper Pyrites, by M. J. Pelouze.

Sulphuric Acid has of late years been almost exclusively manufactured of Sicilian sulphur. The quantities exported from this island are truly immense, for France alone imports not less than thirty millions of kilogrammes.

At the present time sulphur is being replaced by iron pyrites or by ferruginous pyrites, more or less rich in sulphide of copper. This latter kind of pyrites is chiefly obtained from the shores of Spain, whence it is exported to England. It serves at the same time for the manufacture of sulphuric acid and for the extraction of copper.

France possesses several deposits of pyrites. The factories of Paris, Lille, Chauny, Rouen, &c., are principally supplied from Chessy and Sain-Bel, near Lyons. Those of the south obtain their pyrites from the neighbourhood of Alais, and other factories derive it from Belgium and Rhenish Prussia.

It will readily be imagined that several sources are necessary for a substance the annual demand for which approaches 100,000 tons.

The composition of these pyrites being extremely variable, the mercantile transactions connected with them are necessarily based on the amount of sulphur they contain, and it is requisite to determine it frequently and with care. On the other hand, it is not requisite for the manufacturer to know the quantity of sulphur left in the residue after roasting the pyrites. He should endeavour to exhaust these residues as much as possible, for hitherto the roasted pyrites has not been usefully employed. It has been recently sought to be utilised for the manufacture of cast iron of an inferior quality, but appears to have been given up. This is easily explained when it is remembered that the unburnt sulphur which remains mixed with the oxide of iron amounts to 3, 4, and 6 per cent., and sometimes to a more considerable amount.

In the present state of things, analyses of metallic sulphides are in general performed with accuracy, but unfortunately with extreme slowness. They are treated with *aqua regia*, the solution diluted with water filtered, and the sulphuric acid precipitated by a baryta salt. From the weight of the sulphate of baryta the proportion of sulphur may be calculated. This method requires, like all the methods of analysis by the wet way, a certain skill in chemical manipulations.

I know that manufacturers of sulphuric acid are anxiously seeking for a simpler and more rapid process. That which I am about to propose cannot fail to come into use, for it is in principle nothing else but an alkali-

metrical assay—of all industrial processes the one without exception best known and practised.

That will be understood when it is remembered that the manufacture of salts of soda is so bound up with the manufacture of sulphuric acid that a soda furnace is never seen in a factory without at the same time the leaden chambers being met with.

My new process is based upon the property which chlorate of potash possesses in the presence of an alkaline carbonate of transforming into sulphuric acid the sulphur contained in metallic sulphides, especially those of iron and copper, the only ones employed in the manufacture of sulphuric acid. This re-action, if well managed, is complete; that is to say, the whole of the sulphur passes into the state of sulphuric acid. This unites with soda or potash, or with both these bases at once, which is immaterial when regarded from a purely analytical point of view.

It is necessary to employ more carbonate of soda than is pointed out by theory, so as to avoid losing sulphuric acid. This excess of carbonate of soda is easily estimated by the ordinary alkalimetrical means.

The neutralisation of the carbonate of soda is therefore performed twice, first by the sulphuric acid formed at the expense of the sulphur during the calcination of the above-named mixture, and secondly by dilute sulphuric acid of any known standard.

Normal sulphuric acid being met with in laboratories, I employ this in preference to any other acid solution. This is of such a strength that 10 grammes of pure and dry carbonate of soda are exactly neutralised by 92·4 cubic centimètres of normal acid. These numbers correspond to equal equivalents of carbonate of soda and of monohydrated sulphuric acid. A litre of normal acid contains 100 grammes of monohydrated acid, or 32·653 of sulphur.

Suppose, now, that in an analysis of pyrites I have employed five grammes of carbonate of soda. I know that it would have required 46·2 cubic centimètres of normal acid to neutralise it directly; but if after the combustion of one gramme of pyrites, for example, I only require 30·2 cubic centimètres of acid, that shows that there has been formed by the oxidation of the sulphur an amount of sulphuric acid precisely equal to that contained in 16 cubic centimètres of normal acid for 16 cubic centimètres and 30·2 cubic centimètres make together 46·2 cubic centimètres. There only remains, therefore, to calculate how much sulphur there is in 16 cubic centimètres of normal acid. I obtain this by the following proportion:—

1000 c. c. : 32·653 : : 16 c. c. : x = 0·522 of sulphur.

Thus 1 gramme of such a pyrites contains 0·522 of sulphur, or 52·2 per cent.

I now pass on to the description of my process. I will suppose that the analysis of an iron pyrites is to be performed.

I accurately mix, in a porcelain mortar, 1 gramme of porphyrised pyrites, 5 grammes of pure and dry carbonate of soda, 7 grammes of chlorate of potash, and 7 grammes of fused or decrepitated marine salt. I introduce this mixture into a platinum crucible, and gradually expose it for eight or ten minutes to a dull red heat; the marine salt is added to prevent too violent an action.

When the mixture is nearly cold I add warm distilled water to it, and remove the solution by means of a pipette, and filter it. I repeat the washing five or six times, and finally boil the residue with water in the crucible itself. I throw it on a filter and wash it again with boiling water.

A little experience soon enables one to effect completely, and without any loss, the thorough washing of the substance operated upon. The solution and the washing waters are lastly neutralised with the normal sulphuric acid, without any modification of the method and precautions recommended by Gay Lussac.

Supposing that it has been found necessary to employ 34 cubic centimètres of normal acid; subtract this from 46·2 cubic centimètres, and there will remain, therefore, 12·2 cubic centimètres, which represents the sulphuric acid formed by the pyrites. This number multiplied by 32·653, and divided by 100, gives the weight of the sulphur sought,—·0398, or 39·8 per cent.

A quartzose, barytic, or calcarious gangue does not in the least interfere with this process.

The residue, after washing, should dissolve in hydrochloric acid without deposition of sulphur. It is easy to ascertain this, for, in a badly-managed assay, the sulphur separates from the gangue in the form of light flocks, recognisable by the blue flame and by the odour of sulphurous acid which they give when burning. When this happens, which is very rare, and indicates generally a badly-incorporated mixture, the analysis should be recommenced.

I am satisfied (and this is an essential point) that there is no disengagement of sulphurous acid during the combustion of the pyrites by receiving the gas either in a warm solution of *aqua regia*, containing a small quantity of chloride of barium, or, which is still better, in a solution of permanganate of potash. I have detected neither the precipitate nor the decolouration, which are characteristic of sulphurous acid.

I have made some other experiments to ascertain the accuracy of my process; they are the following:—

I. Specimens of pyrites in cubes of the most perfect sharpness, for which I am indebted to the kindness of M. Combes, yielded me in six analyses quantities of sulphur comprised in each case between 53 and 54 per cent. The formula FeS_2 contains 53·3 per cent.

II. Specimens of natural and roasted pyrites from the manufactories of Channy have been analysed both in the laboratory of the factory and in my own, by the *aqua regia* and baryta method, and also by my new process.

These substances have furnished, by this double treatment, quantities of sulphur which in no case differed from each other more than 1½ per cent., and which generally corresponded.

III. The product of the calcination of the mixture above named, well extracted with water and saturated with hydrochloric acid, gave with baryta the same weight of sulphate of baryta as by the ordinary *aqua regia* process.

I have also found similar results with several specimens of copper pyrites.

I have not hitherto mentioned any but iron and copper pyrites. I will now explain, in a few words, the application of my process to roasted pyrites in which the makers of sulphuric acid have so much interest in knowing the amount of sulphur, and of which they are constantly obliged to analyse a great number of specimens.

In this case I dispense with, as useless, the employment of marine salt. I mix accurately five grammes of roasted pyrites, five grammes of pure and dry carbonate of soda, and five grammes of chlorate of potash.

I expose the mixture to a dull red heat in a platinum crucible. The oxidation of the sulphur takes place slowly and without any deflagration. The rest of the experiment does not differ from that pointed out for iron and copper pyrites. If it has required 40 cubic centimètres of acid to neutralise it, it shows that the 5 grammes of roasted pyrites contained 0·202 grammes of sulphur, or 0·0404 for 1 gramme, or 4·04 per cent.

When the roasting has been badly performed, it is not uncommon to find pieces of pyrites still containing from 12 to 15 per cent. of sulphur; in these cases the above-named proportions of carbonate of soda and chlorate of potash must be increased.

In conclusion, I must urge the necessity of washing with boiling water, which is a matter of no difficulty; washing in the cold is long, and generally insufficient. The reason is, undoubtedly, that with pyrites having a silicious gangue, there is formed a small quantity of alkaline silicate which only dissolves easily in warm water.

I will add, that any loss of carbonate of soda occasions an apparent increase of sulphur; this is evident, since the amount of sulphur is judged from the volume of normal acid employed to complete the saturation. The carbonate of soda *lost* will wrongly be supposed to have passed into the state of sulphate, and the calculation of the proportion of sulphur will be established on a false basis. It is, however, easy, with a little care, to avoid errors of this sort, and also of others which I will now mention.

I need scarcely say that the carbonate of soda should be perfectly pure and dry, and that it must be weighed with as much accuracy as the pyrites itself. This care is not necessary in the case of the chlorate of potash and chloride of sodium. The proportion of the latter salt may be varied according to the combustibility of the pyrites, and must be increased until the oxidation of the mixture takes place without deflagration. Finally, the most necessary precaution of all consists in very finely powdering the pyrites, and mixing the whole very intimately together.

To sum up, the new method of analysing the metallic sulphides consists in the combustion of the sulphur by means of chlorate of potash in the presence of carbonate of soda. The sulphur passes entirely to the state of sulphuric acid, which neutralises a portion of the alkaline carbonate. The excess of this salt is ascertained by the volume of normal sulphuric acid employed to complete the saturation; this volume is subtracted from that which would have been required, by five grammes of pure carbonate of soda to directly neutralise it, and the difference shows the amount of sulphuric acid produced by the pyrites. From the amount of sulphuric acid that of the sulphur may be obtained by calculation.

The process does not occupy more than thirty or forty minutes; the errors involved in it do not exceed 1 to 1½ per cent. of the weight of the sulphur to be determined.—*Ann. de Chim. et de Phys.*, Third Series, Vol. lxiii.

Royal Institution.—On Tuesday, January 21, in the afternoon, John Marshall, Esq., delivered a lecture on "The Physiology of the Senses." On Thursday, January 23, in the afternoon, Professor Tyndall delivered a lecture on "Heat." Last evening Professor Rolleston delivered a lecture on "The Affinities and Differences between the Brain of Man and the Brains of certain Animals." On Saturday (this day) January 25, the Rev. A. J. D'Orsey will deliver a lecture on "The English Language," at three o'clock.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Six Lectures on Light[1] *(adapted to a Juvenile Audiitory), by* John Tyndall, *Esq., F.R.S., Professor of Natural Philosophy in the Royal Institution.*

Lecture III. (Dec. 31, 1861.)

NOTES TO THE LECTURE:—

When a ray of light passes from a rarer to a denser medium it is bent *towards* the perpendicular—When it passes from a denser to a rarer medium it is bent *from* the perpendicular—But the density or rarity here meant is not that which is expressed by the weight of a body. A body may be *optically denser* than another, though it be the lighter of the two—Spirit of turpentine floats on water, and is therefore lighter, or, in ordinary language, less dense than water; but a ray of light, in passing from turpentine to water, is bent *from* the perpendicular, and in passing from water to turpentine it is bent *towards* the perpendicular—In optics the densest body is that which refracts most.

Conceive a ray of light passing from a denser medium to a rarer, striking the common surface of both so obliquely, that on quitting the denser medium it is refracted so as just to graze the surface—The angle between that ray and the perpendicular is called the *limiting angle*, and for this reason—Because no ray that strikes the surface at a larger angle than the limiting angle can get out of the denser medium. All such rays as striking the surface are *totally reflected*, according to the law mentioned in the notes of Lecture I.—The limiting angle then marks the limits of possible transmission from a denser to a rarer medium. This is the only case in which the reflection of light is *total*—A liquid vein may be filled with light which cannot escape from the vein in consequence of total reflection—Mirage is produced by the total reflection of rays passing obliquely into the rare air close to the hot surface of the earth. Trees and houses may be seen thus reflected from the air as if from water.

The human eye is composed of three principal optical parts; the aqueous humour, the crystalline lens, and the vitreous humour—Behind the vitreous humour is the retina, which forms a screen to receive the images of external objects produced by the eye; these images are always *inverted*—For distinct vision it is necessary that the rays from every point of an object should come to a focus *upon* the retina—Some eyes refract too much and bring the rays to a focus too soon; to remedy this defect a *divergent* lens is placed before the eye; this is *short* sight—Some eyes do not refract enough, and to help them we place a *convergent* lens before the eye; this is *long* sight—Divergence is promoted by bringing the object close to the eye; convergence is assisted by holding the object far from the eye; hence the terms "short sight" and "long sight."

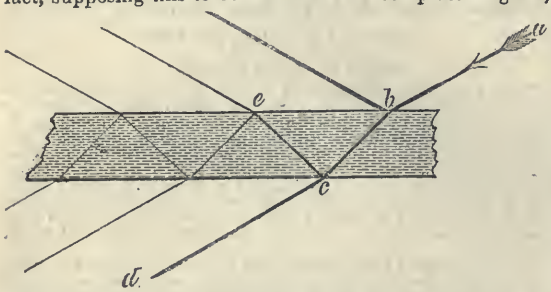
The action of light upon the eye does not subside the moment the light ceases; the impression endures in some cases nearly a quarter of a second after the light has ceased to shine—A succession of sparks, therefore, which follow each other at intervals of a quarter of a second, would appear as a continuous light; in fact, each impression would arrive before the preceding one had disappeared—To the eye a luminous object appears larger than it really is, and the more intense its light, the larger does the object appear. This effect is called *irradiation*—The full moon appears larger when looked at with the naked eye than when looked at through a dark glass. The bright new moon appears to belong to a larger sphere than the dusky globe which it partially encircles.

The white light of the sun is made up of an infinite number of rays of different refrangibilities—Each particular refrangibility corresponds to a particular colour; hence the number of colours involved in solar light is infinite—But for convenience sake we divide these colours into seven, which are called primary colours. These are red, orange, yellow, green, blue, indigo, violet—Of these colours the red is the least refrangible, and the violet the most refrangible; the other colours being intermediate between these two—The solar beam is resolved into these colours by passing it through a prism; the coloured image thus formed is called the *solar spectrum*—The colours of the spectrum, when suitably blended, produce white light.

I have now to say two or three words in completion of the memoranda of the last lecture. I have said there that "If two bodies refract a ray of light equally, one of them cannot be seen within the other." Now let me say one or two words upon the refraction and reflection of light in completion of what I have already mentioned. I have here a large piece of glass with parallel polished surfaces; if a beam of light falls upon that glass you know that a portion of it is reflected, making the angle of incidence equal to the angle of reflection, a portion, however, goes through, and may be found on the other side after having passed through the glass. After it has passed the first sur-

[1] Reported verbatim by special permission.

face of the glass and arrived at the lower surface, it does not all escape through to the air, some is reflected back again, although the greater portion is refracted through the surface below. Now, wherever you have refraction you always have reflection. There is, therefore, reflection at the bottom surface as well as the top, and thus you have two beams reflected instead of one. In fact, supposing this to be the outline of our plate of glass,



and supposing a ray of light (*a b*) to fall upon it thus, it is bent as you know towards the perpendicular, and on leaving the glass it is bent again from the perpendicular thus (*b c*, *c d*), but here (at *b*) there is a portion of light reflected, and here (at *c*) there is also light reflected. Another portion is reflected and refracted here also (at *e*), and thus you get a series of reflections from the two internal surfaces of this piece of glass. When the piece of glass is thick you see the difference between those rays more strikingly than when it is thin. Take a looking-glass, and look very obliquely in it at a white object, you do not see one image merely. If you place a candle near the surface of that looking-glass and look at it in the glass you see a series of images. First of all, you have a tolerably bright image, then a very bright image, then the rest get dimmer and dimmer till they actually become too dim to be seen. The first image you see, which is rather bright, is the one reflected from the first surface of the glass, the second, which is extremely bright, is from the silvered surface behind, and then you get a series of reflections from side to side. I have here such a piece of glass, and will make the experiment with the help of my lamp, and will show you these images obtained at the forward and the backward surface of the looking-glass. If I cause a beam of light to strike obliquely upon this glass you see first of all an image reflected from the anterior surface, and then you see further on another very bright one reflected from the silver surface, and next you have a series of images becoming gradually fainter and fainter until they are invisible.

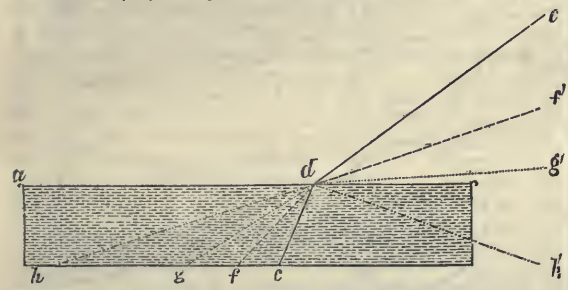
Now, I want to turn for a moment to another matter of some importance. I have said, that wherever you have refraction you have reflection, and if you have no refraction you have no reflection. Take a liquid and a solid; no matter how different the solid may be in substance from the liquid, no matter how much heavier, if it only bends a ray of light to the same degree as the liquid, it acts just the same as the liquid itself, and becomes invisible when it is plunged into it, owing to the absence of refraction and reflection at the two bounding surfaces. I have said in the memoranda that if you plunge the eyeball of an ox into water, it vanishes; it appears like the water, although it is a totally different substance.

When light passes from one medium to another of a different refrangibility, there is always reflection. And thus, if you mix two transparent bodies together, having different powers of refraction, the reflection may occur so often as to render this mixture perfectly opaque. Thus foam—which is only water—is as white as snow; and if I take snow itself, its particles are perfectly transparent, but it is because the particles of ice have air mixed up with them,—because when the light passes from every particle of ice to the particle of air, there is a little reflection, that the snow becomes an opaque body; and if you observe snow after it has fallen, upon the mountains of Switzerland, and has had the air squeezed out of it by the enormous pressure of the superincumbent mass, you will find it is actually converted from white snow into beautiful pure, transparent ice. To make an experiment to illustrate this, I will take first a piece of bibulous paper. This paper was once a pulp or jelly, as you know, and was partially transparent, because the fibres of the paper were then mixed up with water, and in passing from the fibre to the water there was not much light lost. These fibres are little transparent threads, and in the case of this paper they are separated from each other by little interstices filled with air; in passing from one fibre to the air, and from the air to the next fibre, there is reflection of light; and this occurs so often that the paper is converted into an opaque substance. If, instead of having air between the fibres of the paper, I throw between them a liquid which has almost the same power of refraction as the fibres themselves, then I destroy this reflection, in virtue of which it is rendered white, and I think you will see when I make use of such a liquid that I powerfully augment the transparency of this paper. That simple experiment when you dip your towel in the basin in the morning, is extremely full of philosophy; the towel becomes darker because the interstices are filled with water instead of air, and becomes more tranaparent. I have here some of the bibulous paper. Mr. Anderson will give me a little olive-oil, a substance possessing pretty nearly the same refractive power as the fibres of the paper. I will dip a rod of glass into the oil, and cause a drop of it to fall upon the paper. First of all I cast the image of the white paper upon the screen. Having done that, I will show you how I at once augment the transparency of the paper if I allow a drop of oil to fall upon it. There is the oil trickling down, and you see how greatly the transparency is augmented by the saturation, the filling up of the little interstices of the paper with the oil, which possesses a refractive index very nearly equal to that of the fibres of the paper itself. Thus you see in this way we can render this opaque substance in some measure transparent: and this is the philosophy of the tracing paper used by engineers; they take tissue paper, saturate it with oil, dry it, and then place it upon their drawings; then they can easily see through it, and copy the plan underneath upon the tracing paper. These things have all their peculiar philosophy.

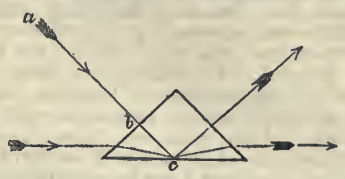
Let me now go on to the more immediate subject of this day's lecture. I have said at the commencement of the list of memoranda that when a ray of light passes from a rarer medium to a denser it is bent towards the perpendicular. You must not, however, imagine that the heavier the body the more power it has to bend a ray of light. That is in a great many instances the case, but not always, as I will prove to you. For instance, I have here a cell, and this cell contains two substances perfectly transparent, one floating above the other. The under liquid is water, the top is turpentine. The spirit of turpentine you see is lighter than the water, and is therefore less dense. It floats upon the water; but still you will find that it will bend the ray more than the water, notwithstanding its being less dense. We will make our experiment in the usual way. I will project its image on the screen, and you will see very clearly the limiting surface between the turpentine and the water. There is a little undulation of the surface caused by the motion, and you see our beautiful beam divided by a thin dark line which corresponds to a portion of light reflected by that surface. The beam is at present going straight through; but I will turn my cell so as to cause the beam to fall obliquely upon both liquids (and remember, the image is inverted, the turpentine, which is really at the top, appears at the bottom; the

cell being apparently turned upside down, as I have already explained to you), when, if the turpentine, the lighter liquid of the two, possesses a greater power of refraction than the water, its image will move the most. When I move the cell obliquely you see that the line of continuity is immediately broken, and it is perfectly manifest that the lower image is moved further than the other, and if I turn it in the other direction you will have the same result, thus proving that the turpentine has a greater power of refraction than the water, although it is less dense. Hence, when we speak of one body being denser than another in optics it will not do to affix the ordinary idea to the term dense. What is meant by a dense body in optics is one which refracts the light powerfully; and when one body refracts more than another it is the denser of the two.

Let me pass on to another very important portion of our subject, and that is the subject of total reflection. This is very important, and is capable of being understood by all of you. I will suppose this to be a vessel containing water, this (*a b*) being the surface of the liquid. We know



that if a beam of light fall upon the surface obliquely, it is bent down on entering. We also know that, supposing we had a luminous point (at *c*) underneath the surface, at the bottom of the vessel, and a ray of light quitted that luminous point along this line (*c d*), it would on emerging from the water be bent in this way (*d c'*). Suppose a second ray of light starts from the bottom of the vessel in this direction (*f' d*), it will be refracted on quitting the water, thus (*d f'*). Supposing now I draw a third ray (*g d*) starting from this point (*g*), a ray of light coming along that line (*g d*) would be bent so that it would just cross the surface and emerge along the line (*d g'*). Now supposing you come to a point beyond that ray (*g d*), which, when refracted, just crossed the surface, and supposing a ray of light were to come up from that point (*h*) to this (*d*). What would become of that ray? This one (*g d*) we suppose has just crossed the surface; this one, then (*h d*), will not be able to get out of the medium at all, but will be totally reflected or thrown back along the line (*d h'*); we have passed the limit of refraction, and got into the limit of what is called total reflection; the ray is wholly driven back into the medium again, and cannot escape from it. I find it so in this very turpentine; I look upon it and find that when a ray of light falls obliquely from the turpentine into the water, the light falling upon the surface is totally reflected, and communicates to it a shining metallic lustre. This is an experiment you can make for yourselves. Take a little water in a common glass tumbler of this kind, put a spoon in, and look at the spoon from beneath through the side of the glass; it is evident that the rays of light from the spoon strike against the upper surface of the water and cannot get out, so that you have a beautiful image of the spoon above. Put a shilling in the glass underneath the water, look from below at the surface, and you have a second splendid bright burnished shilling apparently floating upon the water above; because the rays of light, not being able to get out, are reflected back again and strike your eye, and thus you have it floating upon the surface of the water. I have here a little triangular glass prism, polished on the sides and face. Suppose a ray of light were to strike straight against the



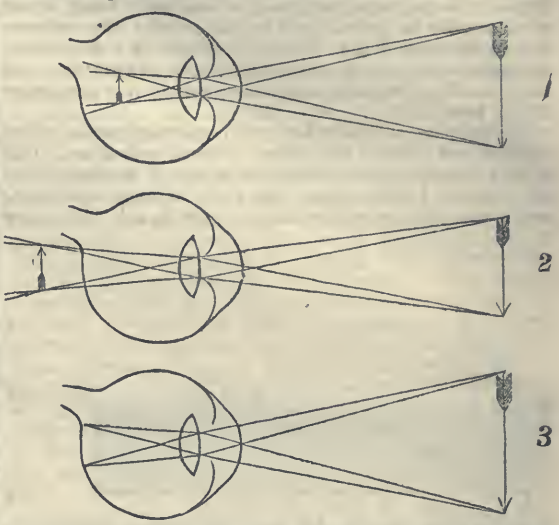
surface (*c*) in the direction (*a b*), then it so happens that in the case of glass it meets it so obliquely that it is totally reflected downwards or upwards, as the case may be. I think I cannot do better than show you the total reflection of the image of the coal points, which you know so well already. There you see that beautiful image, and I will let the ray strike right plumb against the face of the prism; it goes through and is reflected from the other, so that you will see the image of the coal points upon the ceiling. I will now send the ray of light in another way through the prism, parallel to the long surface; when it enters the prism it is refracted through the first surface, and strikes against the hypothenuse; it is there again totally reflected (see Fig.), and will quit the surface exactly at the same angle at which it entered. This is also a case of total reflection; when I turn the prism, you have the coal points turning round horizontally, one, as it were, attacking the other.

I will now direct your attention to another very interesting case of total reflection, a case which I intend to illustrate if I can by means of this apparatus. I have on this stool an electric lamp, and in front of it is a hollow iron vessel connected by a pipe with the water-pipes of the building, so that I can allow the water to enter it, and issue forth in the form of a jet from a hole near the top of the vessel. At the back, opposite the hole from which the vein of water will issue, there is a plate of glass, and I can send, as you will see immediately, a beam of light from the electric lamp through this plate of glass and straight through this vessel: I think you will see a cone of light passing through the hole and falling upon the part of the audience in front. I will now have the vessel filled with water, and when the jet issues, the beams of light that you saw striking against the audience will strike obliquely against the interior surface of that vein of water. What is the consequence? They cannot get out of the vein, they will actually be washed down as if the light was a solid thing, and reflected from side to side, but so obliquely that they cannot quit the liquid; and I trust in that way to illuminate the vein of water from top to bottom, by carrying down the light which formerly passed straight through. If I interpose a coloured glass the vein of water will be coloured. [The lecturer interposed glass of different colours, thereby colouring the vein of water.] So much then for this beautiful effect of the total reflection of light within the vein.

You have seen that by the most simple arrangement of lenses we have obtained very beautiful effects; we have obtained images of medals and of these coal points by one simple lens. You know also that with a convex pair of spectacles we can obtain inverted images of candles or any other object sufficiently luminous to cast a reflection on the screen I am now about to show you one or two effects with an apparatus which is a little more complicated than the simple lens I have hitherto used. The apparatus I have in my hand depends upon the same principle as, but is much more refined than, the magic lantern. The magic lantern consists simply of two parts, one part to illuminate the object, and the other part to make a magnified image of that object on the screen. I have here the representation of a little boy who is brushing a boot. First of all I illuminate that boy by casting a beam of light on this glass transparency. The light goes through

it, and then by means of this lens that you see here, I cast an image of the boy on to the screen. The lamp does nothing more than illuminate the thing. Thus I have to all intents and purposes a magic-lantern. There is the little fellow you see. We are not here merely for the purpose of looking at those ridiculous things, but for the purpose of knowing the principle on which all these things, ridiculous and un-ridiculous, depend. Here also we have a microscope formed by the combination of lenses. I can throw a powerful light upon the object that I put between these two slides, and thus illuminate it very powerfully. Here in front, you see, I have a little system of lenses, by means of which I shall get a very high magnifying power, and I trust to be able to show you objects which would entirely escape your power of sight if I made use of a lens of the former kind. Every boy present must have seen those figures that are not at all uncommon now in London and elsewhere—those beautiful figures of the frost—those beautiful crystals that are frequently formed upon the surface of the window panes. I am sure those things are worthy of every boy's attention, and not only worthy of boys' attention, but worthy of men's also, for they are among the most wonderful things in the world. We hear people talking contemptuously of matter, and pouring scorn upon matter; but these are only people who do not understand matter. Matter, like everything else in creation, is glorious when you see its laws and phenomena with a clear eye. Now, these little particles of water have a power inherent in themselves of building themselves up in geometric forms when they are chilled—when they are cold: they have the power of building themselves up in these wonderful and beautiful crystals that you see upon the surface of the window panes. I have sometimes warmed a pane of glass on which those crystals were deposited, and I have produced thereby a liquid film all over the pane, and I have looked on that film, and I have seen it just begin to move, and then the little atoms have run together as if they were alive, and have weaved a web of such beauty, that nothing man ever did, or can do, can approach it, and still this is very common. It is a thing occurring every winter, and I do not know whether boys have ever sufficiently reflected upon the marvellous beauty—upon the wonderful miracle—that is involved in the formation of these splendid frost crystals. There is not a bit of sugar-candy which you suck which does not involve questions before which Sir Isaac Newton, Mr. Faraday, or the wisest man who ever lived, is a mere child—he absolutely knows nothing about it; and yet we continue thoughtlessly to pass over these beauties without opening the eyes of our minds to their wonderful history. I will try now to show you a similar case by freezing some other substance—not water, for I cannot readily freeze water with a strong beam of electric light upon it, but I will take some other substance that I *can* freeze, or at least, try to freeze. In the first place it is absolutely essential for this experiment that I should have my plates of glass perfectly clean; and so I do not wash them simply with water but first with strong solution of potash. Mr. Anderson will now give me a solution of sal-ammoniac; and this is the thing that I am going to freeze—or rather to crystallize:—freezing is a process of crystallization, but you would not perhaps apply the term freezing to this action. I have now a film of liquid upon this glass plate, somewhat similar to that of water upon the window pane, and I will try and introduce the plate between those two slits and illuminate it by means of the electric light and cast the image on to the screen. [The experiment was then performed and in a few seconds the crystals of sal-ammoniac were seen shooting into the field of view.] There you see it, boys; look at those crystals which are being formed, do they not march like an army; the "atoms march in tune," as some poet says. The process that goes on upon the window panes on a winter's night is equally beautiful. There they are; how they rush like living things through the liquid! This then is one of the things that this beautiful microscope is capable of showing.

I will now go on to the consideration of another portion of our subject, and that is the most wonderful optical instrument of all—the human eye. You will understand the general structure of the eye in a moment. Cast your eye upon any one of those figures here [referring to Figures 1, 2, and 3.]



You have there the general structure of the eye, supposing it to be cut through, and that you look at the cut sideways. There is a thing just in front of the eye like a watch glass, called the cornea; it holds a little fluid called the aqueous humour and behind that there is a little lump of jelly-like matter called the crystalline lens; and it would really be worth while to get an ox's eye from a butcher and cut off the coat and get at this little lens. Behind is the general mass of the eye-ball, filled with what we call the vitreous humour. As I look at the audience, what takes place? Something that ought to excite your wonder and astonishment. When I look towards you, if any of you could get behind and look at the back of my eye you would see printed upon a little space at the back of my eye an image of the whole audience, some perfectly distinct, others not so distinct. Those that I look at distinctly are perfectly imaged. Now, there is another remarkable thing that instead of sitting upright, you are depicted in my eye sitting with your heads downward. Supposing that (No. 3) to be an arrow, the rays of light go through the pupil of the eye, through this round hole that you see in the centre, which is surrounded by what is called the iris. The light comes from the pupil thus, and the point of the arrow sends its light upwards, and the feather end sends its light downwards; so that you see the image of the arrow is inverted on the retina, as it is called,—an extension of the optic nerve at the back of the eye.

I want to actually prove to you, by experiment, that that is the case,—a very rude experiment, indeed, but still a very instructive one. I have here tried to make an eye; it is a very large spherical glass vessel filled with water. You see I have surrounded it on one side with black paper. There is a hole in the paper which represents the pupil of the eye. I will place in front of that hole a crystalline lens and will allow the light to fall upon the lens and through the water. Here at the back there is a tracing paper screen sufficiently transparent to allow you to see an object through it. This tracing paper represents

the retina. We will see if we cannot get an inverted image of the candle upon the retina. [A lighted candle was placed in front of the artificial eye.] There is the candle, and you see the image of the candle is inverted on the retina; we will throw a beam of light upon the candle so as to illuminate the stem of the candle, and then, I think, we shall see it still brighter. [This was accordingly done.] You see how finely shown it is: the candle is pretty visible from this point behind the tracing paper, and is inverted. Now, let me take something else; let the assistant go in front and I will illuminate his hands. There, you see the image, and if he puts his hand upright you will see that the image of the finger-nails will be down. In like manner if I take the dial of my watch you see I can throw its image on the retina, and a very beautiful object it forms.

We have to bear in mind, first of all, that in order that an object may be seen with distinctness, the rays of light issuing from it must come exactly to a focus upon the retina. Now, what takes place really? Some boys' eyes very often have too powerful a refraction; they combine the rays of light here (No. 1) [indicating a spot between the crystalline lens and the retina]; and the consequence is they cause the rays of light to come to a focus before they reach the retina, and thus, instead of having a sharp point upon the retina, you have a blurred image as you saw there, when I did not hold the watch at the proper distance. Some eyes, especially old eyes, have not the power of converging the rays of light sufficiently, and what is the consequence? They do not come to a focus at all; the eye is unable to bring them to a focus on the retina; they converge towards a point behind the retina. This is a case of long sight. What does the boy do who is short sighted? He holds the object close to the eye, because he wants to throw the point of intersection back. He has to look closely at the object, and hence he is said to be "short sighted;" and such boys, if they do not wish to look at the thing very closely, have to put a concave lens in front of the eye: this gives the rays a certain amount of divergence, and the focus is thrown back to the retina. On the contrary, those people who wish to bring this point of intersection forward must use a convex lens, as I have said. The eye has insufficient power to refract the rays. To help the refraction we place a double convex lens, or a plano-convex lens in front. I will show you these different lenses. Sometimes when I am tired I require to look through a pair of spectacles, and Mr. Anderson has given me the spectacles that he uses in this room, and here is another pair of spectacles that he uses for reading, or when he wants to see small objects. Now, I will show you the difference between these different glasses. I will take my own spectacles first. Here I want to show you, first of all, the convergence of the beam of light into the middle of the lens; but look how far I have to go away in order to get a perfectly clear image of the coal points. They are quite small; but perfectly clear. Now I will take Mr. Anderson's least magnifying spectacles—those with which he looks at you, and there you see the image is very large—larger than mine. If I take this other pair of Mr. Anderson's —those which he uses to read by—you see for yourselves how large the image is. Many of you, I have no doubt, have fathers who use spectacles of this kind; borrow them and make this beautiful experiment for yourselves.

I will now say a few words on the effect of the impression made upon the eye. When a flash of light falls upon the eye, supposing that flash of light could pass away instantly it strikes the eye, does the impression vanish at the moment the light vanishes? No. Everything in nature takes time to subside; and the consequence is that if you take the burning end of a stick, and then cause it to pass slowly through the air, you can follow the point of that stick; but if you make it describe a circle in about the fifth of a second, you see that circle as a continuous line of light, because in point of fact the impression remains upon the eye during the time the point goes round—that is, if it go round as quickly as I have said. If it travel round the circle in less than the fifth of a second, you find that it forms a continuous line. Now, instead of a burnt stick, I will take our lamp and I will try to show you this line of light on the ceiling. I will reflect this beam of light—this fine index you have there—from a looking-glass, and if I am skilful enough in turning this, you see the light travel round and gives a single image. If it travel quickly enough you see a continuous ring of light, because every revolution is accomplished within the time that the image takes to subside; and thus, as I have stated in the list of memoranda, when a series of sparks follow each other in succession at intervals of less than the fifth of a second, the impression made by one spark remains upon the eye until that made by the next spark, and you see the succession of sparks as a continuous line. Here I have a beautiful case of this kind, which will show it to you very distinctly. Here is a system of tubes, and we have a little battery underneath the table, by means of which I can send an electrical discharge through them. [The experiment was then performed.] Here we have this beautiful effect; you see in each of these branches we have a continuous light. There is a series of discharges, and each single image corresponds to a single discharge. If I turn it at a certain amount of speed, you see that the wheel appears to be resolved into distinct spokes. Here you see [causing the branches of the apparatus to revolve rapidly] I have a star of light. This is owing to the continuity of the impression produced by the succession of these discharges.

Special General Meeting.

Monday, January 13, 1862.

The Rev. John Barlow, *M.A., F.R.S., Vice-President, in the Chair.*

The following Address to Her Majesty, the Queen, in reference to the decease of H.R.H the Prince Consort, Vice-Patron of the Royal Institution, on December 14th last, was read and unanimously adopted:—

To the Queen's Most Excellent Majesty.

May it please your Majesty,—We, the Members of the Royal Institution of Great Britain, respectfully desire to express to your Majesty our grief for the loss which has fallen upon the Kingdom, upon our Institution, and, with exceeding weight, upon your Majesty personally.

May it please God, who grants consolation in His own due time, to give it to your Majesty, even while your thoughts are directed towards him that is gone, and may the recollections of our Prince's doings whilst in life have an abiding influence for good upon the many millions who have heard of and rejoiced in his name.

CHEMICAL SOCIETY.

Thursday, January 16, 1862.

A Paper, by Dr. H. Bence Jones, "*On the Simultaneous Variations of Hippuric and Uric Acids in Healthy Human Urine,*" was read by the Secretary.

The determinations of the amount of hippuric acid

present in urine that had been made both by Dr. A. Wiseman and by M. Ravenshoe, gave very high results; the method employed by M. Ravenshoe being that recommended by Profesor Liebig, two healthy men being the subjects experimented upon; the amount of hippuric acid in the urine obtained from patients suffering from different diseases had also been determined, but Dr. Bence Jones thought that further experiments on its occurrence during disease were needed.

A table was exhibited showing the amounts of uric and hippuric acids occurring in samples of urine obtained from various subjects both before and after food had been taken, control determinations having been made in each case; Liebig's method being employed.

Dr. Marcet remarked, that he had tried to extract hippuric acid from urine by means of ether, but that other acids and urea were partially taken up by the solvent, and also the colouring matter of the urine; it was, however, easier to obtain it from other parts of the system, as for instance from the blood; he had found the following process to answer in this case:—After coagulating the blood by heat, filter, and concentrate, then add alcohol, filter, again concentrate, add dilute sulphuric acid to separate fat, neutralise with chalk, evaporate to dryness, and extract the hippurate of lime by means of alcohol. He also stated that the presence of this acid in diseased urine had been detected by several experimenters.

Mr. Bloxam said he had employed several published methods for the extraction of hippuric acid, but had not found them to succeed satisfactorily.

The President said that Dr. Bence Jones had employed Liebig's process, which was to evaporate the urine and add hydrochloric acid, and then to act on the precipitate obtained with ether, so that the urea did not interfere; that the concordance between the control determinations was a proof of their correctness, and that he himself was assistant to Liebig when he was making experiments upon this method of determining the acid, so that he could speak from experience as to its correctness.

The next Paper was by G. F. Rodwell, Esq. "*On the Solubility of Sulphate of Lead in Hydrochloric and Nitric Acids.*" In order to determine the solubility of the salt in acids of different strengths, pure and dry sulphate of lead was digested in the acid at the ordinary temperature for a period of from one to ten days; the maximum amount was, however, always dissolved before the fifth day. To determine the amount dissolved, a portion of the solution was weighed out, a little sulphuric acid added, and the whole evaporated to dryness, ignited, and weighed. It was impossible to evaporate in the ordinary way a concentrated solution of the sulphate in hydrochloric acid, for a scum was apt to form on the surface, and this was thrown out by the expansion of the steam underneath; this difficulty was overcome by evaporating the solution in a crucible, and finishing the process in an air bath, the lid being placed on the crucible. The following determinations have been made:—A hundred grains of hydrochloric acid dissolved the following weights of the salt:—

Sp. gr. of Acid.	Weight of Salt.	Result of Dilution.
1·0515	0·14665	No precipitate.
1·080	0·35495	Needle-shaped crystals deposited.
1·107	0·946525	A larger quantity deposited.
1·135	2·113800	Ditto.
1·157	2·854000	Ditto.

If the solution be evaporated, plates of chloride of lead are deposited, which, when dissolved in water and recrystallised, furnish needles; but if the solution of the salt be evaporated until the sulphuric acid becomes concentrated, sulphate of lead is deposited. With nitric acid the following quantities were dissolved:—

Sp. gr. of Acid	Weight of Salt.	Result of Evaporation.
1·079	0·329925	Octohedral crystals of nitrate of lead.
1·123	5·755000	
1·25	0·784000	
1·42	0·009725	A crystalline powder of nitrate of lead.

When the salt was digested with nitric acid containing sixty per cent. of acid, it was almost entirely converted into octohedra of nitrate of lead, but the whole of the sulphate was not decomposed even on standing for several days.

Dr. Gladstone remarked that it was a curious circumstance that diluting a solution of sulphate of lead in hydrochloric acid, chloride of lead was deposited, and he thought a redistribution of the acids and base took place. He had examined solutions of phosphates, oxalates, and other salts, and found that no precipitate was produced on dilution; he had observed that if to a solution of sulphate of lead in hydrochloric acid either sulphuric acid or a lead salt were added, sulphate of lead was deposited.

Mr. Field said that sulphate of lead was completely decomposed either by oxalic or hydrochloric acid.

Mr. De la Rue remarked that sulphuric acid will not separate lead completely even from a solution of the acetate, and that more remained in solution than was due to the solubility of the sulphate in water.

The next Paper was by Dr. Hugo Muller, "*On a New Mode of Effecting Chlorine Substitutions.*" He had found that a great difference existed between the action of chlorine alone on organic compounds, and its action on the same compounds in the presence of iodine; also, that in the latter case the action took place with much greater facility. Benzol, for instance, although acted upon with difficulty by chlorine alone, was attacked with ease by the mixture of the two elements; two series of compounds being obtained, the formulæ of which are expressed as follows:—

$C_6 H_6 Cl_2$ $C_6 H_5 Cl$
$C_6 H_6 Cl_4$ $C_6 H_4 Cl_2$
$C_6 H_6 Cl_6$ $C_6 H_3 Cl_3$

From these tables it will be seen that when chlorine alone was employed, direct union took place, but with chlorine and iodine together substitution products were obtained. Burmese naphtha had also been experimented upon and furnished several compounds. With bisulphide of carbon, chloride of sulphur and chloride of carbon were obtained, and a third body, which was probably a compound of carbon, chlorine, and sulphur. When chlorine was passed into a solution of iodine in acetic acid in the cold, no action took place; but on the application of heat chloracetic acid was produced even in the dark, although when chlorine alone is employed strong sunlight is known to be necessary. It was possible that the iodine acted as a carrier of the chlorine, or that the action might be similar to that of pentachloride of antimony, or of phosphorus.

The President remarked that the ordinary process for obtaining substitution compounds was sometimes very tedious, and that a process which would render their preparation easier would be very valuable.

Mr. De la Rue said that the present results were merely the beginning of a series which Dr. Müller and himself hoped shortly to lay before the Society.

The President inquired how rapidly the action of chloroform upon glacial acetic acid took place, to which

Dr. Muller replied that it acted with great rapidity, the limit of production depending merely on the size of the apparatus.

Mr. Bloxam inquired whether phosgene gas could be produced in this manner; but it appeared that this experiment had not yet been tried.

Dr. Frankland suggested that the action might consist in the combination of the iodine with the hydrogen set free by the chlorine, the hydriodic acid being afterwards decomposed.

Dr. Muller thought this was more probable than his original supposition, which had already been upset by further experiments.

The President remarked that reactions of this kind had been observed to take place in other cases, as, for instance, in the action of chloride of cyanogen on aniline. When this substance is exposed to the action of cyanogen itself a direct combination takes place; but with chloride of cyanogen he had found that a substitution compound was produced, the chlorine uniting with the hydrogen set free and forming hydrochloric acid.

It was announced that at the next meeting,

Mr. Bloxam would read a paper, "*On the Source of the Arsenic in the Sulphuric Acid of Commerce, and on a Method of preparing the same free from Arsenic.*"

The following Address, to be presented to Her Majesty on occasion of the death of the Prince Consort, was read and adopted:—

To the Queen's Most Excellent Majesty.

May it please your Majesty,—We, the President, Council, and Fellows of the Chemical Society, beg permission to approach your Majesty and humbly express our great sorrow at the death of His Royal Highness the late Prince Consort, who during his life evinced a warm interest in the progress of the science to which our labours are devoted. We pray that the support and consolation of Almighty God may be vouchsafed to your Majesty under this bereavement, and that your Majesty may long continue to reign over a loyal and faithful people.

(Signed) A. W. Hofmann, *President.*

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, January 7, 1862.

Edward Schunck, *Ph.D., F.R.S., Vice-President, in the Chair.*

Professor Dr. Johannes Gistel, of Kempten, in Bavaria; Federico Lancia di Brolo, Inspector of Studies in the University of Palermo; and James Nasmyth, Esq., C.E., were elected Corresponding Members of the Society.

A Paper was read by J. P. Joule, LL.D., President, entitled, "*Experiments on some Amalgams.*"

The weakness of the affinity which holds the constituents of amalgams in combination seemed to the author to offer the means of studying the relationship between chemical and mechanical force. His inquiries were extended to several amalgams, and gave results of which the following is a summary:—

Amalgam of iron was formed by precipitating iron on mercury electrolytically. The solid amalgam containing the largest quantity of mercury appeared to be a binary compound. Iron does not appear to lose any of its magnetic virtue in consequence of its combination with mercury. Its amalgamation has the effect of making it negative with respect to iron in the electro-chemical series. The affinity between mercury and iron is so feeble that the amalgam is speedily decomposed when left undisturbed, and almost immediately when agitated. The application of a pressure of fifty tons to the square inch drives out so much mercury as to leave only thirty per cent of it in the resulting button.

Amalgam of copper.—By precipitating copper on mercury electrolytically, a mass of crystals is gradually formed. After a certain time the crystals begin to get fringed with pink, indicating uncombined copper. In this state the amalgam is found to be nearly a binary compound. On applying strong pressure to an amalgam containing excess of mercury, the latter is driven off leaving a hard mass composed of equivalents of the metals. If, however, the pressure be continued for a long time, the resulting amalgam contains more than one equivalent of copper, indicating a partial decomposition.

The Author gave an account of his experiments with amalgams of silver, platinum, lead, zinc, and tin. In the case of the latter amalgam, long-continued pressure drives off nearly the whole of the mercury, indicating in a striking manner the efficacy of mechanical means to overcome feeble chemical affinities.

Dr. Angus Smith said: It is difficult to tell the exact limits of chemical and mechanical action, because they flow into each other. Let us call the attraction of surfaces a mechanical action, as it is not to our knowledge a chemical combination. Porous bodies exercise this to a very large extent, and yet do not produce chemical compounds. The amount is limited on one side by the pressure of the atmosphere. Chemical compounds are too powerful to be affected by such slight forces. Porous bodies or surfaces do not take up others in chemical equivalents so far as we know; the full capacity of saturation is not satisfied, because of counteracting influences. Water admits air very rapidly, but it is held so slightly that it is affected by atmospheric pressure, and even seems to follow exactly the atmospheric pressure; but a portion is held with such power that it is extremely difficult to remove, and is not ever removable by the mere removal of pressure as far as we know. The small affinity of the great mass or surface of water is equal to a great affinity for a small amount of air. All masses have more or less this mechanical action, but, as in the case of porous bodies, the attraction is feeble, and not raised into the power of a definite grasp of a given quantity such as an equivalent, which is the case with a powerful affinity forming a chemical compound. Large and watery masses lose their water slowly by the mere force of gravitation. Strong mechanical action rises to an equality with feeble chemical affinity. Weak chemical affinity sinks into an equality with mechanical action. Charcoal absorbs gases more eagerly under pressure, but by a removal of pressure they are still absorbed; so that the mechanical force is greater than the weight of the atmosphere can control. There are many cases in which these two forces, if they be two, meet. This of the mercury and other metal is one case. The feeble chemical affinity is, I suppose, overcome by the powerful mechanical force. The alloy with sufficient chemical affinity remains, that with a weak affinity separates. The mercury flows off following the law of liquids; in like cases it flows off like water flowing slowly from a moist porous mass like wet clay. Instances from the feeblest to the most powerful affinity might be given, showing that only when the power reached a definite point did the law of chemical equivalents come in. At the same time there is a definite point where surface action ceases under certain conditions.

These ideas have arisen partly from experiments on the subject, which may some day be published. I believe they explain the difficulties attending the attraction of masses which seem occasionally to oppose the combination by atomic weights.

A Paper was read "*On the Conductibility of Heat by Amalgams,*" by Dr. F. Crace Calvert, F.R.S., and Mr. Richard Johnson.

The method followed in the investigations described in

this Paper is the same as that detailed in their former Paper on the conductibility of metals and alloys.

In the first part of their Paper the authors treat of the conductibility of mercury, and prove that if the source of heat be applied at the upper part of a column of mercury, so as to prevent any motion of the solid molecules of the mercury, this metal becomes the worst conductor of all known metals; for, silver being 1000, mercury is 54.

In the second part of their Paper the authors examine the conductibility of the solid and semi-solid amalgams prepared in equivalent quantities of pure metals with mercury, and they show that amalgams may be divided into two classes—those containing an excess of equivalents of the amalgamated metal, and those which on the contrary contain an excess of equivalents of mercury. The first class conduct heat at the mean rate of the two metals composing the amalgam, and in accordance with the calculated result, as shown by the following table:—

AMALGAM OF TIN.

Mercury = 21·63 or 679
Tin . . = 13·45 or 422

			Silver = 1000	
	Found.	Calculated.	Found.	Calculated.
6 Sn. Hg. . .	10·60	10·85	332	340
5 „ „ . .	10·30	10·50	322	329
4 „ „ . .	9·65	9·95	302	312
3 „ „ . .	9·45	9·20	296	288
2 „ „ . .	8·65	8·11	271	254
Sn. Hg. . .	5·15	6·05	161	189
„ 2 Hg. . .	4·75	4·36	149	136
„ 3 Hg. . .	4·20	3·70	131	116
„ 4 Hg. . .	3·95	3·21	124	101
„ 5 Hg. . .	3·65	2·93	114	92

The second class, comprising those amalgams containing an excess of mercury, conduct heat as if they contained no other metal, although its proportions may vary from 10 to 34 per cent.

These interesting results were confirmed by observing amalgams of tin, zinc, bismuth, copper, lead, and silver, and applying the source of heat in all cases at the upper part of a perpendicular column of the amalgam.

The third part of their Paper has reference to the conductibility of mercury when mixed with two per cent. of various metals, and when the heat is applied at one end of a horizontal column; and they have obtained the following interesting series of results:—

	Found.	Calculated.	Found.	Calculated.
Pure mercury .	21·63		679	
Mercury with two per cent. of				
Silver . . .	2·30	21·81	72	684
Tin. . . .	5·65	21·43	177	672
Copper . .	13·19	21·62	413	678
Gold . . .	14·50	21·79	454	680
Bismuth . .	18·75	21·20	588	664
Lead . . .	19·25	21·34	603	668
Cadmium. .	20·20	21·51	633	676
Zinc . . .	21·20	21·56	664	676

This table shows that the greater or less conducting power of the metal amalgamated with it has no influence in modifying the conductibility of the amalgam itself; for we find that the amalgam of bismuth (the worst conducting metal) conducts heat eight times better than that of silver (the best conductor), for

Mercury + 2 per cent. of Bismuth. . . . 588
„ + 2 „ Silver 72

They were induced to believe at the beginning of these researches that the cause which impeded the conduction of heat in so marked a manner in some of these amalgams was the presence of small crystals of amalgam floating in excess of mercury, as they had observed that whilst the crystallised amalgams of silver and tin conducted heat badly, that of zinc, which was perfectly fluid and free from crystals, conducted heat very freely. As they pursued their researches they found these views to be incorrect, for the amalgam of bismuth, a very crystalline one, conducted heat with great facility.

Having observed that tin affected the fluidity of mercury in a most remarkable manner, so that even the one hundred thousandth part of that metal would interfere with the property which mercury has of assuming easily a globular form, they prepared the following series of amalgams of tin:—

		Silver = 1000	
	Found.	Calcu.	Found.	Calcu.
Mercury 94·50 + Tin 5·50	4·00	21·14	125	663
„ 97·00 + „ 3·00	4·60	21·35	144	669
„ 98·00 + „ 2·00	5·65	21·43	177	672
„ 98·25 + „ 1·75	5·90	21·45	185	673
„ 98·50 + „ 1·50	10·95	21·47	343	673
„ 99·00 + „ 1·00	19·30	21·52	605	675
„ 99·50 + „ 0·50	19·30	21·56	605	675

This table proves that up to 1·75 the conductibility of mercury remains constant, when by reducing the tin by $\frac{1}{100}$th of the conductibility of the amalgam is doubled; by further abstracting one-third of the tin the conductibility is again doubled.

NOTICES OF PATENTS.

870. *Combustion of Fuel for Generating Steam.* W. H. Phillips, Nunhead. Dated April 9, 1861.

This invention relates to the employment of steam in the place of atmospheric air or other gases for the purpose of supporting the combustion of fuel, and generating heat in furnaces or fireplaces of suitable construction.

The ultimate products of combustion are identical, whether air alone or the same mixed with aqueous vapour be admitted to the burning fuel; the equivalent of heat disengaged must therefore remain constant under the varying circumstances. There are, however, certain practical considerations which are likely to declare in favour of the employment of steam; such, for instance, as the cooling of the furnace bars, and the more rapid generation of inflammable gases in the midst of the burning fuel.

876. *Apparatus for Receiving, Drying, and Deodorising Human Excrement.* F. Taylor, Romsey. Dated April 9, 1861.

This proposal refers to certain mechanical contrivances for separating and collecting the liquid and solid fæcal products, and for drying the latter *in situ* with the aid of a powerful current of air.

894. *Obtaining Ammoniacal Salts and other Valuable Products from Liquors or Substances Containing Ammonia.* C. N. Kernot, Gloucester House, West Cowes; and M. D. Rucker, Fenchurch Street, London. Dated April 11, 1861. (Not proceeded with.)

In carrying out this invention it is proposed to mix the liquor in a tank or other suitable vessel, with crude sulphate of zinc, or other sulphates, or a liquid specially prepared by acting on animal or mineral substances with diluted acids, the object being to produce gelatinous precipitates; or the ammoniacal liquor may be passed through filters containing the sulphates or the preparation already referred to.

The aim of this specification appears to depend on the employment of a gelatinous precipitate for the purpose of absorbing and thus concentrating the free ammonia contained in dilute gas liquors, &c. The compounds derived

from the zinc salts are, however, all or mostly of a soluble nature (the oxide of this metal being easily dissolved by ammoniacal salts), and consequently are ill-fitted to perform the function here required to be exercised by the chemical agent.

Grants of Provisional Protection for Six Months.

2293. Marc Antoine Francois Mennons, Rue de l'Echiquier, Paris, "An improved apparatus for the conveyance of medicinal substances into various parts of the human body."—A communication from Salvator Vinci, Rue de la Pepinière, Paris.—Petition recorded September 14, 1861.

3092. William Ford Stanley, Great Turnstile, Holborn, London, W.C., "The use of aluminium for the construction of mathematical instruments used for geometrical drawing, surveying, and nautical purposes, and improvements connected therewith."—Petition recorded December 10, 1861.

3105. Joseph Schloss, Cannon Street, London, "An improvement in forming the leaves of albums and books for containing photographic portraits and views." — A communication from Simeon Schloss, Paris.

3109. John Potter, Leeds, Yorkshire, "An improved mode of jointing or connecting telegraph wires, which is also applicable to jointing or connecting signal wires, fencing wires, and other wires or rods."—Petitions recorded December 11, 1861.

3116. Robert Mushet, Coleford, Gloucestershire, "An improvement or improvements in the manufacture of iron or puddled steel."

3138. Thomas Killingworth Adkins, Wallingford, Berkshire, and John Bonthron, Regent Street, London, "Improvements in the manufacture of starch, and in apparatus employed therein."

3141. Richard Archibald Brooman, Fleet Street, London, "Improvements in blowers or apparatuses for superheating steam and other gases, and for projecting them, combined with atmospheric air, upon ignited combustible matter."—A communication from Felix Alexandre Testud de Beauregard, Paris.—Petitions recorded December 13, 1861.

3143. Jacques Edouard Duyck, Tutsham Mills, West Farleigh, Kent, "Improvements in the expression of oil from cake and seeds, and in apparatuses employed therein."

3147. William Elliott Debenham, The Terrace, Kensington Gardens Square, London, "An improved plate-holder for photographic purposes."

3148. William Husband, Hayle, Cornwall, "An improved water safety-valve."

Notices to Proceed.

2123. George Nye, Mount Street, Lambeth, London, "Improvements in apparatus for administering injection in a continuous stream, also applicable as an eye-douche, and other purposes."

2142. Benjamin Browne, King William Street, London, "An improved process and apparatus for concentrating ores or tailings, or separating pulverised mineral substances of different kinds or qualities from each other."—A communication from Henry Eberhard Roeder, Broadway, New York, U.S.—Petitions recorded August 28, 1861.

2159. Alexandre Jaille, Agen, Lot et Garonne, France, "An improved manufacture of manure."

2161. Henry William Spencer, Stepney Causeway, Commercial Road, London, "Improvements in the manufacture of animal oils, the said improvements relating more particularly to the processes of refining them to be used for lubricating purposes."

2213. Frederick Bennett, Upper Works, Bagillt, Flintshire, "An improved method of coating the interior surface of lead and lead composition pipes with tin or its alloys."—Petitions recorded September 5, 1861.

CORRESPONDENCE.

Manufacture of White Lead.

To the Editor of the CHEMICAL NEWS.

SIR,—As a general painter I use a considerable quantity of white lead, which for many years I have bought in the ground state, I mean ground with linseed oil, and in this condition I have mostly found it very coarse in texture, and more or less adulterated with carbonate or sulphate of barytes.

If purchased in the dry state it is seldom adulterated, I have, therefore, of late, obtained it in this condition and ground it with oil myself, and here is my difficulty—how to get it fine.

All the dry carbonates of lead I have been able to obtain from some of the best manufacturers have been of a very hard and gritty nature, and when mixed with oil, a very large amount of grinding is necessary to reduce it to the required smoothness, although ground under heavy stones with steam power. The ground lead sold by most makers is much coarser than desirable, and if spread out with the palette knife appears something like a sheet of very fine sand-paper—quite a granulated appearance.

Can any of your readers inform me how I may treat the dry carbonate, so that when mixed with oil it may be easily ground into a smooth mixture? I recollect years ago I did obtain, from an old London house, dry lead, which, with the palette-knife only, could be readily mixed with linseed oil into a smooth, butter-like mixture, without the least granulated form; but the leads now manufactured will not do so unless they are improved by being well levigated in water before being ground in oil. If I could be informed how to soften the carbonate of the manufacturers for grinding in oil, with less of this levigation, or without it altogether, I should be obliged.

I am, &c. A SUBSCRIBER.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Colour of Water.—Wittstein publishes (*Wittstein's Vierteljahressch.*, Bd. x. s. 342) a long memoir on the colour of water. He establishes—1. That pure water is not colourless but blue. 2. That the mineral matters which a water may contain do not affect the colour. 3. That the variety of colour in water depends more on the organic matter in solution. 4. The organic matter is held in solution by an alkali. 5. The amount of organic matter in solution depends solely on the amount of alkali. 6. The smaller the amount of organic matter the less is the colour removed from blue: as the organic matter increases the colour goes from blue to green, and eventually to brown. 6. Periodical changes in the colour of the same water do not depend on changes in the organic constituents, but on rest and atmospheric appearances. 7. It may be universally accepted as a rule that the browner a water is the softer it is; and the nearer the colour approaches blue the harder it is. The difference does not depend so much on the amount of organic ingredient, as on the amount of alkali. There is nothing very new in this. Experienced water testers have for some time judged or guessed the amount of organic matter from the colour of the water.

II. ORGANIC CHEMISTRY.

New Derivative from Benzoic Acid.—Schutzenberger and Sengenwald (*Comptes Rendus*, t. liii. p. 974) have examined the brown product, which is obtained when oxybenzoic acid is prepared by passing a current of nitrous acid into a solution of benzamic acid. They found that it dissolved in alkalies and formed brown, soluble, uncrystallizable salts, the solutions of which gave

brown amorphous precipitates with most metallic salts. Their analysis led the authors to the conclusion that the brown body is a bibasic acid, which they propose to call *benzulmic acid*. Its production at the expense of oxybenzoic acid is explained by the following equation:—

$$2\,(C_{14}H_6O_6) + 2NO_3 = 2HO + C_{28}H_{10}O_{12} + 2NO_2.$$

III. TECHNICAL CHEMISTRY.

Preparation of Arseniate of Soda.—Dr. Aug. Streng gives (*Chemisches*] *Central Blatt*, No. 54, 1861, s. 852) some researches he has made on the preparation of monobasic arseniate of soda for technical purposes. He concludes that the best process for obtaining this salt on the large scale is to heat together 30 parts of nitrate of soda, and 37 parts of arsenious acid, at first gently for 6 to 9 hours, and then raise the temperature for the same time. The mass should be allowed to cool slowly. The product has not exactly the theoretical composition of monoarseniate of soda, but the salt answers well for practical purposes. Only about 4 per cent. of arsenious acid is left.

New Way of Producing Local Anæsthesia.—M. Fournié has employed very successfully a mixture of glacial acetic acid and chloroform to produce local anæsthesia (*Comptes-Rendus*, t. liii. p. 1066). He puts a mixture of equal parts of these bodies into a conveniently shaped glass vessel, holds this in his warm hand, and applies the mouth of it to the part to be rendered insensible. In about five minutes, he says, complete insensibility, which extends to some depth, is produced. Of course the method can only be applied when the skin is sound; and if we wanted an abscess opened we would certainly give it a trial.

MISCELLANEOUS.

Royal Institution.—The following Lectures will be delivered in the ensuing week:—Tuesday, January 28, at three o'clock, John Marshall, Esq., "On the Physiology of the Senses." Thursday, January 30, at three o'clock, Professor Tyndall, "On Heat." Friday, January 31, at eight o'clock, William Hopkins, Esq., "On the Theories of the Motions of Glaciers." Saturday, February 1, at three o'clock, Rev. A. J. D'Orsey, "On the English Language."

Manufacture of Cement.—The following case, heard at the Court of Common Pleas before Lord Chief Justice Erle, on Tuesday the 14th inst., possesses interest from the circumstance that it refers to an unsuspected ingredient in builders' cement:—Fulwood *versus* Akerman and another.—The plaintiff was a cement manufacturer, and the defendants were executors of one Board, who owned quarries near Bridgwater. In 1849, the plaintiff having discovered a method of burning shale, and thereby making a bituminous powder, which was valuable in the manufacture of paint, ink, &c., disclosed his secret to Board, and entered into a contract with him to share the profits which should arise from the manufacture in Board's quarries of the article, which they denominated "Imperial Russian Black." After some time Board discovered that he could make a greater profit of the black by employing it in colouring cement than in selling it in its simple state. He, thereupon, ceased selling it in its pure state, mixed it with cement, and sold the cement in large quantities. The plaintiff then claimed his moiety of the profits on the sale of the black so sold in cement, as well as in its pure state. Board and his representatives refusing to pay this, the plaintiff brought the present action. In the course of the case it appeared that a previous action between the parties had been disposed of in 1853, and that although the plaintiff was aware that Board was mixing the black with cement, he made no claim then, nor till he brought the present action. The Court were of opinion that the agreement between the parties contemplated a division of the profits arising from the sale of the black in its pure state only, and, as it was not alleged that Board had mixed it with the cement fraudulently, or to evade payment, that the plaintiff was not entitled to recover. The rule must therefore be discharged.—Rule discharged accordingly.

American Chrome Iron Ore.—Asbestos Paper.—We have received from a correspondent in Baltimore, Mr. Oudesluys, of South Gay Street, an excellent sample of the chrome-iron of that locality. It occurs in the form of small black lustrous granules, many of which, under magnifying power, appear to be regular octahedrons. There is no appreciable quantity of magnetic oxide of iron intermixed with the sample, nor of other impurities which would tend to lower the per centage of sesquioxide of chromium. The amount of this latter constituent as determined by Dr. Genth, is stated to be equivalent to 63 per cent. of *chromic acid*—a mode of expressing the value of the ore by the quantity of chromic acid produced on fusion with an alkali, and not that of the green sesquioxide actually contained therein. By a qualitative examination we have ascertained that the proportion of chromium must certainly be very large, and have had at the same time an opportunity of corroborating a statement made by our informant to the effect that the mineral requires long-continued fusion to separate the whole of the chromium in a soluble form. Ore of this superior description may be obtained in casks ready for shipment at the rate of about one dollar for each one per cent. of chromic acid per ton, and in quantities of about 200 tons annually. It is, however, considered more judicious to work this ore in admixture with other qualities which are produced in greater abundance,—1500 tons annually,—the average composition of such samples furnishing usually about 50 per cent. of chromic acid. The ore last described was accompanied by specimens of asbestos, and of paper containing about one-third proportion of the same. The mineral may be procured from Mr. Oudesluys at the rate of 1½ cents per pound,—a low price considering the high quality of the article offered. The specimen sent is beautifully white, and the fibres are long and delicate. It has been tried in America for paper-making and for the manufacture of steam-packing, in both of which applications it is said to be very serviceable. Its property of resisting heat and its bad conducting power would render this material particularly valuable in connection with steam machinery. The sheet of paper sent is a portion of an experimental manufacture; it burns with flame, leaving a white incombustible residue, which, with careful management, retains the form of the original sheet, the weight of ash amounting precisely to 30 per cent. Characters written on the paper with ordinary black ink are still legible after burning.

ANSWERS TO CORRESPONDENTS.

*** All *Editorial Communications* are to be addressed to the Editor; and *Advertisements* and *Business Communications* to the Publisher at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

Vol. IV. of the Chemical News, containing a copious Index, is now ready, price 12s., by post, 12s. 8d., handsomely bound in cloth, gold-lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. A few copies of Vols. I. II. and III. can still be had. Vol. V. commenced on January 4, 1862, and will be complete in 26 numbers.

J. V.—We will bear your several requests in mind.

A. B. P.—It is not too late, such matter is always interesting. We shall be glad to receive the abstracts alluded to.

A. P.—See the "Chemistry of Calico Printing," noticed in our Second Volume, p. 214.

F. W. Primrose.—We cannot give the address. Apply to Mr. B. Woodcroft, Patent Office, Southampton Buildings.

A Subscriber.—Probably a letter addressed to the gentleman who exhibited the specimen would obtain the desired information. Direct to the Society.

THE CHEMICAL NEWS.

VOL. V. No. 113.—February 1, 1862.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Remarks on M. Stas's Researches on the Relations Existing between Atomic Weights, by M. MARIGNAC.

AFTER passing an eulogium on M. Stas's meritorious work, Professor Marignac urges the following considerations:—

He thinks the final conclusions of M. Stas too absolute. He will not be convinced of the exactness of the determination of an atomic weight; or, rather, he cannot entertain a clear idea of the degree of confidence to be reposed in it, only when the weight has been obtained by several perfectly independent methods, and determined by an analysis of several distinct compounds. Thus, M. Marignac expresses a wish that M. Stas—who obtained 107·943 and 107·924 for the equivalent of silver as the mean of each of two series of experiments—should analyse, with his usual care, salts of silver with organic acids. M. Marignac adduces the fact that in 1846 he arrived at the number 107·968 for silver by the latter method. Perhaps other methods will be found, showing whether the differences between the results furnished by the various methods are really less than the differences between these results and those of the calculation founded on Prout's hypothesis.

According to M. Marignac it is necessary for the settlement of the question to study, by analysis and synthesis, perfectly distinct compounds, and not to be satisfied with experiments differing only in the manner the same compounds are made to react.

When M. Stas sets forth, as a control of the synthesis of nitrate of silver, the experiments by which he determined the proportional relation between this salt and chloride of potassium, M. Marignac sees in this only a proof of the exactness of the experiments themselves, but not a control of the experimental method. For instance, let it be supposed that nitrate of silver, prepared in the most accurate manner, does not contain its elements in the rigorous proportions of their atomic weights, then the most exact methods applied to its analysis or synthesis, will only give, with the same degree of inaccuracy, the relation of these weights.

To develope his ideas on this subject, M. Marignac cites his own experiments on monohydrated sulphuric acid. He shows, in fact, that this compound, hitherto considered so stable, is, on the contrary, very unstable. It is only when it contains a slight excess of water (about 1 per cent.) that it becomes perfectly stable; otherwise, the slightest elevation of the temperature induces a disengagement of vapours of anhydrous sulphuric acid.

Who can affirm *à priori* that sulphide or nitrate of silver is incapable of retaining, at a high temperature, a small quantity either of sulphur or nitric acid, when sulphuric acid can retain a slight excess of water at above 100°?

Causes of error of this kind, to which others might be added, prevent M. Marignac from being convinced that the discrepancies between experiment and Prout's law cannot be explained by the imperfection of experimental methods.

Even admitting that these objections are groundless, M. Marignac urges a second consideration, preventing his concluding, with M. Stas, that Prout's law is nothing better than a delusion, and that simple bodies are necessarily distinct entities.

If M. Stas's numbers do not exactly coincide with those of Prout, at least they so nearly approach them that this fact cannot be regarded as accidental. The remark made on the laws of Mariotte and Gay Lussac, relative to the variations of the volume of gases, is applicable to the laws of Prout. These laws, long considered absolute, have been found inaccurate when examined with the degree of accuracy attained by MM. Regnault and Magnus. Nevertheless, they will always be considered as expressing the natural laws, whether from a practical point of view—for, in most instances, they allow a sufficiently approximative calculation of the changes in a volume of gases—or even from a theoretical point of view; for they very probably express the normal law of these changes in the volume, allowance being made for several disturbing influences to which we perhaps return further on to calculate the effects. It may well be believed that this is likewise the case with Prout's law.

M. Marignac concludes with an important observation due to the illustrious and sagacious chemist who drew attention to these interesting questions:—"The fundamental principle which led Prout to propound his law—that is to say, the idea of the unity of matter, and all the more or less brilliant conceptions based upon this principle—are altogether independent of the size of the elementary atom, which would serve as a common divisor of the weight of simple bodies, and which may, consequently, be considered as expressing the weight of atoms of primordial matter. Let this weight be that of an atom of hydrogen, or half or a quarter of an atom, or an infinitely smaller fraction of one, these considerations would not the less retain the same degree of probability. The result would simply be less simple relations of constitution between the various elements."

On the Formation of the Topaz and the Zircon, by M. H. SAINTE-CLAIRE DEVILLE.

IT is sometimes possible, during laboratory experiments, to foresee the mode of formation of certain kinds of minerals; but our conclusions cannot be perfectly exact unless we can show that the efficient physical and chemical circumstances are adequate not only to account for the formation of the minerals, but are essential for their production. This happy degree of certainty is not often attainable, but in some cases it is very nearly approached; as, for example, with topaz and zirconium.

By passing fluoride of silicium over calcined alumina placed in a porcelain tube and heated to whiteness, the whole is converted into staurotide;[2] fluoride of aluminium is disengaged, which may be collected, and which is identical with that I prepared after obtaining crystallised silicium (silicium diamond).[3] This staurotide, which is in right rhomboidal prisms, resembles natural staurotide in form and optical properties. Their composition is identical, as my analyses prove,

Silica . .	29·1	29·5	SiO_3 . .	30·2
Alumina .	70·9	70·2	$2Al_2O_3$ . .	69·8
	100·0	99·7		100·0

and ought to be represented by the mineralogic formula $SiAl_2$. It contains no trace of fluorine.

This circumstance led me to make the following experiment:—Into a porcelain tube, placed vertically, I put alternate and cylindrical layers of alumina and quartz, commencing with alumina, and finishing with quartz, and through this pile, when heated to white heat, I passed a current of fluoride of silicium. The layer of alumina became transformed into staurotide ($SiAl_2$), with production of fluoride of aluminium, which was entirely absorbed either by the quartz or silica. By this means the same staurotide ($SiAl_2$) is obtained, and the fluoride of silicium regenerated, and so on; the alumina and the quartz both being changed into the same crystallised matter, staurotide, of which I have given the analyses. As the last layer is composed of quartz, and as no trace of fluorine remains in the contents of the porcelain tube, it follows that after all these transformations the identical quantity of fluoride of silicium issued from my apparatus as entered it; a fact which is besides easily proved by experiment. Thus, fluorine itself, without becoming fixed, served to combine two substances the most fixed and most difficult to enter into combination—silica and alumina. Thus, a very small quantity of fluorine is required to transform into staurotide or to mineralize indefinite quantities of silica and alumina.

My analyses, which very nearly confirm those of M. Forchhammer, give for the composition for topaz the following numbers, which have reference to the least volatile elements of this mineral:—

	Saxony topaz.	Brazilian topaz.
Silica.	22·3	25·1
Alumina	54·3	53·8
Silicium	6·5	5.8
Fluorine	17·3	15·7
	100·4	100·4

At first sight it appears that it is possible to form this singular substance by the action of fluoride of silicum on alumina, under the above circumstances; but in my experiments I have never found any substance resembling topaz. Moreover, if Brazilian topaz is put in a current of fluoride of silicium in contact with alumina, which becomes transformed into staurotide, the topaz entirely decomposes, losing 22 per cent. of its weight. This experiment clearly proves that topaz cannot be reproduced in our laboratories, nor be naturally formed by the contact of alum and fluoride of silicium at a high temperature.

Topaz must be formed by the wet way. This is proved by Dr. Brewster's observations on the liquids which it contains, and by the results of my own analyses,[4] in which I estimated a volatile matter which I considered to be water; but according to M. Lervy it is an organic matter, and a nitrogenised organic matter according to M. Delesse. To these observations let me connect a remark to which I attach some importance. I have found vanadium in a great many hydrated aluminous matters, particularly in the Gibbsite of Baux. I have also found it in topaz which I believe came from Brazil; and this characteristic, common to many substances which decompose or are transformed by the action of fire, appears to me to indicate the intervention of water in the formation of minerals which contain vanadium. I hazard the hypothesis that it is decidedly from the hydro-fluo-aluminic acids which I have described in a recent memoir that topaz may be most easily derived.

Chondrodite, humite, or even lime and magnesia silicates cannot be formed under the influence of fluoride of silicium, for the magnesia or lime when heated in this gas are changed into vitreous or crystalline matters with a composition bearing no relation to veins of minerals and metamorphic earths. The following shows the composition of these substances, formed atomically in the most simple manner. Their formulæ, given in the following hypothesis, clearly shows their formation:—

Silica . . .	25·3	SiO_3 . . .	25·3
Magnesia . .	22·8	2 Mg.O . .	22·3
Magnesium . .	20·9	3 Mg. . .	20·4
Fluorine (by loss).	31·0	3 Fl. . .	32·0
	100·0		100·0
Silica . . .	24·3	SiO_3 . . .	23·8
Lime . . .	14·7	CaO . .	14·6
Calcium . .	31·0	3 Ca . . .	31·6
Fluorine (by loss).	30·0	3 Fl. . .	30·0
	100·0		100·0

With glucina, which, like alumina, yields a volatile fluoride, I hope to obtain phenatite. By passing fluoride of silicium at red-white heat over glucina, I obtained, besides fluoride of glucinium, some beautiful crystals which I have not yet measured, and which I can identify with no known mineral, for they have given the following results:—

Silica	65·8	2 (?)
Glucina	33·3	1
Oxide of iron . . .	0·6	
	99·7	

Thus fluoride of silicium yields by the dry way no known earthy mineral; but this is not the case with a substance found in volcanic earths—zircon—which is produced in the most beautiful forms by passing fluoride of silicium over zirconia. The octahedral crystals thus obtained, which I have measured, are exactly analogous to the zircons of Somma (Vesuvius). They have the same facettes, the same angles, the same external appearance; and it is almost absolutely certain that they were formed under the influence of fire. Here again it is evident that fluorine in small quantities present in

[1] It is advisable to introduce into this porcelain tube another tube of gas carbon, and to put the alumina also in carbon boats. (See the description of this apparatus in the *Annales de Chimie et de Physique*, Third Series, vol. xlvi. p. 194).

[2] This staurotide, obtained by both M. Caron and myself (*Comptes-Rendus*, vol. xlvi. p. 764), must not be confounded with natural staurotide, which, containing always a large proportion of iron, I have never succeeded in producing by this process. I have even some doubts as to the identity of their crystalline forms. By-and-bye I shall revert to this interesting question in natural philosophy.

[3] See *Annales de Chimie et de Physique*, vol. xlix. "Memoirs on Silicium and the Metallic Sesquifluorides."

[4] "On the composition of volatile substances in natural silicates." (Lithographed lessons on mineralogical analysis, given at L'Ecole Normale.)

metamorphic earths of this kind has been sufficient to produce indefinite quantities of zircon.

If, in fact, alternate layers of zirconia and quartz are placed in a porcelain tube, the first layer being of zirconia and the last of quartz, the zirconia by contact with fluoride of silicium is transformed into zircon and volatile fluoride of zirconium, which encountering the quartz converts it into zircon and fluoride of silicium, &c. Finally, the mineralisation extends throughout the porcelain tube, and exactly the same quantity of fluoride of silicium issues from as entered the tube. No portion of the fluorine becomes fixed.—*Comptes-Rendus.*

On the Adulteration of Tin Foil, by J. H. Baldock.

Having occasion, a short time back, to prepare some compounds of tin, I employed for the purpose, in the absence of granulated metal, some tin foil, and was surprised to find my product very largely contaminated with some other metal. Upon careful examination of the tin foil which I had used, I found it to contain a large amount of lead. Believing that the presence of this metal in such considerable quantity in the foil is not generally known and recognised, I obtained a number of specimens of commercial foil and submitted them to analysis. Some of these were kindly supplied to me by Mr. Haselden, of Conduit Street, the remainder being collected by myself. They consisted of,—1. Ordinary commercial foil; 2. Embossed foil; 3. Tinned paper; and 4. Foil lining the packets of Horniman's tea; together with a sample of "pure tin foil," from Mr. Saddington's, and some of Betts' patent capsules. The following table of the results of the analysis in the several cases will show the proportion of lead and tin present in the different samples. The processes employed will be subsequently described.

	I.	II.	IV.	Pure Foil.	Betts' Caps.
Lead	86·93	76·57	88·665	34·375	84·56
Tin	13·06	23·42	11·345	65·625	15·46
	99·99	99·99	100·000	100·000	100 02

It thus appears that tin foil is such only in name, though the reason of this large admixture of lead is not very apparent, because the so-called pure foil noticed above, which contains a much smaller proportion of lead, appears to be in every respect better suited to general requirements, being at once thinner, lighter, and tougher. The only cause that I can assign is, that the alloy may be more easily rolled than the pure metal. I may remark that the specific gravity of the alloy is less than would be indicated by calculation from the specific gravity of the two metals. In Gmelin's Work a table is given showing the actual and calculated specific gravities of these two metals when alloyed in different proportions.

Not long since there was a great deal said respecting the packing of tea, tobacco, snuff, &c., in lead foil, as in several instances accidents had occurred, owing, as it was then shown, to the formation of a sub-carbonate of lead, which was disseminated through the contents of the packet; and, if I recollect rightly, it was suggested that tin foil should be substituted for that made from lead. If it is true, however, that tin foil is itself so extensively adulterated with lead, it is obvious that no advantage would be gained. On the contrary, such an alloy is more easily acted upon than either of its components would be alone. Some time back I noticed on some foil, which had been in contact for some time with a packet of acid papers for Seidlitz powders, evidence of corrosion or chemical action, and the presence of lead was shown by the resulting compound blackening in the presence of HS.

For simply detecting the presence of the lead, I found the readiest method to consist in oxidising the foil with nitric acid, and to dissolve out the nitrate of lead from the insoluble peroxide of tin with hot water. Crystals of the salt were generally deposited from the solution on standing. For the quantitative estimation of the two metals, I have tried several processes, with varied degrees of success.

First, dry chlorine gas was passed over the foil, heated in a bulb tube, when the volatile bichloride of tin distilled over, leaving the fixed chloride of lead in the bulb. This process was not, however, adopted, for two reasons: 1st, because some difficulty was experienced in condensing the whole of the distillate; 2ndly, and most important, that some of the foil at the termination of the process fused and escaped the action of the gas.

Secondly, the foil was treated with nitric acid, specific gravity 1·37, whereby the tin became converted into the insoluble binoxide, and the lead into nitrate; the latter was dissolved out by water, the lead precipitated as sulphate, which was washed, dried, and calcined, and from which was calculated the amount of the metallic lead; the binoxide of tin was also washed, dried, and calcined, and the amount of metalic tin calculated from it; this process is simple, expeditious, and appears to give very good results.

Thirdly, the foil was treated with nitric acid, and evaporated nearly to dryness, with a small excess of sulphuric acid, whereby the two metals were converted into sulphates; the nearly dry residue was then washed with water, and digested repeatedly with solution of acetate of ammonia of specific gravity 1·06 or above, which dissolved out the whole of the sulphate of lead. Sulphide of ammonium was added to the solution, and the precipitated sulphide of lead washed and re-converted into sulphate by heating with a mixture of nitric and sulphuric acids, and from this the metallic lead was calculated as before: the sulphate of tin was then calcined to convert it into binoxide, from which the metallic tin was calculated. There appears to be no objection to this process, except that it is rather long, as it gives very good and accurate results; it has, moreover, the merit of simplicity. The annexed table gives at A the results of the second process upon sample No. 1; and at B the results of the third process upon the same sample:—

	A.		B.
Lead	85·52	Lead	84·84
Tin	14·12	Tin	15·06
	99·64		99·90

Both these, it will be observed, estimate the lead a little too low, arising from insufficiency of washing, some time being required to complete that part of the process.

The process, however, which I found to give me by far the best results, and to be the most convenient in execution, is one described by Rosé. It consists in fusing the alloy with an excess of a mixture of sulphur and carbonate of soda. The metals combine with the sulphur to form sulphides, while at the same time the sulphur and carbonate of soda react to form sulphide of sodium. The sulphide of tin combines with the alkaline sulphide, forming a soluble compound, while the sulphide of lead remains unaffected. The fused mass is digested with water, and filtered. The insoluble sulphide of lead is

washed, dried, and calcined with a mixture of nitric and sulphuric acids, whereby it is converted into sulphate, from which the lead is calculated as in the previous cases; to the filtrate excess of hydrochloric acid is added, which decomposes the sulphur compounds, and precipitates sulphur and the sulphide of tin; the precipitate is washed, dried, treated with nitric acid, and calcined, when binoxide of tin results, from which the metal is calculated.—*Pharmaceutical Journal.*

On the Preparation of Picramic Acid.

We are generally directed to dissolve picrate of ammonia in alcohol, saturate with ammonia, and then with sulphydric acid. These saturations are tedious and troublesome, and as picrate of ammonia is but sparingly soluble in alcohol, much of the latter is consumed, and the solutions are very bulky. The following process will be found greatly preferable:—

Picric acid (which is very soluble in strong alcohol) is dissolved in cold alcohol, and excess of sulphydrate of ammonia added. The liquid then only requires to be evaporated over the water bath, the residue to be exhausted with boiling water, filtered, and treated with acetic acid. The picramic acid obtained in this way is very pure, and the quantity large. In one experiment, were the quantities were weighed, over 63 per cent. of the weight of the picric acid consumed was obtained. If too little sulphydrate be used, picric acid remains in the mother water, from which the picramic acid crystallises, and may be recovered by precipitating with carbonate of potash.—*American Journal of Science*, No. 95.

TECHNICAL CHEMISTRY.

On the Reduction of Binotronaphthaline by Sulphuric Acid and Zinc, by M. E. Jaquemin.

But little convinced, *d priori*, of the possibility of transforming binitronaphthaline into alizarine—which would constitute a singular anomaly,—I felt I ought to repeat M. Roussin's experiment. The results I have arrived at are entirely opposed to the facts announced by M. Dumas.

I have three times treated binitronaphthaline by commercial sulphuric acid and zinc, at the temperature and for the length of time indicated by M. Roussin, carefully following the stages of the reaction, collecting separately the chief part of the colouring matter, and urging the reducing action to the highest point with the last portion.

After having diluted it with about eight times its volume of water, and then boiled it, I let it cool, and threw it on a filter. The liquid which passed, was of a beautiful violet-red colour; this could not be alizarine, as this is entirely insoluble in water charged with sulphuric acid.

By washing the precipitate with distilled water, the liquid passes coloured so long as there is acidity. The insoluble portion in the pure water, dissolved in alcohol, to which it gave a violet-red colour; now alizarine, dissolved in alcohol, takes a yellow colour. A black carbonaceous residue remained.

The new colouring matter is soluble in ether, to which it imparts a violet-red tinge; while alizarine gives it a golden yellow colour.

A small quantity of hypochlorous acid does not sensibly alter the colour of the solution; but a large quantity changes it to orange, then to yellow, and finally the colour disappears.

Potash and ammonia dissolve it, forming a purple liquid. Notwithstanding this apparently similar reaction, it is impossible to confound it with alizarine; for by adding alum to this alkaline liquid, I obtained a violet lake; while alizarine, dissolved in an alkali and treated by a salt of alum, produces a red lake.

By combining this new colouring principle with zinc, stannous, stannic, and mercuric oxides, I have produced violet lakes, with more or less blue in them.

An alcoholic solution of the colouring matter, diluted with twice its volume of water, is made opaline by acetate of lead; the addition of a few drops of carbonate of soda forms a violet-blue precipitate. Basic acetate of lead forms an opaque blue in a similar solution, from which carbonate of soda produces a clear blue-violet precipitate.

I have obtained a brown lake with ferric oxide, violet-brown with ferrous oxide, and red-brown with cupric acid.

The facts I have stated will suffice, in the absence of a closer examination, to characterise this colouring principle, and to differentiate it, not only from alizarine, but from every other colouring matter. To ensure conviction, I prepared two symmetrical specimens mordanted with alum and iron. One I dyed with madder in the ordinary way, the alum mordant in red, the iron mordant in violet; the other, on the contrary, dyed with the colour obtained by reducing binitronaphthaline, is coloured in an entirely different way,—the alum mordant became violet, and the iron mordant grey.

The violets and greys appear to be very fast on cotton; for they resist soap and concentrated acetic acid. I have not yet had time to discover what influence light has upon them. Whatever it may be, I believe this new body will be of great service in dyeing and printing stuffs.—*Comptes Rendus.*

On the Colouring Matters Derived from Naphthaline, by M. Sheuerer-Kestner.

M. Roussin's recent works, as well as a paper sent by M. du Wildes to the Chemical Society, have determined me to send the following paper to the Academy, which is a copy of a sealed packet deposited by me in the records of the Industrial Society at Mulhouse, on November 15, 1860.

The specimen of silk I exhibit was dyed with the colouring matter obtained by treating naphthylamine at 200° by dry nitrate of mercury. The matter is more or less red according to the quantity of metallic salt and degree of heat employed. The shade may thus be varied, from a slight tint of ordinary aniline violet to the colour of fuchsine. The nitrate of mercury may be replaced by bichloride of tin, and generally by the other bodies which are used to produce aniline reds and violets. These methods of preparation have been taken from the works on Organic Chemistry, in which they are partially described; for example, I quote the principal passages from these works:—

1. "Naphthylamine, when treated by concentrated nitric acid, is transformed into a brown powder, forming, when dissolved in alcohol, a red or violet liquid. Sometimes, also, it forms gold-like crystals, similar to

murexide. Nitric acid colours all sorts of naphthylamine violet."[1]

2. "When chlorine is passed into an aqueous solution of hydrochlorate of naphthylamine, it takes a violet colour and a brown resin is separated."[2]

3. "The salts of naphthylamine, treated by perchloride of iron, chloride of gold, or nitrate of silver, produce a purple precipitate, soluble in alcohol and ether, to which they impart a violet colour."[3]—*Comptes-Rendus.*

PHARMACY, TOXICOLOGY, &c.

On the Conversion of Monohydrated into Common Phosphoric Acid, by J. M. Maisch.

Since the investigations of Graham into the nature of phosphoric acid, and his important discovery of the three hydrates, which form three corresponding series of salts, no further researches appear to have been instituted. The standard works on chemistry contain scarcely anything beyond an abstract of the statements made by Graham about thirty years ago, and thus it happens that we know very little yet about the transition of one hydrate into another one. Starting with the phosphate of lime in bones and the common phosphate of soda, the pyrophosphate of soda and metaphosphoric acid have undoubtedly been often the subject of experiments. What is known, however, of the reconversion of meta and pyrophosphoric acid into the ordinary tribasic acid is contained in the following paragraph of Graham's "Elements of Chemistry":—"When solutions of the metaphosphate and pyrophosphate of water are warmed they pass gradually into the state of common phosphate, combining with an additional quantity of water; and the metaphosphate of water appears then to become at once common phosphate without passing through the intermediate state of hydration of the pyrophosphate." Otto says that on boiling the solution of metaphosphoric acid it is very rapidly converted into the common acid, without, it seems, previously forming the deutohydrate.

The glacial phosphoric acid consists chiefly of $HO\,PO_5$. To obtain from it the ordinary acid would appear to require merely to warm or boil its aqueous solution. On making the experiment, however, it will not be found so easy as supposed. The committee having charge of the final revision of the Pharmacopœia experienced this difficulty, and handed the subject over to me for investigation.

It is well known that the monobasic acid produces gelatinous precipitates in the solutions of most metallic oxides, coagulates albumen, and after neutralisation yields with nitrate of silver a white precipitate. The deutohydrate resembles the former only by precipitating silver salts white after it had been previously neutralised. The terhydrate under the same circumstance yields a yellow precipitate. For the following experiments Merck's glacial phosphoric acid was employed, and care taken to select pieces perfectly transparent and free from earths and other acids.

If some of this glacial phosphoric acid is thrown into cold water it is slowly dissolved, and the solution shows the above reactions of the monohydrated acid. Set aside at our summer temperature for two or three weeks the solution ceases to coagulate albumen, and yields now a purely yellow coloured precipitate with salts of silver. The acid has been converted into the terhydrated, and apparently without becoming first the deutohydrate; for in proportion to the decrease of the coagulation the yellow colour of the precipitate becomes more apparent. The change is not brought about suddenly, but gradually; and similar is the behaviour of the deutohydrate. Dissolved in water, it is gradually converted into the terhydrate. It is very probable that the bulk of the solution and its density—that is, the concentration of the solution—may have a marked influence on the time requisite for forming the terhydrate, as I shall show in another place, but my experiments were not extended so far.

When a strong solution of metaphosphoric acid in water is heated to the boiling point, and the boiling continued with the precaution to condense the evaporating water, or most of it, in the retort, so that the solution may at no time assume a syrupy consistence, it will be found that the bulk of the precipitate produced in a solution of albumen will gradually lessen, and after an hour or two, according to the amount operated with, will cease entirely; but now and at all times during the ebullition, the liquid, neutralised with soda or ammonia, produces a white precipitate with nitrate of silver, free from any tinge of yellow. The solution, it appears, contains pyrophosphoric acid, and is not perceptibly changed on continuing to boil for several hours more.

The syrupy liquid contained in bottles filled with glacial phosphoric acid, after having absorbed some moisture, consists at first of the mono- and deutohydrate, dissolved in water; heated to boiling, it changes very slowly into the bibasic acid. As the conversion proceeded much slower than in the former instance, experiments were made with a very dilute solution.

This diluted solution, containing about a drachm of the glacial acid in eight or ten fluid ounces of water, was heated to boiling and kept at this temperature; in about twenty minutes it had lost the property of coagulating albumen; but previous to this, the precipitate with silver was perceptibly yellow, though not quite as deep coloured as from the common phosphate of soda.

This precipitate had not become of a deeper yellow after the liquid ceased to coagulate albumen, and if the boiling was continued for several hours, little change could be perceived. It was evident that the solution contained some terhydrate and deutohydrate mixed. Experiments were now made with the same solutions, the strong, syrupy, and dilute, at temperatures between 110° and 200° F. In all instances the result was similar to that just described, with the exception that generally a longer time was required to alter the monohydrate, the lower the temperature was to which the solution was exposed. Now, too, the strong solutions afforded no evidence of the presence of the terhydrate after the monohydrate had disappeared, while the weak solution, on the contrary, contained terhydrate, as was shown by the colour of its silver precipitate.

If phosphorus is oxidised by nitric acid, the solution contains common phosphoric acid, which, after evaporation, may by heat be converted into the deuto- and monohydrate. If the monohydrate is now dissolved in diluted nitric acid and heated, it will lose its property of coagulating albumen, and will produce a yellow precipitate with silver salts, identical in colour with that obtained from the common phosphate of soda. This change in the presence of nitric acid does not appear to be unaffected by the density of the solution; but, as before, a more dilute solution is more readily converted into the tribasic acid than a concentrated one, and

[1] Liebig, *Chimie Organic*, vol. iii., p. 178.

[2] Ibid., vol. iii, p. 179.

[3] Piria, *Annales de Chimie*, vol. xxxi., p. 217.

apparently without the previous production of the bibasic acid.

At first, I supposed that the metaphosphoric acid contained some compound requiring oxidation, before the change could be effected, but a careful trial showed the absence of any nitrous acid vapours by the time the conversion was complete. It is, likewise, not the presence of another mineral acid which effects the change of the mono- into the ter-hydrate; for when boiled with hydrochloric acid, the solutions scarcely yield more of the terhydrate than is obtained by the aqueous solution alone, while pyrophosphoric acid remains mixed with it, and an addition of common phosphoric acid does not exert any influence on the solution provided it be perfectly free from nitric acid.

That the cause cannot be looked for in the higher temperature necessarily produced by the addition of nitric acid is evidenced by the facts that the more concentrated solutions of the glacial phosphoric acid yields no terhydrate by boiling, while the diluted solutions boiling at a lower temperature, yield some, and that after the addition of nitric acid, the liquid need be but heated to near its boiling point to effect the change completely though more slowly than by boiling. The density of the solution, as will be seen from the above statements, exerts a strong influence in this process; but nitric acid converts both concentrated and diluted solutions into the common phosphoric acid.

It will require more and very careful experiments to determine precisely the way in which nitric acid acts in this case. That nitric acid merely acts as a catalytic agent, seems probable, from the absence of nitrous acid vapours; but it might be possible that a compound between metaphosphoric and nitric acid is formed for a short time, and decomposed again into nitric acid and the common phosphoric acid; or the monohydrated acid might be oxidised to a still unknown oxide, PO_6 or PO_7, which is instantly decomposed into common phosphoric acid and oxygen, the latter in its nascent state uniting with NO_4 or NO_3 to form nitric acid again. Certain it is, that a small quantity of nitric acid will, with proper precaution to prevent its evaporation, change a considerable amount of metaphosphoric acid; and it acts quite or nearly as quickly upon the pyrophosphoric acid.

Graham, though he does not state so, undoubtedly operated with diluted solutions of metaphosphoric acid, by which his statement above quoted will be partly explained, and the entire conversion into the terhydrate is merely an oversight, as a partial change only takes place. It is self-evident, that if a portion is thus altered, there ought to be a possibility of altering the whole in a like manner; the difficulty appears to consist in placing the entire solution in the same favourable condition, which is the proper large amount of water, and probably a certain temperature, which, when at the boiling point, induces the change most rapidly. If the proper conditions are ascertained, we may perhaps succeed in converting the entire bulk of a solution into the terhydrated acid; but an important point must be to prevent the formation of the deutohydrate, which, it appears to me, offers under all circumstances more resistance to form the terhydrate, than the pure monohydrate. Both, however, when in solution, succumb to the influence of time and of nitric acid, assisted by heat.

Of the behaviour of the three hydrates in their free and combined state to reagents, we still know too little, though it is an important and at the same time an interesting subject for further researches.—*American Journal of Pharmacy.*

On the Behaviour of Essential Oils to Iodine and Bromine, by John M. Maisch.

The reaction of iodine with the essential oils has first been noticed and recommended as a test by Dr. Tuchen, a German apothecary; it has subsequently been studied by Winckler, Beschoener, Flashaff, Walker, and others, and particularly by G. H. Zeller, who has laid down his classical researches on the properties of the essential oils in his important work, "Studien über die ætherischen Oele." He has also first directed attention to the importance of employing certain quantities and proportions of the oil and of iodine, so as to make the examinations under conditions as nearly alike as possible. His suggestions were to place five or six drops, according to their size, of the oil in a watch crystal and add to it two grains of iodine, previously fused and rubbed into powder; the iodine is to be added at one time and right into the middle of the oil; after the reaction has ceased, the oil is stirred with a glass rod, and the nature of the mixture is noted. If oils of a thicker consistency are tested, Zeller has been in the habit to heat slightly the crystal containing the oil. In all the following experiments, I have omitted this precaution of Zeller, because, undoubtedly, the reaction between the oil and iodine is increased by the application of heat, while it appears to me essential for the detection of adulterations by cheaper oils, to be familiar with their behaviour under like circumstances and conditions.

Acting under this conviction, I have taken the precaution to make the experiments at a medium temperature, selecting the medium heat of summer, ranging from 70° to 85° F.; but few experiments were performed at a higher temperature, and they were verified or corrected at the lower one mentioned. By adopting these limits, I thought to confine the influence of heat within practical bounds, which may easily be observed in all cases without any great inconvenience.

The difference in the reaction of iodine with volatile oils suggested to me the idea of employing bromine, which, while in its chemical properties closely resembling iodine, has for this purpose the advantage of being in a liquid state, so that a more energetic and complete action was to be expected from it. This expectation was verified by experiment, but I soon found that it was so violent as to most likely exclude the possibility of noticing a difference occasioned by the presence of other oils. The behaviour of a few of the oils to pure bromine will be noticed under their respective heads; it was employed by allowing a drop of bromine to fall in the centre of five or six drops of the volatile oil, placed in a watch crystal. By the violent reaction which was shown by all, I was induced to think of a diluent for the re-agent, and found ether to answer this purpose best.

After some trial experiments, I have selected a solution of one measure of bromine in about five measures of officinal ether; with a solution of this strength the reaction does not proceed too quick, while the changes in colour and consistency may be easily observed. An ethereal solution of bromine is decomposed spontaneously; it is, therefore, best to prepare it extemporaneously. While mixing the two liquids some caution is necessary to prevent their becoming heated, as decomposition would take place, and so much of both liquids volatilised that the resulting mixture would be of uncertain strength It is, therefore, best to immerse the vial containing the ether in cool water, and for each fluid drachm of this solvent add fifteen drops of bromine to it. To five or six drops of an essential oil placed in a

watch crystal five drops of the ethereal solution of bromine are added.

In using this test, the evaporation of ether which takes place from the crystal absorbs so much heat that the mixture is kept cool, and no generation of surplus heat can be expected. All the articles used in this examination are of a volatile nature, and their evaporation is favoured by a constant change of air; it is, therefore, requisite to prevent this, and make the experiments in a place excluded from draughts.

The employment of an ethereal solution of bromine suggested the use of iodine in a state of solution; its ethereal solution appeared to have many advantages over the concentrated spirituous tincture, owing to the solubility of oils in ether, by which property a more intimate connection and a quick and sure reaction would be promoted. I have employed a concentrated ethereal tincture of iodine, which was prepared by adding to officinal ether sufficient iodine to leave some of it behind undissolved. For the same quantity of essential oils as in the above-mentioned tests, three drops of the ethereal tincture were added, in a place protected against draughts of air. The drops of this ethereal solution were larger than was anticipated; but I am not prepared to say whether their size is to be ascribed to the presence of iodine, or chiefly to the peculiar form of the lip of the vial from which they were dropped.

When testing with the ethereal solution of bromine, a peculiar phenomenon had been observed, consisting of a spreading out of the mixture, up the sides of the watch crystals. This was referred to the evaporation of the ethereal liquid, and no further notice was taken of it; but while experimenting with the ethereal tincture of iodine, and noticing the same behaviour, there was a marked difference observed in this "spreading" from the presence of some oils, and attention was directed to it. The "spreading" consists in the uniform working up on the side of the crystal, towards the circumference of a larger or smaller quantity of the mixture, with a flapping or wavy motion, in some instances up to the very edge of the vessel; and then returning to the bottom again by forming streams from the upper margin down. Some oils show but little of this spreading motion, but mix quietly with the ethereal liquid, being acted on by the iodine and exhaling the ether; others, again, while they are miscible with little disturbance, subsequently commence to spread, gradually running over the edge of the vessel, leaving but little oil behind. Sometimes the commotion of the liquids is such as to resemble a brisk effervescence or the phenomenon of boiling.

While I at first supposed it to be all owing to the evaporation of ether, I am now inclined to think that the composition of the various oils exerts its influence on this activity, while a neutral behaviour in this respect of other oils can scarcely be founded on any grounds except peculiarity in their composition. I have in this connection to draw attention to a phenomenon, which, it seems to me, may be referred to the same reason. I refer to the peculiar motion imparted by certain volatile oils to a solution of bichromate of potassa mixed with sulphuric acid, which was described by Dr. J. T. Plummer, of Richmond, Ind., in the *American Journal of Pharmacy*, vol. xxviii., p. 197.

I regret that my attention had not been drawn to the difference in this behaviour at an earlier season, so as to allow me sufficient time to report all the experiments with the ethereal solutions, with a view of observing the particularities of the various oils in this respect. My notes on this subject are, for the reasons above named, rather meagre; but I give them as they were put down at the time, reserving for future observations, to correct and enlarge them, if the matter should turn out to be really of interest and importance, in recognising the volatile oils.

Upon the behaviour of the volatile oils towards iodine various classifications have been based. Tuchen classified them into fulminating oils, in oils which dissolve iodine completely, and such which dissolve it but imperfectly. Zeller, from his careful investigations, divided them into five classes, and each of them again in various subdivisions. They are as follows:—

I. Fulminating or decomposing with detonation, with much heat, and the generation of violet and yellowish red vapours;

(*a.*) Quick and violent fulmination, with mostly violet vapours. Oleum terebinthinæ, sabinæ, juneperi, macidis;

(*b.*) Brisk but less quick and violent fulmination, with principally yellowish red vapours. Oleum neroli, bergami, limonis, aurantii, lavandulæ, spicæ, origani, vulganis, petroselini, herbæ, copaivæ.

II. Quiet and noiseless evolution of yellowish red or gray vapours, accompanied with a rise of temperature.

(*a.*) Many yellowish red vapours, considerable rise of temperature. Oleum cardamomi, melissæ, majoranæ, asari Europe;

(*b.*) Few yellowish red vapours, with perceptible heat. Oleum rosmarini, serpylli, hyssopi, anisi vulgaris;

(*c.*) Few yellowish red vapours, little heat. Oleum thymi vulgaris, salviæ, millefolii, cubebæ, cejeputi, menthæ crispæ, matricariæ, arnicæ flor, anethi, foeniculi, anisi stellati, carui;

(*d.*) Few greyish yellow vapours, little heat. Oleum calami, valerianæ;

(*e.*) Few gray vapours, little heat. Oleum nigellæ, cumini.

III. Solution without vapours, but with a rise of temperature.

(*a.*) Considerable heat. Oleum cinnamomi Ceylon;

(*b.*) Little and very little heat. Oleum cascarillæ, cydoniæ, absinthii, cinnamomi Chin, caryopylli.

IV. Solution without vapours and heat;

(*a.*) Forming a homogeneous solution. Oleum cynæ, tanaceti, menthæ piper, origani cretici, sassafras, rutæ, arnicæ radicis, petroselini seminis, sinapis;

(*b.*) Forming two strata. Oleum asphalti, ceræ, succini.

V. Partial and very sparing solution without reaction. Oleum amygdalæ amræ, rosæ, petræ.

The description of the colour as it is affected under the influence of iodine, is a matter of some difficulty; it occurs very often that in the first or a subsequent stage of the reaction, a colour is produced which strongly reminds one of the colour of an iodine solution, but at the same time so different as to be easily noticed. From the similarity of appearance it was suggested to compare them with the colour of a solution of iodine, and wherever in the following observations, the expression "iodine colour" has been used, it is understood to apply to a colour resembling that of tincture of iodine, diluted with alcohol to about four times its volume. From this explanation, other expressions, such as pale, deep, yellowish, reddish, &c., iodine colour, will be readily understood.

I have to remark yet, that the oils which I have used in these experiments, were mostly obtained from reliable sources; in some instances the commercial articles were used, in which case I took pains to examine them first in

other ways, so as to be assured of their freedom from adulterations. The physical properties of each of the oils examined by me will be briefly stated.

In proceeding to state the reactions as they belong to the various oils, I thought proper to give also the observations of Zeller, which in all cases will be distinguished by an affixed Z.

Reactions of the Volatile Oils with Bromine and Iodine.

I. The Carbohydrogens.—*Oleum Copaibæ.* Thin, colourless.

Iodine.—Faint fulmination, with yellowish red vapours and considerable heat, developed on stirring. After the reaction the residue consists of a reddish brown iodine compound, adhering like resin to the glass, and a syrupy liquid of a brown colour, changing into greenish by exposure to the air. Z.

The reaction is not violent; the sediment has a blackish brown colour with a margin of yellowish brown, not iodine colour, the oil is yellowish, the whole miscible without separating.

Ether sol. iodine.—Some spreading, mixes with little sediment and a brown yellow colour; after six hours, greenish brown, little thickened, scarcely any sediment.

Ether sol. bromine.—The reaction is accompanied by white vapours and a green colour; after the reaction the sediment is brown, the oil brownish green.

Oleum Cubebæ. — Oily consistence, limpid, a faint greenish tinge.

Iodine.—Little heat, radiating motion, few gray and reddish vapours; at first the oil is violet; after mixing, the colour is changed to yellowish brown, and the residue has the consistence of honey, and a somewhat modified odour. Z.

Brisk reaction, without fulmination; yellow vapours: the colour is blue, afterwards bluish green; after the reaction the sediment has a nearly black colour, with a bluish green margin, around which the oil is of a lemon colour, growing fainter towards the edge. The whole is miscible to a dark greenish liquid, from which the sediment subsides again, leaving the oil of a greenish lemon colour.

Ether sol. iodine.—Little spreading. The iodine colour quickly changes to yellowish and greenish brown, then greenish black. After six hours the oil is considerably thicker and of a black colour, which in very thin layers appears blackish green.

Ether sol. bromine produces white vapours and a violet colour, growing deeper in a short time; after the reaction the sediment is of a deep violet, almost black; the supernatant oil of a dark greenish blue.

Oleum juniperi baccæ.—Thin, limpid.

Iodine.—The oil fulminates quickly, with evolution of violet vapours and much heat, particularly on stirring. The residue is an oleoresinous, blackish brown mass, and some scarcely coloured oil, the whole miscible with some difficulty to a greenish brown, afterwards olive green liquid. The oil from unripe berries fulminates more violently and gives out with the iodine vapours, also many of a gray colour. With old oil the reaction is of shorter duration and shows less heat, the residue is easily miscible, and has then a reddish yellow-brown colour, and the consistence of an extract. In all cases the residue has a balsamic little modified odour. Z.

By the reaction much heat is produced and many gray, but few violet vapours; the residue consists of a dark brown resin and an olive green liquid, which after a while are miscible to a half syrupy greenish red-brown liquid; the odour is scarcely modified.

Ether sol. iodine.—Some effervescence and spreading; the oil assumes a greenish yellow colour, while an iodine coloured resin-like mass collects on the bottom, which after mixing separates again; the oil has now a light iodine colour, afterwards a brownish olive-green.

Ether sol. bromine mixes with the oil with a radiating motion; the colour is brownish yellow, with streaks of brown, which turn to a brownish purple, afterwards purplish black; the supernatant oil is now olive green.

Bromine produces a very violent detonation and many gray vapours; the residue by one drop of bromine is thin, pale yellow; by two drops, oily, dark olive green; by four drops, thick syrupy reddish brown; the odour of the first is nearly unaltered, of the others more modified, but plainly like juniper.

(To be continued.)

PHYSICAL SCIENCE.

On Platinum Standard Kilogrammes.

At a late meeting of the French Académie des Sciences, M. Regnault presented a copy of the report on the comparison made in Paris in 1859 and 1860 of numerous kilogrammes in platinum and brass with the platinum standard kilogramme in the Imperial Archives. This report is printed at Berlin:—

"The Prussian Government possessed a platinum kilogramme, which on the 24th of October, 1817, was compared by Arago and H. de Humboldt with the standard platinum kilogramme in the Archives of Paris. The Austrian Government, on its part, had a platinum kilogramme made at Paris, which on August 20, 1857, was compared by MM. Silbermann and Froment, in the presence of M. Tresca, Sub-Director of the Conservatoire Impérial des Arts et Métiers with the standard platinum kilogramme in the Archives. A comparison instituted between the two platinum kilogrammes of Berlin and Vienna showed that they differed considerably. One of them at least must have been faulty. The only way to elucidate the question was to again compare these kilogrammes with the platinum kilogramme in the Imperial Archives of Paris. At the request of the Prussian Government, the Minister of Public Instruction named a Commission, composed of MM. Regnault, Member and President of the Institute; Le Verrier, Member of the Institute and Director of the Imperial Observatory; Morin, Member of the Institute and Director of the Conservatoire des Arts et Métiers. On the other hand, the Prussian Minister of Commerce and of Public Works named M. Brix, Conseiller Intime and Director of the Central Commission of Weights and Measures of Berlin, to co-operate with the French Commissioners in again comparing the Prussian kilogramme of 1817 with the standard platinum of the Imperial Archives. M. Le Verrier was prevented by his occupations from performing his share of the work. The direction of the experiments rested then with MM. Regnault, Morin, and Brix. But we ought to mention the active and intelligent aid given by M. Silbermann, M. Tyarn, and by M. Deleuil, philosophical instrument maker. If the Commission had confined itself to re-comparing the Berlin kilogramme with the standard one of the Archives, and making the necessary correction, its work would have been less long; but on this occasion the Commission proposed to itself a longer and more difficult problem.

It proposed to study carefully all the circumstances which might influence determinations of this kind, and to find out all the circumstances which could occasion the errors found in the old stamped kilogrammes. Finally, it was proposed to ascertain whether certain phenomena revealed by the recent progress of science, and which were formerly unknown, have not vitiated the ancient determinations, and obliged us now to modify or to make more precise the first definitions."

We will pass on to the definitive conclusion of the report:—

"Our principal object was to compare the two new platinum kilogrammes verified at Paris with the platinum standard in the Archives. We have weighed them in atmospheric air, and for adequate reasons not in a vacuum. We have before shown the exactness of the corrections we made for the displaced air. Thus, for the Berlin platinum kilogramme No. 1 we have obtained the following results:—

From the weighing of 1859 . . $K'b = Ka + 0{\cdot}048$ mgr.
" " 1860 . . $K'b = Ka - 0{\cdot}364$ "

Of which the . . . . . $K'b = Ka - 0{\cdot}158$ mgr.

"For the Berlin platinum kilogramme, No. 2, lately made for the Prussian Government by M. Froment, we have found:—

$$K''b = Ka - 1 \text{ mgr. } 85.$$

"We can confidently state, then, that in a vacuum the Berlin platinum kilogramme No. 1 is too light by 0·16 mgr.; and the Berlin platinum kilogramme No. 2 is too light by 1·85 mgr."

We will now enumerate some of the most important results obtained by the Commission. The platinum kilogramme of Berlin verified by Arago and de Humboldt in 1817 was too light by 12·020 millimètres. The brass kilogramme of the College of France, too heavy by 3·846 millimètres, has not sensibly changed in weight during ten years' exposure to the air without covering in the glass case of a balance. A similar platinum kilogramme, after having been several times *in vacuo*, where it often remained for several days, did not show the slightest alteration in the weighings, nor when weighed in the air. When a platinum kilogramme is placed in each pan of a balance, and successively weighed in air more and more rarified, the changes which take place in the apparent weight correspond exactly with the weight of air displaced in each case. In other words, an abnormal condensation of the air on the surface of the two platinum kilogrammes does not produce a sensible change in their relative weights. If there exist a condensation of this nature, it produces the same effect on each kilogramme. The condensation of air on the surface of a platinum kilogramme is in no case sufficient to alter sensibly the apparent weight. Two brass kilogrammes have the same relation of weight, whether they are weighed *in vacuo* or in the air, and the apparent weight is corrected by the weight of air displaced, calculated by the ordinary principles of physics. Under the conditions in which the Commission operated, glass produced no abnormal condensation of air nor humidity on its surface. The surfaces of platinum plates did not condense a quantity of hydrogen appreciable by the most sensitive balance; or, not to go farther than is warranted by experience, the condensation, if it exist, is exactly the same under a pressure of 6·4 millimètres as under the ordinary pressure of the atmosphere.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Six Lectures on Light[1] *(adapted to a Juvenile Auditory), by* JOHN TYNDALL, *Esq., F.R.S., Professor of Natural Philosophy in the Royal Institution.*

LECTURE IV. (Jan. 2, 1862.)

NOTES TO THE LECTURE:—

The white light of the sun is made up of an infinite number of rays of different refrangibilities—Each particular refrangibility corresponds to a particular colour; hence the number of colours involved in solar light is infinite—But for convenience sake we divide these colours into seven, which are called primary colours. These are red, orange, yellow, green, blue, indigo, violet—Of these colours the red is the least refrangible, and the violet the most refrangible; the other colours being intermediate between these two—The solar beam is resolved into these colours by passing it through a prism: the coloured image thus formed is called the *solar spectrum*—The colours of the spectrum, when suitably blended, produce white light—A colourless image of the coal points o the electric light may be built up from the colours of its spectrum.

Some substances have the power of drawing the colours more widely apart than others; glass, for example, does this more effectually than water, and bisulphide of carbon more effectually than glass—The drawing asunder of the colours by a prism is called *dispersion*; thus, the greater the distance between the red and violet ends of the spectrum, the greater is the dispersion.

When sunlight falls upon a body, a portion of *white* light is reflected from the surface of the body—A second portion is reflected after it has entered the body to a greater or less depth—It is this latter portion which gives the body its colour—Different bodies have the power of absorbing, or quenching within them, different kinds of light—A red body is red because it has the power of quenching all rays except those which compose its red. A blue body is blue because it has the power of quenching all rays except those which compose its blue—Brilliant crimson feathers, for example, if illuminated by pure blue light, are as black as those of a raven;—conversely, a pure blue would be black if illuminated by red light—When the human face is illuminated by a flame which contains no red rays, the lips and cheeks lose all their redness; in the same light red and crimson flowers lose their bloom—In fact, the colours of bodies are entirely due to the light which falls upon them. If the white light of the sun were simple instead of compound, we should have only light and shade in the world; but we should have no colour.

A metal heated to whiteness gives a *continuous spectrum* as long as the metal remains in the solid or liquid condition—But when a metal has been reduced to vapour, and when that vapour is rendered luminous by intense heat, the spectrum of the vapour is usually composed of brilliant bands—Every metal has its own distinct system of bands—When metals are mixed together so as to form alloys, the bands of each metal are produced in the spectrum of the alloy—Thus, knowing the bands that each separate metal produces, we can determine from the spectrum of an alloy the metals of which it is composed—The bands of the metal also exhibit themselves when the salts of the metal are raised to a sufficiently intense temperature.

A luminous vapour absorbs those rays which it can itself emit—Thus the light of incandescent sodium vapour is intensely yellow; but when a beam from the electric lamp is sent through this vapour, the yellow rays of the electric light are intercepted—The sun is supposed to be composed of a solid or liquid central portion, which of itself would give a continuous spectrum—But this nucleus is surrounded by a flaming atmosphere, through which the rays from the nucleus have to pass—This solar atmosphere, or photosphere, as it is often called, intercepts those rays of the nucleus which it can itself emit, and hence the solar spectrum is always furrowed by dark lines (*Fraunhofer's lines*)—From these lines we can determine the metals which produce them; and in this way it has been found that many of the terrestrial metals are present in the sun.

With regard to the duration of the impression upon the retina, there is a little experiment which all my pupils present can make for themselves, and which is a very pretty one. It consists simply in taking a knitting needle, sticking a little silvered bead on the top of it by means of marine glue or sealing wax, and fastening the other end of it firm, and then striking it so as to cause it to vibrate. When you allow the sunlight, or even the light of a lamp or candle, to fall upon the bead, you see upon striking it, the needle vibrates, and the bead performs certain excursions, and goes on describing the most beautiful figures.

This I say is an experiment that each can make for himself, it is extremely pretty and well worthy of your attention.

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[1] Reported verbatim by special permission.

Now, let me show you what may be done in this way. We have here such a piece of vibrating rod, it is not exactly a knitting needle, but something stronger and better made. I will illuminate this bead by a beam from the electric lamp and project an image of it upon the screen. You see the little spot of light upon the screen; I will now bring that quite to a point, and if I cause this to vibrate, you will see what beautiful figures I get. Here you see we get a curious figure of 8, which denotes the kind of vibration of which this rod is capable. You can also get figures far more beautiful than that. If I touch it with the fiddle-bow, I shall produce a figure of 8 with a fine crimped outline. Now instead of this vibrating bead I will take simply a knitting needle, and introduce it into the beam of light as before. We will alter our lens so as to bring it in focus to a little point, and cause it to shine brightly upon the screen. Then I touch that, and you have a beautiful circle of light. See what a lovely figure you have when I touch this knitting needle with the fiddle-bow. These experiments, and numberless others, you can all make for yourselves, and they are really well worthy of your attention.

Now, a number of interesting and ingenious toys depend for their beauty upon this principle of the persistence of image upon the retina of the eye: and among the number is the chromatrope. I will just show you one that will suffice. Here we have a specimen, and we will throw our fine disc of almost solar light in intensity, upon the screen, and then bring the chromotrope to a perfect focus, so that the definition may be perfectly sharp. If I now turn the winch, you see the curious appearance of motion produced, and if I turn it the other way the motion you see appears to take place in the other direction—all these movements depending for their effect upon the power of the eye to retain an impression for a certain definite period of time.

I want next to draw your attention to another subject mentioned in our last programme (for I wish to make clean work of our programmes, and not desert the memoranda until we completely finish them). I mentioned there a peculiar effect upon the eye which I have called *irradiation*. It is a learned word, but it is very well that you should understand such, because you will frequently meet with these terms when you read, as I trust you will read, books on Natural Philosophy. You know that when I look at any of you, as we explained in our last lecture, there is an image painted on the back of my eye. The friend whom I see before me is, at the present time, depicted with perfect distinctness at the back of my eye, turned, with his head downwards. At the present time he is moderately illuminated; but suppose, instead of the mild light upon a little boy's face illuminated by the lamps that light this theatre—suppose him intensely illuminated—suppose his face to shine with the brilliancy of the sun, or very nearly so, then, in virtue of the intensity of the light, the image of his face on my retina would appear a little too large and would encroach upon the circumjacent space, and I should see his face larger than it really is. I should also see it so much the larger the farther I went away from it. I have here drawn two rings, and I would ask you to direct your attention at the present time to them. The black line is exactly the same thickness as the white one. I think those at the distance if they were asked which of these lines were the thickest, even with the present illumination, would be inclined to give the preference to the white lines. But, in reality, the one is not a bit thicker than the other. I will ask Mr. Anderson to hold the diagram in the light, and I think you will see that the white line is apparently the thicker of the two. Those at a distance will see the effect of this more clearly than those near at hand; and if Mr. Anderson now partly withdraws that white ring from the light and lets one side be illuminated and the other not, I think you will be able to perceive the difference in the thickness of the ring itself; the portion illuminated will appear perceptibly thicker than the portion not illuminated. This explains the effect I have spoken

of, viz. that the farther you go away from the object the greater is the amount of irradiation. Hence it is that, looking at the new moon, which is a body 240,000 miles from the earth, you see it encircles the dusky portion of the moon and appears to belong to a larger sphere altogether. This effect can be illustrated in another way. You see this fine platinum wire stretched from one end of this instrument the other. When I connect it with the galvanic battery, the mysterious power which rushes through it from the battery down stairs will heat the wire to a very high degree of incandescence, and make it white with heat; and then, I think, those at a distance will observe that the wire appears to augment in thickness, that is, its apparent thickness will be much greater than at present.

The wire being now heated, appears to me to be much thicker than it was formerly, and I dare say to those at a distance the difference will be more apparent. If I brighten that wire, you will find it will appear still thicker. I can do so by sending a more powerful current through the wire. This I shall be enabled to do by shortening the wire, and thus lessening the resistance which it opposes to the passage of the current. You see how intensely it is illuminated now, and I have no doubt it appears thicker than in the former experiment. I think the wire will bear yet a little more heating. I will shorten it a little more, and then I will diminish the light of the wire by interposing a coloured glass. [In this experiment the heat was so intense that the wire fused and parted.] The heat has fused my wire, but I will try again. I want to show you the effect of darkening by cutting off a portion of the light of this wire.

You will have the wire bright as before until I interpose this piece of coloured glass. Look through the glass and at the wire, and compare that portion of the wire which has the light cut off with the rest, and you will see the difference. If I were to take a still darker piece of glass, the wire would appear much thinner, exactly as the size of the moon, looked at through a dark glass, on the Alps, becomes apparently less.

So much, then, for this question of irradiation. Now we come to the main subject of the day's lecture, and I will go on building up my argument by facts. I will project, first of all, a slice of light from the lamp, upon the screen, by means of this lens. Now, would you suspect that there was within that image—white, and beautiful and colourless as you see it there—would you suspect that, in that white image, you would have the most splendid red, the most vivid orange, the most burning yellow, I was going to say,—and green, blue, indigo, and violet,—

all mixed up in that space of white. So that that light consists, not of a simple thing, but of the blending of all these colours—colours so intense, so beautiful, that no painter could imitate them.

I will now take a little prism of glass and refract this beam of light upwards; you now see the image is refracted, and goes up as you remember it did in our former experiments. But if you look closely at that image you will notice a little colour—a little red at the bottom, and blue at the top. Now, I will take a larger and more powerful prism, and will cause the light to pass through it, so that it will be refracted upwards, and I think this powerful prism will be sufficient to cast the beam high up upon the wall, and you will then find this white light reduced to its coloured components. This is the grand discovery of the great Newton—for it was Sir Isaac Newton who discovered that the white light that you see is produced by the blending together of these splendid and beautiful colours.

I want now to carry you through the proofs and arguments and considerations which depend upon this point. First, I will try and produce the self-same coloured image, or *spectrum* as it is called, by deflecting a ray of light sideways instead of upwards. I will place my slit vertically, thus, and if I make my coal points touch, a beam of light will pass through. I interpose my lens, and place the prism behind the lens and turn it so that the beam of light shall strike the prism. There, you have this beautiful image upon the screen—perfectly sharp and brilliant. I will now see whether I cannot actually squeeze those colours together, and mix them up so as to destroy that coloured image that you see. I place a lens behind the prism and I will see whether or not by means of this lens I can actually recompose the light—reblend it—remix it so as to produce the original white image. There, you see the light is so reblended that you have the various colours in a great measure destroyed. Supposing, now, I cut off with a screen the red which is on this side, what do I leave? You see the blue coming in on the other side; and so you see when I take a prism of this kind I can actually separate one image from the other. Now, I will try to deflect the red beam away. I can do it by partly interposing this small prism which you have had already. Here, then, you see the red is pulled away and now stands beside the blue; and again, if I deflect the blue and throw it to the other side, you see the red start out. These two colours which I thus separate when blended together produce white light. They are called the complementary colours.

Now, let me see whether it is not possible, from this coloured image, to actually take the light of which it is composed, and to rebuild from it the very coal points from which the light issues. Let me see whether I cannot do that. These rays of light are, so to speak, the rough bricks and mortar of which the coal points are built. Here, then we have the light which composed the image, and now we have recomposed the coal points from which it issued.

Let me now make an experiment of Newton's, for you to see the way in which he satisfied himself that the blending of these colours produced white light. He took a circular piece of paper, or paste-board, and he divided this circle into various parts, and coloured those various parts with the colours of the spectrum—one part red, another blue, another orange, and so forth, and then he set in it motion. He knew very well that by setting it in motion, because every colour remained on the retina during the time of the revolution of the circle, he actually threw all the colours together into the eye, and re-blended them. And in that way, when he set the card in motion, he produced a white disc. Now I have here such a circle, not as Newton made it, but of glass painted with transparent colours, and I project the image of this upon the screen, when you see that by turning this handle the colours completely vanish, because all the colours are thrown simultaneously into the eye, and, being blended there, produce the impression of white light.

Now this must satisfy you that the white light that comes to us from the electric light, and also from the sun, is composed of rays of a variety of colours. Some of these rays are more capable of being bent than others, and that is the reason why we are able to separate them. The blue is more so than the red, and the consequence is that, when we send them both through the prism, the one is separated from the other.

I will now make an experiment with a prism of another kind. We have here a hollow glass prism with the sides cut and polished, and in this vessel is placed the liquid, bisulphide of carbon, that you see mentioned in the memoranda of this day's Lecture. This gives me a more richly-coloured spectrum than the glass. Now I will place this bisulphide of carbon prism on the stand. The light will come through this lens, and strike upon the surface of the prism; and it will pull the blue aside more than the red, and thus will separate the colours better. Here, you see, the colours of the spectrum are richer than those produced in my last experiment.

Now, bear in mind that the beam of light, before refraction, would go on in a perfectly straight line. It is pulled aside by the prism, and you see the blue is pulled more aside than the red, and is, therefore, said to be the more refrangible colour, and the red is the less refrangible colour. Newton divided this bluish portion of the spectrum into three kinds; he called the first, that is the portion next the green, blue; the next indigo, and the extremity of the spectrum he called violet. Thus we have the seven primary colours: red, orange, yellow, green, blue, indigo and violet. I want now still more to augment the dispersion—remember that word; it means the pulling aside the colours of the beam. The amount of dispersion is expressed by the length, from the extreme red to the extreme violet: the greater its length, the greater is said to be the dispersion. Having now sent a beam through one prism, I will catch that coloured beam as it issues from this prism, and send it through another, and thus I can pull it round still further. In that way I expect to get a spectrum that will stretch almost across the entire screen. I can twist that beam round so as to bring it upon the screen with a greatly augmented dispersion. There you see you have this glorious exhibition of colour actually stretched almost across the entire screen. Now you would call no portion of this image white, the paper has actually become coloured in virtue of the light falling upon it—the light has painted it. In point of fact all the colours that we see in Nature are produced in this way. Colours are not inherent in the bodies themselves at all. It is the light which falls upon them—which is in fact the paint—which gives beauty to every colour. However, I shall develope this subject more clearly as we go along. Now here at this end you see we have an intense beam of red light. I will let it fall upon a red object, and you will see that the object will appear red in it. I will show you this bunch of artificial flowers which are red, with green leaves: when I allow the red light to fall upon these flowers I think you will see that they are red, and you saw that they were red when the white light of the gas fell upon them. Now this is a very important point which I wish you to perfectly understand. Why are these things red when the white light of the gas falls upon them? Simply because the cloth, or whatever it may be, or the colouring matter used in its manufacture—the dye of the cloth—has the power of completely quenching, absorbing, drinking in, destroying the blue, green, and yellow—all these are destroyed, and the only light which it is capable of sending back is the red. If what I say be true, if I pass these flowers into another coloured light, you will find that it has no power of showing itself red, or any other colour. If I put

it into one of those colours that I say it is capable of quenching, it will become black. Now observe. Here in the red ray we have the red very beautiful indeed, but the green is made quite black. Now I move the flowers into the green portion—what are they now? Black. And here, again [moving the flowers into the red light], they are a beautiful bright red. The green light is much more intense to look at than the red light, but this substance is not capable of giving back the green light: it is only capable of giving back the red light. The consequence is, that when the green light falls upon it its colour is destroyed—we have darkness instead of light. Here we have three sticks of sealing-wax, beautifully red. What are they now? [Placing them in the green part of the spectrum.] Look at them now: the light shining on them is very intense, but the sealing-wax is quite black.

Now, with regard to the influence of this coloured light upon the human countenance, I wish I could get some boy, in all the bloom of rosy youth, to let me try the influence of the light upon his cheeks. You would see how he would look in this green light. I wonder whether any of my pupils will volunteer. However, in the absence of a volunteer, I will take Mr. Anderson; he has a tolerably fair colour, you know. He will stand here, and I will illuminate his face. I dare say I shall succeed in making one cheek appear full of the bloom of youth, and in reducing the other to a state of almost death-like paleness. Let us see if that is not possible. [Mr. Anderson then walked slowly along the coloured rays of the spectrum.] There, you see his face is full of radiant bloom. As he passes along, you will find one side of his face will assume a very cadaverous aspect. He will go back again so as to show you the effect produced: by-and-by his whiskers will become quite radient—now he is in the midst of the red ray—there, he is in almost the full splendour of boyhood.

Now, I want to show you some of the spectra produced by the combustion of various kinds of metals. Hitherto, as you have observed, we have been dealing with a spectrum or coloured image, that was quite continuous from beginning to end, commencing with the red, and passing through various gradations to the extreme violet; it was continuous, there was no breach of the spectrum. I want now to show you some spectra where there are the most singular breaches of continuity, and to do this I operate thus:—You know the energy of this electric current we use for our light; you know its power to render metal white-hot, and even to dissipate it, and to reduce it to a state of vapour. Here I have a small piece of carbon, and on the top of that I have scooped a little hole, and into that hole I will put a little bit of metal. The metal I take is cadmium, well known to chemists. When I bring this little crucible close to our coal point and send the current through it, the metal will become intensely heated; it will, in fact, be reduced to a state of vapour. In the first place, I want to show you the light produced by this cadmium. I will take away the ordinary coal point which I have used for the light and place in its stead this crucible of carbon. I will then place within it this bit of cadmium, and bring down the upper coal point to meet it. I want you to observe the beautiful coloured light produced by this cadmium. You have already seen the image of the coal points that we have worked with throughout the lectures; I want to show you the distinction between this image of the coal points and the image produced when we introduce a metal into the electric current. You see the splendid blue light produced by the combustion of the metal; it is entirely different from the voltaic arc we have been hitherto operating with. Instead of this cadmium I will take a little copper, and then a beautiful green light will be produced, due to the vaporisation,—the reduction of the copper to a state of utter vapour. We may take gold or silver and produce similar effects. The green light issuing from the copper is most beautifully shown passing along the smoke of the room. Now, I want to examine that light which is thus given out by the copper,—that is, I want to produce a spectrum of that light. And when I do so you will find that this spectrum, instead of being continuous, as in the former case, now consists of a series of brilliant bands of light, and in particular you will notice the very brilliant green bands produced by it. There you see the spectrum produced by the combustion of the copper, and the beautiful green bands we have already noticed, with the dark spaces between. I will now take another metal instead of copper. I will place a bit of zinc upon the crucible, and just be kind enough to bear in mind the kind of bands you saw with the copper; they were chiefly green, while those that you now see produced by the zinc are of a brilliant blue. You see the spectrum covered with these blue bands. Now, I will take a substance formed by the mixture of zinc and copper, that is, brass, and in the spectrum produced by the combustion of that brass I trust you will be able to see the bands of both the metals of which it is composed. You remember that copper gave a series of green bands, and zinc a series of blue bands, and I hope in the spectrum of the brass, if we are skilful enough to make the experiment aright, we shall see both the green of the copper and the blue of the zinc. In this way, knowing the character of the spectrum, we can tell the metals of which it is composed. Now, I want to show you another very beautiful experiment before we part. In the first place, I will show you the light produced by the combustion of a little sodium. I have here a portion of this metal, which I can cut with a knife, like cheese. I have here a lamp, giving a strong heat, but very little light, and I will take a portion of this sodium and put it upon a little platinum cup and hold it in the flame of this lamp, and then I think you will see that the sodium has the effect of giving a very intense yellow light indeed. Let us see whether we can produce this. At the best of times I have not much colour, but what little I have will certainly disappear if I cause this light to become more intense. That light does not only appear yellow, but it has actually no red in it at all. Look at the red sealing-wax, and at that bottle; that bottle contains iodide of mercury, which is of a brilliant red colour, but you see the red is now completely gone.

I want, now, to make a very peculiar experiment. I want to show you that that very yellow light which you saw there is capable of cutting off the yellow light that emanates from the electric lamp. In order to do that, I must first of all produce a continuous spectrum, such as we had before. I want to get it very exact, for the experiment is an important one and worthy of some labour. The experiment I am going to make is this. You saw that beam issuing from the lamp, and the spectrum produced from that beam upon the screen. I will send that spectrum through the sodium flame, and you will find that that flame will be capable of chiselling or cutting out with the greatest nicety the yellow portion of the spectrum. I want you particularly to observe the action of this piece of sodium upon the flame. I want you to observe the yellow part of that spectrum when I introduce this metal. A certain duskiness will come over the yellow portion there. Do you see how it cuts out the yellow of the spectrum? Here, in the platinum spoon, I have a quantity of soda produced by the combustion of the sodium. I will pour this soda upon one of the coal points, and you will then see that it produces a yellow line exactly where it formerly cut it out. Thus, the sodium flame is capable of absorbing those particular rays of light which itself can emit.

I do not know that any more time remains to me to make experiments, but I will just carry you, in conclusion, to consider what has been done by experiments of this kind. When we take, instead of the electric light, the light of the sun, there are certain lines always

observed in its spectrum. These lines have been compared with the utmost exactitude with the bands produced in the spectra of various terrestrial metals, and have been found exactly to coincide with them. In this way the actual composition of the sun has been proved by a very able man, a German. It is a wonderful thing to see the human mind, as it were, rising up to the sun itself, and being able to tell us, from the very lines that you see in the solar spectrum, the metals that are present in the sun. And not only so, but the experiments have led us to several conclusions regarding the nature of the sun; and we now know, which is very different from the conclusion adopted formerly, that the sun consists of a vast central solid or liquid sphere, in a state of high incandescence,—burning hot. Round this sphere there is a great atmosphere, like a vast flame; the rays of the central solid or liquid portion of the sun have to pass through this fiery flame, and in that atmosphere certain of the rays are quenched and destroyed, exactly as the flame of the sodium quenches or destroys the yellow band of the electric light. Thus, from the lines in the solar spectrum, we can infer that iron, magnesium, and a great many other metals which are well known upon the earth are present in the sun.

NOTICES OF PATENTS.

895. *Sizeing or Preparing Paper and Textile Fabrics in order to render them Waterproof, and to increase the strength thereof.* R. A. Brooman, Fleet Street, London. A Communication. Dated April 11, 1861.

For carrying out the objects specified in the title, the inventor claims the use of a mixture of carbonate of soda, lime, alum, and gamboge, which ingredients may either be incorporated with the pulp, or used subsequently to the manufacture of the paper.

The combination of gamboge and alumina is brownish yellow in colour, and must therefore communicate a somewhat dark tinge to the paper stuff.

911. *Improvements relating to Ornamental Cotton Fabrics, having Turkey Red Grounds.* G. Graham, Dumbarton, N.B. Dated April 13, 1861.

The mode of proceeding described in the specification, is substantially as follows:—The goods, after dyeing uniformly with turkey red, are subjected to pressure between lead plates, from which the required device is cut out. Thus partially protected, the red portions are bleached by exposure to chloride of lime and sulphuric acid. This having been effected, the material is well washed by a current of pure water passing through it, and then a mordant applied to the bleached surfaces, either wholly or partially, according to whether it is desired to leave some parts of the fabric white. The mordant may be any one of those commonly employed, but the inventor recommends the use of a mixture of acetate of lead and bichromate of potash, of a strength of about two and a-half degrees Twaddell. Logwood liquor, of the same gravity, is finally passed through the goods to produce a black dye.

It is manifest that numerous combinations of turkey red, white, yellow, and black may be obtained according to the mode of employing the series of perforated lead plates.

The second part of the invention relates to the production of continuous patterns, by the employment of blocks or rollers.

929. *Manufacturing Silicate of Lime, or Hydraulic Cement.* F. M. Eden, Hare Court, Temple, London. Dated April 16, 1861. (Not proceeded with.)

For the preparation of a cement, (which is stated to be chemically identical with Portland cement,) the inventor directs an intimate mixture to be made of chalk and clay. Carbonate of lime, or the same burnt to lime, may be employed indifferently. This mixture is then dried and burnt upon hot tiles or plates, or in suitable ovens or kilns, and the product (called silicate of lime,) ground to an impalpable powder.

Portland cement is usually prepared according to a process very similar to that under consideration, the mud from the river Thames being taken in preference to more compact clays. If chemically identical with the product of ordinary manufacture, the cement must contain a large proportion of alumina, derived from the clay, and would, consequently, be more correctly described by including the two bases in the expression, and calling it a silicate of alumina and lime.

927. *Obtaining Light.* F. Gye, Wandsworth Road. Dated April 15, 1861.

In the arrangement of apparatus for the production of the lime light, the patentee claims the application of discs or plates of lime, instead of the usual balls or cylinders, and the imparting rotation to the same by means of a new combination of mechanism.

932. *Improvements in the Manufacture of Nitric Acid and Caustic Soda.* J. D. Malcolm, Brixton, Surrey. Dated April 16, 1861. (Not proceeded with.)

The inventor mixes intimately one part, by weight, of nitrate of soda with three parts of fine clean sea sand, introduces the mixture into an earthen retort, and proceeds to distil as in the ordinary process of preparing nitric acid with the aid of oil of vitriol. The red nitric fumes are absorbed by water contained in a series of stoneware Woulfe's bottles, and the soda is said to remain in the retort partly in combination with silica, but principally in the form of caustic soda, which may now be recovered by washing the residual matter (sand and alkali), and evaporating the aqueous solution in order to procure caustic soda; the sand left insoluble may be dried and used in a repetition of the process.

A question of greater importance than the economisation of the sand has reference to the durability of the retorts themselves under the corrosive action of so powerful an alkali, the destructive influence of which would even be materially assisted by the elevated temperature required for the distillation under these circumstances. The distillation of saltpetre with sand and clay was precisely that process which led to the first discovery of aqua fortis. It has not, however, of late years been resorted to for the manufacture of nitric acid on account of the partial decomposition of the product at the high temperature at which it is liberated. As an illustration of the antiquity of this method, we quote from Bishop Watson's Essays,[1] published in 1789,—"It is certain that the nitrous acid may be disengaged without the assistance of the vitriolic acid: thus I remember having many years ago obtained a very strong fuming acid of nitre by distilling nitre with white sand."

Grants of Provisional Protection for Six Months.

2917. Francis Puls, Francis Terrace, Hackney Wick, London, "Improvements in treating fatty and oily matters."—Petition recorded November 20, 1861.

3027. Andre Marie Augustin Pichery and Pierre Louis Danais, Nantes, Loire Inférieure, France, "Improvements in hermetically stoppering or covering jars, pots, vases, and other like articles."—Petition recorded December 3, 1861.

3076. Balthasar Wilhelm Gerland, Newton-le-Willows, Lancashire, "Improvements in the manufacture of sulphate of copper and other salts of the same metal."—Petitions recorded December 7, 1861.

[1] Chemical Essays. Vol. I. p. 253.

3142. Eduard Claude Barbotte de Beaulieu, Avallon, Yonne, France, "Improvements in apparatus for extracting gold dust from auriferous sands."

3150. Emile Cajot, St. Servais, Belgium, "Improvements in the treatment of pyrites for the manufacture of iron."—Petitions recorded November 14, 1861.

3195. Vasco D'Almeida, Nottingham-street, Marylebone, London, "An improved mode of obtaining colouring matter applicable to dyeing skins, silk, wool, and other fibrous materials."—A communication from Edward Smith, Place Cruzquebrada, Lisbon.

3225. Francois Laurent and John Casthelaz, Rue St. Croix de la Bretonnerie, Paris, "Improvements in the manufacture of colouring matters."

3232. Joseph Schloss, Cannon Street, London, "Improvements in envelopes for containing photographic portraits and pictures."—A communication from Simeon Schloss, Paris.

3257. William Edward Newton, Chancery Lane, London, "Improvements in the manufacture of cube sugar."—A communication from Gustavus Finken, Brooklyn, King's County, New York, U.S.

3259. Alexandre Isaac Austen, Millwall, Middlesex, "Improvements in the manufacture of night-lights."—Petitions recorded December 30, 1861.

CORRESPONDENCE.

Manufacture of White Lead.

To the Editor of the Chemical News.

Sir,—If your correspondent "Subscriber," in your last Number, will favour us with his address, we shall be happy to supply him with white lead (the pure carbonate), either in a dry state or ground in linseed oil, perfectly fine and free from the defects he mentions.

We are, &c.

W. W. and R. Johnson and Sons.

White Lead Works, Limehouse, London.

MISCELLANEOUS.

Chemical Society.—The next Meeting of this Society will take place on Thursday, the 6th inst., when the following Papers will be read:—Mr. Adie, "On Ground Ice." Dr. Bence Jones, "On Crystalline Xanthin in Human Urine." Mr. A. H. Church, "On Silica." Professor Bloxam, "On Arsenic in Sulphuric Acid."

Royal Institution.—The following Lectures will be delivered in the ensuing week:—Monday, February 3, at two o'clock, General Monthly Meeting. Tuesday, February 4, at three o'clock, John Marshall, Esq., "On the Physiology of the Senses." Thursday, February 6, at three o'clock, Professor Tyndall, "On Heat." Friday, February 7, at eight o'clock, T. H. Huxley, Esq., "On Fossil Remains of Man." Saturday, February 8, at three o'clock, Rev. A. J. D'Orsey, "On the English Language."

Adulteration of Food.—Dr. Letheby, the Medical Officer to the City Commission of Sewers, states in his quarterly report that no application has been made to him during the quarter for the analysis of food or drink, and he adds that "it is to be feared, from the experience of the last six months, that the Adulteration of Food Act will become a dead letter."

Science a Civilizer.—Dr. J. Bullar at a recent meeting of the Southampton Microscopical Society, said:—"The social aspect of our Society commends it. It is a pleasant way of spending an evening where there is a scientific object of natural interest, and, at the same time, a social gathering of many having the same tastes and objects, and, therefore, the same sympathies. The anatomy of an insect, too, is a more harmless occupation than the minute dissection of a neighbour's natural history. Tea and coffee, pleasant chat with those of like tastes, and then the table covered with microscopes and specimens explained by one and passed round for each to examine, calling out animated talk on subjects worth discussing, or a short paper read and discussed on the subject illustrated, are civilizing. For science is a civilizer. It refines the tastes and elevates the thoughts, as it is the search after truth for truth's own sake. And in this age, when the progress of the nation and of the world is estimated by the money-value of exports and imports (and in this aspect the world's progress is prodigious and annually increasing), the danger must lie in estimating all things in reference to money rather than to truth. Now, science is a counteracting force. It neither brings wealth to its true cultivators, nor can wealth buy scientific tastes or scientific fame. It belongs to a higher region than 'the diggings.' It must breathe 'a purer ether, a diviner air.' And those who are engrossed in commerce would often do well, for their own content and happiness, by seeking in the recreations of science a complete change of action, thought, and feeling. Obviously the eye service which the microscope requires, trains the eye to minute and discriminative observation, and the hand to delicate accuracy. It leads on, if used scientifically, to the improvement of the scientific powers. The memory, the investigation of causes, the estimation of evidence, the power of distinguishing and of generalizing may be called into activity. But the mind has other and deeper needs than these. The senses lead to the awakening and culture of deeper powers inherent in the soul itself, and the microscope may excite and cultivate, not only the sense of the true, but of the beautiful. Constable, the landscape-painter, said that, pictorially, nothing in nature was ugly; and surely we may say the same microscopically. The higher the magnifying powers, the more minutely extensive the investigations, the more beauty do we see. Even in the unhealthy secretions—in what look to the unscientific eye like repulsive fluids, in the very disorganisations which slowly ruin this goodly human frame, the microscope discovers forms of the highest geometrical accuracy, as well as of the most delicate beauty. And this beauty and consummate finish are everywhere, and are found farther and deeper as our powers increase of observing them. Here, too, at every step we find the limitation of our own powers, and the illimitable field of nature; the infinite contrasting with the finite, teaching us 'the moral lesson of science—humility.'"

ANSWERS TO CORRESPONDENTS.

*** All *Editorial Communications* are to be addressed to the Editor; and *Advertisements* and *Business Communications* to the Publisher at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

Vol. IV. of the Chemical News, containing a copious Index, is now ready, price 12s., by post, 12s. 8d., handsomely bound in cloth, gold-lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. A few copies of Vols. I. II. and III. can still be had. Vol. V. commenced on January 4, 1862, and will be complete in 26 numbers.

American Journal of Pharmacy.—We have regularly forwarded our exchange copy and much regret that it has not arrived. Enquiries shall at once be made on the subject. Far from declining to exchange, there are few periodicals which we receive with so much pleasure as our Transatlantic contemporary.

F. W. Joy.—The matter has been referred to our publisher, who will attend to it.

J. Horsley.—The controversy had better cease.

Subscriber.—We believe the process to be impracticable. If not, it would, however, be terribly expensive.

Erratum.—Page 52, line 7, from bottom, *for* "chloroform" *read* "chlorine."

THE CHEMICAL NEWS.

VOL. V. No. 114.—February 8, 1862.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Nascent Oxygen Tests for Aniline, by H. LETHEBY, *M.B., M.A., Ph.D., &c., Professor of Chemistry and Toxicology in the London Hospital College.*

IN the course of the last twelve months I have had to investigate, on behalf of the Coroner, two cases of accidental poisoning by nitro-benzole; and finding that this compound was changed in the animal body into aniline, my attention was directed to the most delicate means for discovering the presence of that alkali. At a future time I will seek an opportunity of describing the process for extracting the aniline from the stomach, tissues, and urine; but having procured it, I found that nascent oxygen, from whatever source it was obtained, gave to aniline a rich blue, violet, or red colour, according to the proportion of oxygen acting and the strength of the acid solution containing the alkali. The best means of applying the test is by the agency of the galvanic battery. If a single drop, or even half a drop, of a solution of 1 part of aniline in 1000 of dilute sulphuric acid (1 of acid to 7 of water) is placed on a clean piece of platinum, and touched with the negative pole of the galvanic battery (a single cell of Groves' arrangement) while the positive pole is in contact with the platinum, the liquid acquires a bluish, then a violet, and finally a pink colour. If the sulphatic solution of aniline is stronger, or the drop is concentrated on the platinum foil by evaporation, the colour is of a deeper blue, and by using the $\frac{1}{100}$th of a grain of aniline with a drop of dilute sulphuric acid, consisting of equal parts of acid and water, the galvanic test gives a rich blue or purple blue reaction of considerable intensity.

This result is very much like that which I have described as the galvanic test for strychnia,[1] but the two alkaloids are distinguished, not merely by the volatility of aniline and its appearing in a medico-legal inquiry in the distillate from the suspected matters, but also by the circumstance that while strychnia requires the concentrated acid to show its violet colouration on platinum, aniline is best seen with the dilute acid.

Other sources of nascent oxygen may likewise be made available as a means of discovering the presence of aniline, but they are not all equally sensitive. The $\frac{1}{1000}$th of a grain of aniline in half a drop of dilute sulphuric acid (1 to 1) on a white plate will gradually become blue if it be treated with a little peroxide of lead or red prussiate of potash; but it requires twice this quantity of aniline to show the tint with peroxide of manganese or bichromate of potash, and still more is required to produce a marked and decisive blue or purple reaction with peroxide of hydrogen, or peroxide of barium and sulphuric acid, or a solution of chlorine or chloride of lime.

This, however, is the general fact, that nascent oxygen from any source may be made the means of discovering the presence of aniline by the blue or violet colour which it imparts to the alkali, and that of all these sources of oxygen that from the positive pole of a galvanic battery is the most sensitive and the most manageable.

Finally, I may observe that these results from galvanic oxygen indicate the rationale of the action of many of the substances which have lately been patented for the production of aniline blues, purples, and reds; and by operating on a salt of aniline in dilute sulphuric acid in a porous cell, so as to prevent the nascent hydrogen from the negative pole of the battery influencing the effect produced by the oxygen at the positive pole, very brilliant effects may be obtained.

[1] *Vide Lancet*, June 28, 1856, p. 708, and *Chemical News*, vol. ii., p. 3.

On the Analysis of Mixtures of Potash and Soda in Neutral and Alkaline Solutions, by M. MOHR.

THIS method is based upon the conversion of the potash into cream of tartar, and the estimation of the cream of tartar by a standard alkaline solution.

When the potash is in the state of carbonate, the operation is as follows:—The weighed potash is dissolved and saturated with powdered tartaric acid taken from a vessel which has been weighed beforehand. When the solution is saturated, there is added to it as much more tartaric acid as has been used: a slight excess of tartaric acid is of no consequence, the solubility of bitartrate of potash in tartaric acid not being very great. This solution is evaporated to dryness on a water-bath, and the residue extracted with a saturated alcoholic solution of cream of tartar. This is thrown upon a filter, washed with the same alcoholic solution, and then estimated.

When the potash is in the form of a neutral salt, tartaric acid cannot be used, since this acid sets the mineral acids at liberty in which the bitartrate is partially dissolved; it must, therefore, be contrived that the mineral acids remain combined. All neutral salts of potassium are changed on contact with bitartrate of soda into bitartrate of potash and a soluble soda salt. The solution is concentrated and evaporated to dryness; the residue extracted with alcohol saturated with bitartrate of potash placed upon a filter, and washed as before.

The difficulty consists in seizing the exact moment when there is sufficient bitartrate of soda added to decompose the potash salts. It is, however, easy to arrive at this in the following manner:—Standardize 10 c.c. of the alcoholic solution of cream of tartar; these 10 c.c. ordinarily require from 5 to 7 drops of normal soda solution. As the bitartrate of soda is soluble in the alcoholic liquid, a sufficient quantity of this salt should be added so long as the acidity of the liquid increases, that is to say, so long as it requires to saturate the 10 c.c.

of alcoholic liquid, more of the normal soda solution than would previously have been required before the employment of the alcoholic liquid. When the 10 c.c. require 20 to 30 drops of normal soda solution, it may be filtered, washed, and estimated as before, taking care to continue the washings until the strength of the alcoholic solution has returned to its original standard.

As in all methods of analysis by standard solutions, a certain amount of skill is required to arrive at accurate results; but this plan seems to be capable of extreme accuracy, as the following experiments will show:—

One gramme of chloride of potassium was dissolved and treated with bitartrate of soda:

Potash found . . .	0·6312 grammes.
Potash calculated . .	0·6320 „

In two grammes of the same substance there were obtained:

Potash found . . .	1·2625 grammes.
Potash calculated . .	1·2640 „

One gramme of sulphate of potash treated in the same manner gave a result of 1·010 grammes.

It is true that a portion of the alcoholic solution remains in the filter-paper; but, according to an experiment of the author's, on a paper of 115 millimètres, he found that it retained at most only one-sixth of solution.

This method may also be applied to the estimation of potash by weighing. It is only necessary to precipitate the potash with chloride of platinum, and to weigh the resulting chloroplatinate as in the ordinary process. There is an advantage in this plan, in being able to precipitate the potash by chloride of platinum, not only when it is in the state of chlorate, but also when it is present in the form of sulphate.

To estimate the potash in acid solutions, recourse must be had to the formation of alum, or to the following method recently proposed by M. Maumené.—*Annalen der Chemie und Pharmacie*, cxix. 123.

On the Analysis of Mixtures of Potash and Soda, by M. Maumene.

The estimation of potash in the presence of soda is effected by means of a salt of alumina; the alum is collected, and from the weight of this the amount of potash is calculated. Analyses made by this means give an approximate result which generally suffice for industrial operations. M. Balard has largely used it for his work on the constitution of the saline mother-liquors.

According to M. Maumené, the double sulphate of potash and copper

$$CuO.SO_3, KO.SO_3, 6HO$$

can be used, like alum, to condense the potash in an alkaline mixture.

The bases are converted into neutral sulphates, the mixed salts dissolved, the sulphate of copper is added, and the mixture left to evaporate spontaneously. Fifteen days are required for the crystalline deposit to be complete; it is then washed, dried in the air, and weighed.

M. Barreswil remarks upon this (*Répertoire de Chimie*, 1861, p. 367) that this enormous time might undoubtedly be considerably abridged by precipitating the salt with alcohol, by effecting its crystallisation in concentrated liquids, or by many other plans.

On some Naphthalic Derivatives, by M. L. Dusart.

Some years ago I showed how potash-lime might be employed as an oxidising agent when the object is to fix oxygen on an organic matter without greatly altering the molecule. Thus I have been able, by the action of this reagent, to produce phenoic acid (Runge's rosolic acid) at will, by heating phenic acid in contact with the air, and at a fixed temperature.

By applying this process, on this occasion not to a body having a certain chemical affinity, but to an agent entirely dissimilar—nitronaphthaline,—I have obtained a new product, which appears to me from its composition to be as interesting from its derivatives as from the uses to which it may be susceptible. Mononitrated naphthaline, like naphthaline, offers great resistance to ordinary oxidising agents. Under the influence of such reagents, a portion of the substance is generally destroyed, without the formation of any new product; sometimes the molecular group is altered by the elimination of a certain quantity of carbon, and the naphthalic type disappears. Direct oxidation, simply fixing the oxygen, has not yet been effected. However, it may easily be produced, at not very high temperature, by the above-mentioned reagents, the energy of which is relatively very feeble.

To this end I mix one part of nitronaphthaline, one part of caustic potass, dissolved in as little water as possible, and two parts of slaked lime; if too much water has not been added to the potass, the mass is pulverulent. I introduce it into a tubulated retort heated in an oil-bath to a mean temperature of 140°, and pass into it a slow current of oxygen or of atmospheric air. The gas is slowly absorbed, the mixture takes a yellow colour, which deepens as the operation proceeds. In about ten or twelve hours the oxidation is complete, and almost all the nitronaphthaline oxidised.

I am persuaded that the presence of air or oxygen is indispensable to the reaction. I have failed to produce oxidation by replacing the potash with soda; the lime also appears to be of especial service, for an inert matter, sand, for instance, cannot be substituted for it.

Taken from the retort, the mixture yields to water a salt of potash of a reddish yellow colour, possessing considerable colouring power. Acids added in slight excess to the solution change it into a thick magma formed of a beautiful yellow body, which, on being washed in distilled water, becomes almost perfectly pure.

This new body, which I will call nitroxynaphthalic acid, preserves, when dry, all its brightness. Its taste, at first fresh, becomes bitter; it is inodorous, but in a pulverulent state it excites the nasal mucous membrane. It melts towards 100°, and is not volatile; it dissolves in ordinary water, alcohol, pyroligneous acid, and acetic acid. In the latter it crystallises, by cooling, into beautiful golden-yellow needles.

It acts as a feeble acid, and, with alkalies, forms deeply-coloured, very soluble, and crystallisable salts, which yield, by double decomposition with metallic salts, variously coloured precipitates. Combined with bisulphite of soda, it forms a colourless salt, crystallising in fine needles; nitric acid attacks it with energy, forms oxalic acid, and at the same time a reddish resin, which by prolonged action is transformed into phthalic acid.

Contact with sulphuric acid heats it, developing sulphurous acid.

Energetic reducing agents transform it into a new substance, oxinaphthylamine.

The analysis of nitroxynaphthaline acid purified by crystallisation, and the estimation of its salts of baryta, lead, and copper, have led me to represent it by the following formula:—

$$C_{20}H_7(NO_4)O,NO,$$

that of the salts being

$$C_{20}H_7 (NO_4) O.MO.$$

It is evident that it differs from its generator, nitronaphthaline, $C_{20}H_7 (NO_4)$ only by one equivalent of oxygen and one of water.

The tinctorial power of this acid is considerable; it may be made very useful for dyeing purposes.

Oxynaphthylamine.—This, as I have stated above, is the new product formed by the action of reducing agents on nitroxynaphthalic acid.

Oxynaphthylamine is a feeble base, which cannot remain in a free state without rapidly becoming coloured; a greenish-black colour is instantly produced by contact with excess of alkalies. It combines with energetic acids, the salts of which, often crystalline, become rapidly coloured; heated with excess of potash it disengages ammonia whilst dissolving, and yielding an intensely green liquid similar to that of manganate of potass; acids precipitate from it a violet-red acid. Alkaline nitrites occasion an abundant disengagement of nitrogen in the neutral hydrochloric solution, and at the same time colourless crystals are separated, which have not yet been analysed.

The estimation of the elements of the crystallised hydrochlorate assign to oxynaphthylamine the formula

$$C_{20}H_{10}NO_2,$$

that of the hydrochlorate being

$$C_{20}H_{10}NO_2Cl.H.$$

The equivalent has been corroborated by the analysis of the platinum salt, which, on account of its instability, is with difficulty obtained pure.

The electropositive action of oxynaphthylamine, which may be considered as the amide of its generator nitroxynaphthalic acid, adds yet another support to M. Cahour's recent researches in this class of bodies.—*Comptes-Rendus.*

On the Production of New Colouring Matters by Decomposition of Nitronaphthaline and Dinitronaphthaline, by M. Carey Lea, *Philadelphia.*

1. In the process for preparing naphthylamine by the action of acetic acid and iron on nitronaphthaline, the nitronaphthaline is placed in a retort with iron filings and acetic acid, and after the first action has passed off, the contents are heated, a receiver attached, and hot lye is added to disengage the naphthylamine. But if a well cooled receiver be attached at the outset of the operation, and if heat be applied for some time before the addition of the caustic alkali, a liquid passes over which exhibits the following reactions:—

It has a pale reddish colour, and exhales the disgusting odour of naphthylamine. The pale reddish colour becomes pale violet by addition of mineral acids. If it is placed in an open capsule and heated on the sand bath, with addition of dilute sulphuric acid, the pale violet colour gradually deepens in intensity to rich blue purple. After a time a black crystalline precipitate falls, which must be separated. The brown filtrate by further heating again becomes rich purple, and deposits a further quantity of precipitate. But eventually the liquid becomes muddy brown (A), and yields no more of the precipitate.

This latter is produced at best in extremely small quantity, and sometimes scarcely appears at all. A grain or two is all that can be obtained from 50 or more grammes of nitronaphthaline.

The properties of this very interesting and beautiful substance, as far as could be determined from the very scanty amount obtained for examination[1], were as follows:—As caught on the filter, it constituted nearly black needles with a most brilliant golden green glitter. After being dissolved in alcohol, it was obtained as a dark red powder, which when placed on glass and a platinum spatula drawn a few times over it, gave a bright green, almost metallic reflecting surface, contrasting strongly with the red powder around it.

It dissolved somewhat readily in alcohol, colouring it an intensely deep blood red. The addition of a very small quantity of sulphuric or nitric acid brought this through a succession of shades as the quantity of acid increased, first ruby, then crimson, then rich purple, and finally blue purple, all of the richest shades, and so intense as to require great dilution to render the solution at all transparent. The substance exhibits considerable resistance to acids. The alcoholic solution acidulated with sulphuric acid may be boiled without destroying the colour. If nitric acid be substituted, the solution by boiling becomes pale straw colour, possibly an effect of the reaction of the nitric acid on the alcohol present.

The production of a red colour by alkalies and a blue by acids is becoming characteristic of a large number of organic colouring matters. Amongst these are, the colouring matter obtained by Church and Perkins from tincture of madder; by the resinous body obtained by Schiff in the spontaneous decomposition of naphthylurea; by the body obtained by Church and Perkins from nitrosonaphthaline[2]; by carotin, as observed by Dr. Husemann[3]; by a blue colouring matter obtained from picric acid described by myself. The frequency of this reaction is constantly increasing as we become better acquainted with organic colouring matters.

The substance which I have here described would doubtless be valuable as a dye if it could be obtained in sufficient quantity, for the richness of its colours leaves nothing to be desired, but it is only a secondary product in the reaction which produces it. Until it can be obtained in sufficient quantity to admit of its constitution being determined, I propose to call it Ionnaphthine, from ἰον, a violet.

2. If the muddy brown liquid mentioned at (A) in the second paragraph be treated with liquid ammonia, brown flakes separate. If these be treated with dilute sulphuric acid and bichromate of potash they become black. They then do not dissolve in water or alcohol, but dissolve in dilute nitric acid to a deep violet solution, greatly inferior, however, in colour to the solution of ionnaphthine. This substance may possibly be identical with that described by M. Du Wildes[4] and obtained by him by oxydating naphthylamine by means of nitrate of mercury.

3. If the solution of dinitronaphthaline in alcoholic ammonia be heated with solution of sulphite of ammonia, the red solution assumes a rich deep rose colour, far richer and more brilliant than the original solution. I have not as yet been able to isolate this substance.

Dinitronaphthaline is as fruitful in coloured derivatives as aniline. Treated in solution in alcoholic ammonia with stannous chloride, it yields a fine blue. Roussin's

[1] Much to the author's regret, he was obliged to discontinue this examination in consequence of an unexplained injurious effect upon his health by manipulating with naphthylamine.

[2] *Jahresbericht*, 1857, p. 390.

[3] *Chem. Centralb.*, May, 1861, p. 347.

[4] *Rep. de Chimie appliquée*, Mai, 1861, p. 172, M. Du Wildes is in error in supposing that a reaction which he has obtained is the first instance of a reproduction of an original body from a nitro-substitution compound.

"artificial alizarine" affords fine shades of purple; the reaction is obtained with great facility. Hofmann and Wood's ninaphthylamine, as I have obtained it, varies from copper to sealing wax red, but does not seem to me likely to be valuable as a dye. Roussin's alizarine will no doubt be very much so.—*American Journal of Science*, No. 95.

On Stibiconise, a Natural Oxide of Antimony from Borneo, by T. L. Phipson, *Phil. Nat. Doct. Bruxelles University, Member of the Chemical Society of Paris, &c.*

We get from Borneo a compact mineral, somewhat resembling certain varieties of Leptynite feldspar, and which is found more or less abundantly among the Stibine (sulphide of antimony) which the Island of Borneo launches into European commerce. It was at first looked upon as a portion of the rock which enveloped the native sulphide of antimony, and I believe many smelters have thrown it aside as such. It results from the examination to which I have submitted this mineral, that it is an oxide of antimony often very pure, and constitutes an ore sometimes superior in value to Stibine itself.

The mineral in question presents itself in form of a compact matter with a crystalline structure, yellowish-white or reddish, always giving a yellowish-white powder, and showing here and there crystals about half-an-inch in length, of a peculiarly pearly lustre, and having numerous horizontal stripes; these striped crystals are straight rhomboïdal prisms, terminating with two facettes (biseau), and modified upon two of the perpendicular edges.

This substance is not volatile in a tube closed at one end (by which character it is distinguished from oxide of antimony $Sb\ O_3$); before the blow-pipe, the purer samples are entirely volatilized in the flame of reduction, but are not volatile in the outer or oxidating flame. It cannot be melted before the blow-pipe (by which it is distinguished from exitelite, or antimonic acid $Sb\ O_5$, which is fusible); with carbonate of soda upon charcoal it gives a button of metallic antimony.

These characters prove the substance in question to be $Sb\ O_4$—the stibiconise of mineralogists, the antimoniate of antimony of some chemists. The samples which I have examined contain, as impurities, sulphur, stibine, oxide of iron, etc., but they were sometimes so pure that one of them gave me 65 per cent. of metallic antimony, whilst stibine seldom yields more than 45 per cent.

Mineralogists are not agreed upon the quantity of combined water contained in stibiconise, but the analyses I have made of the Borneo mineral appear to leave no doubt as to that question. The following record of one of the best analyses will show in what ratio the water stands to the antimonious acid:—

Stibiconise from Borneo.

		Oxygen.	Ratio.
Water	3·75	3·33	1
Antimonious acid, $Sb\ O_4$	65·00	12·30	4
Oxide of Iron, Alumina	10·00		
Sulphur, silica, etc.	21·25		
	100·00		

From this we may, therefore, deduce as the formula of stibiconise:—

$$Sb\ O_4,\ H\ O.$$

The specific gravity of stibiconise, according to several authors, is 3·80; but all the samples of the Borneo mineral gave me densities varying from 4·64 to 4·68, whence I concluded they would probably contain silver. However, I could not find in them sufficient silver to account for this increase of specific gravity. Neither does the Borneo stibiconise show more than a trace of arsenic.

As the stibiconise (antimonious acid) of which I speak, accompanies stibine (sulphide of antimony) in Nature, and affects the same crystalline form as the latter, it is very probable that the oxide in question has been formed in Nature at the expense of the sulphide, by means of water or steam heated under pressure, as we see it act in the beautiful experiments recenly made by my friend, M. Daubrée, where Wollastonite and some other minerals were artificially formed in perfect crystals by the mere action of superheated steam upon glass in closed tubes. The chemical reaction which Nature appears to have used in forming stibiconise from stibine is as follows:—

$$Sb\ S_3 + 3\ H\ O = Sb\ O_3 + 3\ S\ H.$$

Stibine. Water. Oxide of anty. Sulph. hyd.;

and $Sb\ O_3$ combining with one proportion of oxygen from the air gives $Sb\ O_4$ antimonious acid or stibiconise.

If we suppose, for a moment, that the transformation in question was effected by air (oxygen) instead of water, we find that it would require nine equivalents of oxygen to produce the same effects as three equivalents of water:—

$$Sb\ S_3 + 9\ O = Sb\ O_3 + 3\ S\ O_2.$$

Stibiconise has been regarded as a somewhat rare mineral in Europe, but Borneo seems to possess large quantities of it. Another argument in favour of its formation from stibine is, that it always accompanies the latter in Nature.

The stibiconise of Borneo is readily dissolved in a warm mixture of hydrochloric acid and tartaric acid. To transform it into metallic antimony, I succeeded best by fluxing the pulverised mineral with a mixture of charcoal, bitartrate of potash, and carbonate of soda.

It occurred to me that by mixing equivalent proportions of stibiconise and stibine, and applying heat, I should get metallic antimony thus:—

$$2\ Sb\ S_3 + 3\ Sb\ O_4 = 5\ Sb + 6\ S\ O_2$$

Stibine. Stibiconise. Antimony. Sulphurous acid.

But such is not the case: if the air has free access into the crucibles, the sulphide is converted in $Sb\ O_3$ which volatilises; and if the crucibles are nearly closed, the whole meets and forms a mobile liquid, which in cooling forms a bluish, metallic, crystalline mass, giving a brown powder when pulverised. The compound thus formed I ascertained to be an oxy-sulphide of antimony, analogous to what is called mineral kermes.

The Borneo stibiconise has lately been employed as an oil paint, for which purpose, as recommended by Dr. Stenhouse, it is calcined and pulverised. It is said to possess certain peculiar advantages for house-painting, &c. According to what I have said above, the purer portions of the mineral constitute an ore of antimony of greater value than the sulphide which is generally used as such.—*Technologist.*

Royal Institution.—The following lectures will be delivered in the ensuing week:—Tuesday, February 11, at three o'clock, John Marshall, Esq., "On the Physiology of the Senses." Thursday, February 13, at three o'clock, Professor Tyndall "On Heat." Friday, February 14, at eight o'clock, Dr. W. Odling, F.R.S., "On Mr. Graham's Researches in Dialysis." Saturday, February 15, at three o'clock, the Rev. Alex. A. J. D'Orsey, "On the English Language."

PHARMACY, TOXICOLOGY, &c.

Arsenical Pigments in Common Life, by Dr. A. W. HOFMANN, *F.R.S., Professor at the Royal College of Chemistry.*

THE following letter has been addressed by Professor Hofmann to the Right Hon. W. Cowper:—

In accordance with your wishes, I have examined carefully the green colouring matter of the artificial leaves from a lady's head-dress which you have sent me.

It is well known that such leaves generally contain arsenic, and often in considerable quantities. An experienced eye readily recognises the presence of an arsenic colour (Schweinfurt green) by its brilliancy, the intensity of which is as yet unrivalled by any other green. However, should there remain the slightest doubt, an experiment of the simplest kind would establish the fact. In most cases it would be sufficient to burn such a leaf in order at once to perceive the garlic odour which characterises the presence of arsenic.

In a dozen of the leaves sent to me analysis has pointed out on an average the presence of ten grains of white arsenic. I learn from some lady friends that a ball-wreath usually contains about fifty of these leaves. Thus, a lady wears in her hair more than forty grains of white arsenic,—a quantity which, if taken in appropriate doses, would be sufficient to poison twenty persons. This is no exaggeration, for the leaves which you have sent me were, some of them at least, only partly coloured, others only variegated. In consequence of your inquiries, I have been led lately to pay more than usual attention to the head-dresses of ladies, and I observe that the green leaves are often much larger and more deeply coloured than those which I received.

The question how far arsenic-dyed wreaths may be prejudicial to health is intimately connected with the discussion, so frequently raised of late years, as to the influence which arsenic-coloured paper-hangings exert upon the human system. This influence has been doubted on various grounds, both by the chemist and the physician. The alleged effect has been attributed to the development of arseniuretted hydrogen, or some other volatile arsenic compound, to which the white arsenic, by the action of the damp on the wall, or of the organic constituents of the paper and the paste, might possibly have given rise. Accurate experiments, however often repeated and often varied, have proved the inadmissibility of the assumption of gaseous arsenic exhalations, and, as it so often happens, the injury was denied simply because it could not be explained. Nevertheless, the deleterious effect of arsenic green paper-hangings is at present pretty generally acknowledged. Indeed, it does not require any high-flown hypothesis to explain the transfer of the arsenic from the wall to the system. The arsenic dust, bodily separated from the wall and dispersed over the room is quite sufficient for this purpose. The investigations of the last few years have clearly shown the presence of arsenic in the dust of rooms hung with arsenic-green paper, even when this dust had been collected at the greatest possible distance from the walls. Moreover, the chronic poisoning by arsenic of persons living in such rooms has been proved experimentally, inasmuch as the presence of arsenic may be demonstrated in their secretions, more especially if the elimination of the poison be accelerated by the administration of iodide of potassium.

The employment of arsenic green in the manufacture of paper-hangings, in staining paper, in painting children's toys, &c., has attracted the attention of the sanitary authorities on the Continent for many years past. In several of the German States, more particularly in Bavaria, the very country of arsenic colours (which are manufactured on a very large scale in Schweinfurt), a town in Franconia), the application of these colours to papering or painting rooms has been repeatedly proceeded against. I have before me an edict of the Bavarian Government of July 21, 1845, expressly prohibiting the manufacture and sale of arsenic-green paper-hangings. This general prohibition, it is true, was repealed by an Act of January 23, 1848, "for industrial considerations," and the use of Schweinfurt green permitted as before for house papering and painting, provided the colours were permanently fixed by appropriate means. The relaxation of the measures against Schweinfurt green appears, however, to have given but little satisfaction. In several papers laid both by chemists and physicians before the Academy of Munich, in its sitting of June 9, 1860, undoubted cases of chronic poisoning produced by arsenic papers, even when glazed, were brought forward, and the Academy was called upon to represent to the Government the necessity of strictly enforcing the former regulations against arsenic colours, and of removing all Schweinfurt-green wall-colouring from public buildings, schools, hospitals, &c.

The immense consumption of arsenic colours, and their reckless use under various conditions prejudicial to health, certainly claim the especial notice and the consideration of the public. Not satisfied with poisoning the wreaths which adorn the heads of our women, modern trade introduces arsenic without scruple even into their dresses. The green tarlatanes so much of late in vogue for ball dresses, according to an analysis made by Professor Erdmann, of Leipsic, contains as much as half their weight of Schweinfurt-green. The colour is loosely laid on with starch, and comes off by the slightest friction in clouds of dust. I am told that a ball dress requires about twenty yards of material—an estimate probably below the mark, considering the present fashion. According to the above analysis, these twenty yards would contain about 900 grains of white arsenic. A Berlin physician has satisfied himself that from a dress of this kind no less than 60 grains powdered off in the course of a single evening.

It will, I think, be admitted that the arsenic-crowned queen of the ball, whirling along in an arsenic cloud, presents under no circumstances a very attractive object of contemplation; but the spectacle, does it not become truly melancholy when our thoughts turn to the poor poisoned artiste who wove the gay wreath, in the endeavour to prolong a sickly and miserable existence already undermined by this destructive occupation!

Ladies cannot, I think, have the remotest idea of the presence of arsenic in their ornaments. If aware of their true nature, they would be satisfied with less brilliant colours, and reject, I have no doubt, these showy green articles, which have not even the merit of being, as far as colouring is concerned, a truthful imitation of Nature. There being no longer a demand for them, the manufacture of poisonous wreaths and poisonous dresses would rapidly cease as a matter of course.

Composition of Rhodizonic Acid.—H. Will has studied (*Annalen der Chem. und Pharm.*, Bd. cxviii. s. 187) the origin and composition of rhodizonic acid, which he says has most probably the formula $C_{10}H_4O_{12}$, and is tribasic. The paper contains a description of several of the salts of this acid.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Six Lectures on Light[1] *(adapted to a Juvenile Auditory), by* JOHN TYNDALL, *Esq., F.R.S., Professor of Natural Philosophy in the Royal Institution.*

LECTURE V. (Jan. 4, 1862.)

NOTES TO THE LECTURE :—

Two colours which, when blended together, produce white are called *complementary* colours—The spectrum extends in both directions further than we can see it—The eye often receives the impression of colour where no colour really exists—The eyes of many people are blind to certain colours—The invisible rays of the violet end of the spectrum may be rendered visible—In passing through a simple lens, the violet being more refracted than the red, both colours do not come to the same focus. This is called the chromatic aberration of the lens.

The colours which we see most commonly are colours of absorption, —that is to say, they are due to the fact that a portion of the spectrum is absorbed *within the body*, while a second portion is allowed to pass through the body—But colour is produced in a variety of ways without absorption. The spectrum itself is an example, its colours are those of refraction; so are the colours of the rainbow, which is, in fact, a solar spectrum—The splendid colours of a thin soap bubble are not colours of absorption—The iridescence on the neck of the dove, the hues of a peacock's tail, and the colours of the wings of certain insects are not colours of absorption—Most gorgeous colours are produced when colourless spirit of turpentine is poured on colourless water—The gold, straw-colour, or blue of tempered steel is not a colour of absorption. Such colours are produced by a thin transparent film of gas, or liquid, or solid, which itself has no colour. They may indeed be produced by an exceedingly thin fissure which is perfectly empty —For example, I have often produced fissures in the interior of a mass of ice, which showed splendid colours, though no air could possibly reach the fissure.

Colours are also produced by reflection from scratched surfaces—They glisten on the fine threads of the field spider—Thin clouds, especially in Alpine regions, are often flooded with the most splendid iridescences, even when the light which falls upon them is white—These colours are to be distinguished from those of the morning and evening clouds, which show simply the colour of the light that falls upon them—A dust, whose particles are all of the same size, when shaken in the air produces colours when a light is viewed through it—The colours here referred to have received various names in conformity with their modes of production. Some are called the colours of thin plates, others the colours of striated surfaces, others the colours of diffraction. They all depend on the great principle of *interference*.

This may be a difficult point to explain, but it may be rendered intelligible thus :—All the laws referred to in the Notes of the preceding Lectures, would follow if we supposed light to be produced by waves or undulations—In waves, the angle of incidence is equal to the angle of reflection; the law of refraction is also true of wave motion—Sound is transmitted on waves through the air; the aërial waves striking on the ear produce the impression of sound—In like manner it is believed that light is transmitted in waves to the eye; the waves not being those of air, but of a substance called *ether*, which penetrates all things and fills celestial space.

When a stone is cast into still water, circular waves are propagated round the point where the stone strikes the water. These waves consist in a succession of ridges separated from each other by hollows—Suppose two stones a little apart to be cast into still water, the waves issuing from one stone will cross those issuing from the other—At the places where one ridge crosses another, the water is lifted higher by the combined action of both stones—At the places where one hollow cuts another, the water is more deeply depressed by the combined action—But at the point where a ridge cuts a hollow, one neutralises the other, and the water does not quit its original level—Thus, the wave of one stone can be caused to destroy the wave of another—This is true of both sound and light; we may cause one sound wave to destroy another, so that silence results from their unison; we may also cause one light wave to destroy another, so that darkness shall result from their union—This action of waves upon each other is called *Interference*.

The waves which produce red light are the longest, those which produce violet light are the shortest; the waves which produce the other colours are intermediate in length between these two—All the colours above referred to as not belonging to the class of absorption colours are produced by the extinction of a portion of the white light by interference, the colour complementary to that which is destroyed being then revealed.

In our last Lecture I took a slice of white light; and by passing it through a prism, I tore it asunder—unravelled it—disentangled it, and showed you of what that white light was composed. You saw it consisted of a variety of colours, the red being the least refrangible and the violet light being the most refrangible of all. I then took those very colours which you saw so splendidly spread out before you on the screen,—I took hold of them by a lens, and out of those selfsame colours I built up the coal points, —the points of carbon from which the light emanated. Now, I am perfectly certain that in this room, if this room is a fair average of London society, there are many people who did not see all those colours. They saw perhaps two or three of them, but I am sure that many did not see the red colour at all. I know numbers of people who cannot distinguish between a brilliant geranium flower and the green leaves. A great and eminent man who has left his name in the world, and who will hardly ever be forgotton, Dr. Dalton, of Manchester, could not distinguish between the blushing cherries upon the tree, and the leaves surrounding them; the red and the green were all the same to him. Another man took a scarlet coat off a soldier's back and put it upon the back of a grey donkey and one of his patients could not tell the difference in colour between the soldier's coat and the donkey which it covered. And I say that in this room, extraordinary as it may appear to you—more particularly among the boys—for strange to say, this blindness to colour exists rather rarely among ladies, whether it is that they can disguise it better or not I do not know, but the fact is we do not find so many instances of colour blindness among ladies as among gentlemen,—there are many cases of this kind. Each one of us can render himself in some measure blind to colour. I know a certain botanist who told me sometime ago, that if he smells a certain kind of musk, it paralyses his nerves of smell, it disables, lames the nerves so that he cannot smell even the musk itself for some time afterwards. So it is with light. If strong red light falls upon the eye it renders the eye rather blind to the colour of the light; it disables the eye, and this is an experiment you can each make for yourselves. Take a red wafer; let it be brightly illuminated by the light of day. Look fixedly at that wafer for about a minute, and you will see after a little time a green edge about the wafer. Push the wafer suddenly aside, and you will find that the place occupied by the wafer is green. Why is it green? Because the eye has become partially blind to the red light, and when you move this away the white spot upon which it rested has its red light as it were quenched, and you see the complementary colour of red, which is green. And in this way we can render ourselves partially blind to various colours.

In fact we are all, to a certain extent, blind; and this is an experiment which, in passing, I may refer to. There is a spot on each of our eyes which is quite blind. Many of us do not know it, but it is a fact, and we may all find out this spot in this way. Take two wafers, or make two black spots upon a piece of white paper; place these two black

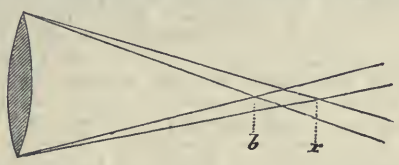
spots at about two and a-half inches apart; then close one eye and look with the other. Say close the right eye, as I am doing, and look with the left perpendicularly down upon the right hand wafer. The light from the left wafer

[1] Reported verbatim by special permission.

is now going into my eye and falling upon my blind spot, and the consequence is that I do not see one of the wafers at all. In this way I can quench the image of many boys present, and the image of the bright moon is not too large to fill up the blind spot. The consequence is that by allowing the image of the bright moon to fall upon this blind spot you have no sensation whatever of light produced, you do not see the moon at all.

I am now going to make one experiment in order to show you the theory of these colours. Some people often see colours where they do not exist. This green colour that I spoke of as being under the wafer does not really exist, still you see it. I want to make one experiment which I think will give you some idea of what I mean. Here we have the light from our electric lamp, and if I interpose this piece of blue glass, you will have blue light upon the screen, together with the comparatively faint light from the gas lamps, and I venture to say that if Mr. Anderson places his arm across you will see the shadow of it is yellow. At the present time, there is no yellow light there at all, but it is simply because you are blind to the blue that the yellow, the complementary colour to the blue, comes out over that portion of the screen.

We have learned that this white light is composed of rays of different degrees of refrangibility. We also know that when light goes through a lens it is refracted, the rays are converged and brought to a focus when they pass through a convex lens such as I hold in my hand. Now the beauty of our science, my friends, is that all these beautiful effects that you see are merely facts popping out and appearing in beauty from an under current of philosophy—from an under current of reasoning. The mind goes along underneath, as it were, these visible experiments, and knowing the law can actually predict the result of the experiment before it is even seen. And this is the beauty of natural philosophy; it is not looking at pretty experiments, fine though they may be, and pleasant as they seem to us, these experiments are, so to say, the outgrowth and the blossom of thought. It is not merely our senses that we exert, but the eyes of our minds are at work as well as the eyes of our bodies. Well now, let us reason upon this light:—if we send a beam of white light through that lens that beam is converged, refracted, but the blue rays of that beam will be more refracted than the red rays, and the consequence is if the blue comes to a focus there



(at *b*) the red will not come to a focus until here (at *r*). These two foci are different, and this distance between the focus of the blue and the focus of the red is called the *chromatic aberration* of the lens. The rays of different colours do not, therefore, come to the same focus; conceive a beam of light passing through that lens, and suppose that (*b*) to be the focus of the blue rays and this (*r*) the focus of the red rays. Now just picture to yourselves the kind of light coming through here. You see that up to this point [*b*] we have a blue cone within a red one; and if you place a screen there we shall find an image with a red rim, because the blue cone is surrounded by the red; but as we pass this focus of the blue rays we shall have instead of a blue object surrounded by a red rim, a red object surrounded by a blue rim. Let me, however, make the experiment, that is the best way of illustrating anything. I take the lens and place it in front of the lamp, and there on the screen we have an image with a blue rim. I daresay most of you can see the focus in the dust of the room. If I place my screen in the centre portion, you will see an image of the coke points. If I put it nearer the lens you have a beautiful image with a red rim, because, as I have already said, between the lens and the focus the blue cone is surrounded by the red one, the red being the least refracted. Now, let me pass beyond the focal point and see what takes place. You have now the blue rim, and this you see is the result of what we have called the chromatic aberration of the lens.

I have now to direct your attention a little farther to this splendid image which we obtained in our last lecture—the coloured spectrum, as it is called, produced by what may be called the unravelling of the thread of white light. Here I will place the slit in front of the lamp, for the purpose of obtaining the spectrum, and will cause the beam to pass horizontally; here we have the prism, and now I have produced that splendid coloured image which I have shown you many times. I have cast the beam in this case only through one prism; in the last lecture I passed it through two for the purpose of making the dispersion greater—of throwing the colours wider apart. You will also notice that the image produced by the glass prism is less than the image produced by the bisulphide of carbon. I make use of glass in this experiment for special reasons which I found out while preparing for the present lecture.

I have been talking about colour blindness, but I have now to refer to a very particular kind of blindness with which we are all afflicted. At least, I would not say it is an affliction; but at all events it is a fact. You see red light there, no doubt, but you see no light where I strike the screen [beyond the apparent red end of the spectrum], and yet at the present time there are millions of rays falling upon that dark place. These rays are not competent to produce light, but are competent to produce heat, and if these were lectures upon heat, I should be able to show you the existence of these rays here, but these being Lectures upon Light, I shall, therefore, beg to direct your attention to the other end of the spectrum. I say that there, also, you have a perfect shower of rays that are invisible to the eye—that are too shrill, I was going to say—for there are notes too shrill to be heard by the human ear, and these colours are, as it were, too shrill to be seen by the human eye. But they can be made visible. I take this pad of blotting-paper, and here I have a solution of sulphate of quinine in tartaric acid, a perfectly clear solution without any colour. [The Lecturer now directed the assistant, who had charge of the electric lamp, to cause the two poles to remain as far apart as possible during the ensuing experiment.] I want to explain to you the reason of that direction; it is not with the luminous portion of the coal points that I want to experiment; but it is with those particular rays in the space between the two carbon points—really a comparatively non-luminous portion. It is this particular place that is most wealthy in these rays that I am now going to render visible. [The Lecturer placed the blotting-paper in the dark space beyond the violet-end of the visible spectrum, and moistened it with the solution of sulphate of quinine, when it immediately glowed with a pale blue light.]

This, then, is the conversion of that dark space into a luminous one, the bringing out of the invisible rays of the spectrum—the rendering of these invisible rays visible, that thus enables you to see that stripe of light upon the paper. I will now show you this in another way. I will cut away from the beam issuing from that lamp those rays that I do not need—those rays of an intensely illuminating power, but which have not the power of being rendered visible—they are not those invisible rays I am now speaking of. I interpose a dark violet glass, such as you see here. I will move the dark glass for a minute, and hold this paper in the bright light; you see nothing; but if I

replace the dark glass, you see the paper now, how luminously it glows. There is the streak of light. And thus I might go on sketching upon my blotting-paper, and writing in lines of light. Poets talk about writing in light, but it is scientifically true; I am making use of light as a means of writing. Here I will write B-O-Y; I might sketch a flower if I were skilful enough to do so. Here is another sheet of paper, you see scarcely anything at present upon it, but when I hold it in the blue light you have the drawing of a flower. Here again I have the name of the discoverer of this phenomena, Professor Stokes, of Cambridge, and there you have what I wish you all most heartily, "A Happy New Year." We can thus render these invisible rays visible; and there are certain kinds of glass which are eminently calculated to render these invisible rays visible; and there are certain kinds of light which possess these rays as I have already stated, in far greater proportion than other kinds of light. You have seen a spark from this machine [pointing to a Ruhmkorff's induction coil]; that spark possesses these rays in an enormous quantity. Here is a piece of a peculiar kind of glass that has the power of converting these non-luminous rays into luminous ones, and if I send a discharge from this instrument through that glass, I think you will see it beautifully illuminated by a light somewhat resembling that that you have just seen upon the screen, a light entirely due to the fact of these non-luminous rays having the power of converting the glass itself into a source of light. Here you see it; it is a peculiarity of this uranium glass to be thus rendered absolutely luminous by the impinging upon it of these invisible rays. I have here a tube containing this very liquid with which I painted upon the screen, and I will place this tube in the same manner before you. This liquid, as I have told you, is the sulphate of quinine; there is in one half of this tube a quantity of this sulphate of quinine, and there is in the other half a quantity of another substance which can also be rendered luminous by these invisible rays, and you will see when I send a discharge, each of these liquids will be rendered visible in its own distinct colour—rendered luminous and showing its own peculiar colour. This, then, is the actually rendering luminous of the glass or liquid by means of this electric discharge; and it was for some time thought that at the moment of interrupting the current the light ceased, but that is really not the case. It is there entrapped, it lingers for a certain time, but for a very short time, so short, that it seems to disappear the moment I stop the current. But that is really not the case, there is a slight lingering of the light. I want to show you a longer lingering of this light, and I can do so by means of this other apparatus. The light lingers a little longer in this apparatus, and you will see in that beautiful garland I have placed on the wall, what takes place in a smaller way in the vessels we have been just experimenting with. You see the light is still lingering, the tubes are still illuminated, so that the light is entrapped,—bottled up, so to say, and given out again.

Now I have another experiment, and that is all I will trouble you with on this particular point. I have here a quantity of powder enclosed in these tubes, if I allow the light to fall upon them for a single instant, they take in a portion of light and give it out at their leisure afterwards. At the present time you see they are quite dark, there is no luminosity whatever, but I place them in front of the lamp for a moment or so, and now what do you see? Why actually the light that has fallen upon them has been retained, and is now being given out leisurely. So much then upon this principle of fluorescence, as it is usually called.

I have now to direct your attention for some moments to some further considerations regarding colour. I dare say every boy present would call the liquid that is in that bottle perfectly colourless. The keenest eye present sees no colour there. That is perfectly pure water—not the river water supplied to our tanks, not even spring water, but it is distilled water, so that there is not a trace of impurity in it. I cannot see a trace of colour there—not a trace; still, as the light passes through that liquid, there is a portion of white light withdrawn by the transparent liquid, and the remaining portion gives colour to the water. You may say the water is not coloured, that you object to drink coloured water; but still I say, if you allow the light to go to a sufficient depth into water, you have the light coloured. There is a very little light taken up by a small portion of the water, such as is contained in that bottle; but here I have a tube fifteen feet long containing water. I intend to send the beam of light through that tube, and the experiment is made in this way:—I want to show you the colour of that which you admit is perfectly transparent. This is a hollow, horizontal tube, the lower half of which is filled with water. These two ends are stopped up by two glass plates, so that when I look through the end I have it half water and half air. I will then send a sheaf of rays—a cylinder of rays, from the electric lamp—through this tube. I will cast the rays upon the screen. Half of those rays will pass through the air, and half of them through the water. At the end of the tube nearest the screen, I have a lens, which gives me an image of the end of the tube upon the screen, and you will then see that image will be one half white, which shows that the light that has passed through the air, whilst the other half will be the colour of the water. Let us now see whether that is the case. I will adjust the focus, and there you see the colour of the water. I am sure if that were presented to you in a glass you would rather hesitate to drink it, and yet that beautiful green is produced by distilled water—perfectly pure water—purer, in fact, than any water that we drink. If we interpose a smaller lens, we can make the image still brighter; and there, you see the beautiful green colour produced by this pure water. So much, then, for the colour of the water. And if you take water in a solid form; take it in the glaciers of the Alps, you see the most magnificent colours. I have often been on parts of the mountains where I could see the light transmitted by thick blocks of clear ice, and it was of a beautiful blue. This, then, is a colour—and it is the representative of a great number of colours—due to what we call "absorption:" the red rays are first absorbed by water, and, if you continue long enough, you take away the green rays, and finally pure blue is left as the colour of the water. It is an important point that I want you particularly to remember, that certain colours—in fact, all colours—are produced in this way, by a withdrawal of a portion of the white light. A portion is withdrawn, and the portion that remains is the colour. In some cases this portion is withdrawn by absorption. There are many other ways of withdrawing it, as you will see by-and-by. Absorption is one means of destroying one portion of light, but we have here another case. I have here some Burgundy wine, if I have not taken the wrong vessel. I do not know which of these two coloured liquids is the Burgundy, but I will take this by chance, and we will try the action of this on our spectrum. We will just ask the Burgundy to be kind enough to tell us to what it owes its colour; we will first produce a spectrum, and you will then see which colour the Burgundy will absorb and which it will transmit. I will use the bi-sulphide of carbon prism to produce my spectrum. I will then ask the question of that Burgundy—and this is really the vocation of the natural philosopher—asking or putting questions to Nature by means of experiment. The question I am going to put is, how this Burgundy gets its colour. I introduce the Burgundy into the path of the spectrum, and you see it is capable of absorbing the greater portion of it; it absorbs the blue, the indigo, the violet, and the green, except, perhaps, a little shade; but you see it transmits the red, and on this fact depends the colour of the Burgundy, because it

absorbs those rays that would give it a different colour. And now, if I take some coloured glasses, I can ask them the same question. I have here, for example, a green glass—why is it green? [Interposing it in the path of the rays of light.] Why, because it cuts off the red altogether, and allows the green to pass. Why is this glass red? Because, if I interpose it, it cuts off the green of the spectrum, and allows the red to pass; and thus you see this transparent to red and this other transparent to green. If I introduce both glasses there is perfect darkness. I have one peculiar liquid here which, by merely looking at, I do not know in colour from the Burgundy; but you will see in a minute that the spectrum will tell us something about its colour. I believe you will find that this substance has the power of chiselling out with the utmost distinctness the centre portion of the spectrum, and allowing these two ends to pass. It is transparent to the two ends. It is a solution of permanganate of potash; and you see how beautifully transparent it is to the two ends of the spectrum, while the central portion is cut completely out. You see, as I bring this into the rays, how it cuts through—how it ploughs out the centre of the spectrum. It will not allow one of these rays to pass. Let us just examine that colour a little, for it is really worth looking at. I will turn the lamp facing the wall, take off this slit, and simply interpose a circular orifice. There, you see a clear disc of white in the screen. I now interpose my permanganate of potash. There it is. Is not it beautiful? Let us see whether we can re-analyse that coloured disc—whether we cannot actually dissect this disc, and throw its two coloured components upon the screen. There it is. There are the two components to which this splendid colour is due,—the purple which you saw is due to the fact of this substance being transparent to both ends of the spectrum.

Now these colours are all produced, as I have said, by absorption. I might show you many others, and I have here some beautiful blue liquids and liquids of other colours, but I think what I have said will suffice to illustrate the formation or production of colour by absorption. This is not the only means I have of producing colour. Let me see what other means we have. After the lecture, if you wish to see some very beautiful colours, I can produce them by shaking a little dust of a seed called lycopodium upon these pieces of glass. The glass has no colour, and the lycopodium has no colour, but when I look through this glass at the electric light, the colours are most lovely. Here you produce colours without any coloured matter whatever, and if you shake this dust in the room and look at the light through it, you see beautiful colours. And so in the Alps I have seen light clouds floating in the air and they have been flooded with glorious colours although at the time the white light of the sun was falling upon them; they were simply produced by the white light of the sun falling upon the powder of water, if I may so speak of it, for the clouds are nothing more. You may also see we have here some pieces of glass, beautiful pieces of transparent glass. Place one upon the other,—we have nothing to do but squeeze them together; take two smooth pieces of glass—and this is one of the most beautiful experiments of colour—squeeze them together so as to bring them into close contact, and you will see the most splendid colours produced. I will now try an experiment by means of which Sir Isaac Newton made great discoveries and that is, the blowing of a soap bubble. This soap bubble, when finely and carefully blown, exhibits most splendid colours although the water is most perfectly transparent. Looked at by the daylight you see the most splendid colours upon the surface of this soap bubble when it is thin. I now purpose to shew you the colours produced between two glasses and how they are altered by the pressure of my fingers. I will hold my glasses in front of the lamp and project their image upon the screen by means of a lens. You see these colours: you see them there coming out as I squeeze the piece of glass. There is no colouring matter between them. As I relax the pressure of my fingers the colours almost disappear, ank as I press the glass, you see them squeezed into existence. This is not a case of absorption, for these beautiful colours are produced without any colouring matter whatever, and thus, if I were to take a soap bubble and exhibit it in that way you would see the most beautiful colours. I have here a piece of iridescent quartz which has by some means been fractured in the middle, and there are little thin fissures produced which do not even contain air; still I think I shall be able to show you the colours of those little fractures in the middle of this piece of quartz; let me try whether it is not possible. Yes, there they are most beautifully coloured. And here I have some skimmings of lead which has been suffered to oxidize so as to get a thin transparent film of oxide on its surface—as thin as a soap bubble, and the consequence is you have these glorious colours. You must consider this as really not produced by any colouring matter at all, but by the presence of a thin film, and, so considering it, the results appear to be something marvellous. Here again are some steel plates. My friend Mr. Gassiot has caused these to be covered with a film of oxide of lead. This is a beautiful discovery made by Nobili, an Italian philosopher, and you will see the splendid colours produced without any colouring matter at all, but simply due to the film of oxide on the surface; and that most ingenious man, Mr. De la Rue, has taken advantage of this to produce some very beautiful paper—such as I have here. He takes a kind of varnish and drops it in water, and it spreads out over the surface of the water; he then places this paper underneath the water, and when the varnish has overspread the surface he lifts it up and gets the varnish adhering upon his paper. He allows his paper to drain, and then he has fixed on its surface these splendid colours. You see in all these cases we have nothing but a simple, thin, transparent film. These colours, as I have said, are not due to absorption. I wish you would let me show you a cheap experiment, but an experiment full of beauty, an experiment to show you the colours produced by dropping oil upon water. I am sure every boy present when he sees the experiment will, after he goes home, go to the nearest chemist and buy two-pennyworth of oil and go to the Serpentine, if he can, and make the experiment for himself; and he can make the experiment there far better than I can here, because the oil has room to spread out. I will take now a quantity of spirits of turpentine and a little glass pipette, and pour a little of this spirit of turpentine on the water, and we will illuminate the surface of the water with the electric light. The water is, of course, a reflecting substance, and, therefore, I shall be able to show you its image upon the screen. Mr. Anderson is now pouring water into the tray which I intend to make use of for the purpose of showing you the production of colours by a thin transparent film of the spirit of turpentine. The moment the turpentine touches the water it spreads out over the surface in a thin film, and you will see what the consequence of that is. It is perfectly marvellous what a small amount of the spirit produces this wonderful effect. There also is another beautiful effect—the reflection of the waves produced by the motion of this tray; the water is seen shifting about, but after a little time it will come to rest, and then we shall see what the effect of our turpentine is. Now, I think it is pretty fair. There is scarcely anything that I am so fond of contemplating as wave motion. I could devote a whole lecture to these beautiful waves, and find means of showing you still more beautiful effects, but we have not time for it. We have here the spirit of turpentine. Watch for the first drop. You see a certain amount of colour; it disperses, for the turpentine evaporates very quickly. I want to draw your attention to the change of colour. See how rapidly it changes. It is now green; it will be some other colour immediately. It is red, perhaps it will

go again into green, I should not wonder if it were to do so. There, you see, the green is now spreading over the surface. What is the cause of all this? The colour depends upon the thickness of the film of the turpentine. This change of colour all depends upon the thickness of the film of turpentine, so that to each particular thickness we have a certain definite colour, and here is one great mark of Newton's genius. He was actually able to determine the thickness of the films from the colour that they showed, and he did it in this way. I have here two pieces of glass something like the two plates of glass I used a moment ago to show you these colours. One of these pieces of glass is a portion of a sphere, and the other is merely a flat plate. This is how he did it; he took a lens—a slice from a sphere of, I think, 360 feet in radius—he placed over that lens a plate of plain glass, and he then calculated, as, indeed, every boy present could, exactly the distance from the one to the other—the thickness of air between the plate of glass and the lens. When these were touching he had round the centre of contact circles of colour, and he determined the thickness of this air from each particular colour, and thus, when he saw the colour of a soap-bubble he could tell exactly the thickness of the film of water giving rise to the colour, which could not be done by any other means. I have here a couple of plates of glass to show you these rings, and I will try to throw the image of them upon the screen. There they are beautifully shown, and you will see when I interpose a plate of red glass, so as to get nothing but red through, that we have simply the ray of black and red.

Let me now just speak to you of what I must explain in the next lecture. These black bands alternating with the red are produced not by absorption, but it is so arranged here that one ray of light shall clash against the other and destroy that other. Thus, the black bands indicate the places where the two rays of light clash together; it is a case of what we call *interference*, which I shall have to explain in the next lecture. But before I pass to the subject of interference, I will make one experiment just to break the ground, and I scarcely know how I shall lead such philosophers as I have before me further in the theory of light. We have hitherto spoken of light as something darting out of a luminous body, as luminous particles which are reflected, like billiard balls from the sides of a board. That is not what we believe light to be. We believe it to be something like sound. Here I have a tuning fork set in a state of vibration, and these vibrations you may perceive are carried through the air to your ears, and produce in your ears the impression of sound. I want to show you that these vibrations actually cut through the air; that at the present time, although you see no motion in the air between you and me, the air is in a state of intense motion; and it is this motion, first of all communicated to the air by my organs of voice, which, carried through the air, and striking against your organs of hearing, cause you to hear the voice. Mr. Anderson is preparing an experiment for me which will show you in a very striking manner that these vibrations cut through the air as I have said. You all see that beautiful little flame. I have here a tube of glass which I will bring over the flame, if I speak to it, if I give it a proper word of command that flame will start into singing. I do not know whether I am skilful enough, but if you will allow me I will try. I want to speak to it in a certain key. (Ex.) [The Lecturer here uttered a loud musical note with his voice which instantly caused the flame to vibrate, giving out the same sound.] This is very wonderful, and I will tell you what it will prove. Do not fix your attention upon the flame alone, but fix your attention upon the air between me and it; and upon the fact that this experiment illustrates in the most perfect manner that some of the vibrations of the air pass from my voice to this flame and cause it to be started into vibration by hearing me. I have stood sometimes at a distance of thirty feet and have been able to say to that flame "sing," and I have been able to say "stop" and it has obeyed me.

In our next Lecture I shall conclude this subject of interference.

ROYAL INSTITUTION OF GREAT BRITAIN.

General Monthly Meeting, Monday, February 3, 1862.

WILLIAM POLE, *Esq., M.A., F.R.S., Treasurer and Vice-President, in the Chair.*

Robert Russell Carew, Esq.; William Whitaker Collins, Esq.; John Parnell, Esq., M.A.; and Major-General Edward Sabine, R.A., D C.L., President of the Royal Society, were elected Members of the Royal Institution.

The thanks of the meeting were returned to Professor Tyndall, for his discourse "On the Transmission of Heat through Gases and Vapours," on January 17; to Professor Rolleston, for his discourse "On the Affinities and Differences between the Brain of Man and the Brains of certain Animals," on January 24; and to W. Hopkins, Esq., F R.S., "On the Theories of the Motions of Glaciers," on January 31.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, January 21, 1862.

E. W. BINNEY, *F.R.S., F.G.S., Vice-President, in the Chair.*

William Arthur Darbishire, Esq., B.A., was elected an Ordinary Member of the Society.

A communication, "*On the Action of Nitrate of Sodium on Sulphide of Sodium at Different Temperatures*," by Dr. PH. PAULI, Union Alkali Works, St. Helens, Lancashire, was read by Professor Roscoe.

The mother liquid obtained in the manufacture of soda ash contains, as is well known, large quantities of sulphide of sodium. In order to oxidise that compound, nitrate of sodium is used. As long as the boiling point of the liquid is between 280—290° F., the sulphide is quietly oxidised to sulphate, nitrite of sodium being formed.

But if the nitrate is added when the temperature of the boiling liquid is about 310° F., a violent evolution of ammonia takes place, according to the following equation:—

$$2\,NaS + NaNO_6 + 4\,HO = 2\,NaSO_4 + NaHO_2 + NH_3$$

As the liquor contains a large amount of sulphide, the quantity of ammonia is so considerable that it may prove worth while to connect the evaporating pot with a tower filled with coke over which a stream of water or dilute acid is running.

If the nitrate be added when the liquor has been heated to a temperature much above 310°, a violent evolution of pure nitrogen occurs.

$$5\,NaS + 4\,NaNO_6 + 4\,HO = 5\,NaSO_4 + 4\,NaO_2H + 4\,N.$$

Mr. LEIGH suggested that the evolution of nitrogen in volcanic eruptions might be due to a reaction similar to that described in Dr. Pauli's Paper.

Mr. SPENCE and Dr. CALVERT explained the processes employed in manufacturing caustic soda for commercial purposes. Mr. Spence also described the beds of nitrate of soda in Southern Peru and Northern Chili, from which nearly all the commercial nitrate of soda is obtained.

Mr. BAXENDELL believed that the preservation of the nitrate of soda in these beds had been due to the dryness of the climate, as rain rarely falls in the region in which they are found. Had the climate been a rainy one, he believes the deposits of nitrate of soda would long since have been washed away.

Mr. Baxendell communicated an observation of Saturn which he had lately made. Owing to the relative positions of the Sun and Earth, with respect to the plane of the ring of Saturn, the ring ought now to be quite invisible in telescopes of moderate power; but on the night of the 18th inst., he had seen very distinctly a portion of the ring on the following or east side of the planet. The telescope used was Mr. Worthington's achromatic of five inches aperture. He also stated that from observations made by himself and Mr. Williamson, in 1848, he had been led to believe that the plane of the ring was not exactly parallel to the dark belts on the body of the planet. As several members of the Society now possess good telescopes, it is to be hoped they will direct their attention to this interesting point, and favour the Society with the results of their observations.

A Paper, "*On the Convective Equilibrium of Temperature in the Atmosphere*," by Professor Wm. Thomson, LL.D., F.R.S., &c., was read by Dr. Joule.

The particles composing any fluid mass are subject to various changing influences, in particular of pressure, whenever they are moved from one situation to another. In this way they experience changes of temperature altogether independent of the effects produced by the radiation or conduction of heat. When all the parts of a fluid are freely interchanged and not subject to the influence of radiation and conduction, the temperature of the fluid is said by the Author to be in a state of convective equilibrium. The equations of convective equilibrium in the atmosphere investigated by the Author are as follows, Π, T, and W denoting the pressure, temperature, and mass per cubic foot of the air at the earth's surface, and p, t, and ρ the same qualities of the air at any height x.

$$\left[\frac{p}{\Pi}\right]^{1-\frac{1}{k}} = \frac{t}{T} \qquad (1)$$

which is the known relation between temperature and pressure.

$$\frac{p}{\Pi} = \left[\frac{\rho}{W}\right]^{k} \qquad (2),$$

the deduced relation between pressure and density; and

$$dp = -\rho\, dx \qquad (3),$$

the hydrostatic equation, the variation of gravity at different heights being neglected, and the weight of unit mass (1lb.) being taken as unit of force. Hence by integration,

$\frac{t}{T} = 1 - \frac{Wx}{\Pi} \cdot \frac{k-1}{k}$, Or if, for brevity, we denote $\frac{\Pi}{W}$ by H,

$$\frac{t}{T} = 1 - \frac{x}{H} \cdot \frac{k-1}{k} \qquad (4)$$

From (4) and (1) it appears that temperature and density would both vanish at the very moderate height $\frac{1}{1\cdot41} \times H$, which is about 90,100 feet, or between 17 and 18 miles, if convective equilibrium existed and if the gaseous laws had application to so low temperatures and densities. It has always appeared to the Author to be most improbable that there is any limit to our atmosphere; and no one can suppose that there is a limit at any height nearly so small as 17 or 18 miles. It is difficult to make even a plausible conjecture as to the effects of deviations from the gaseous laws in circumstances of which we know so little as those of air at very low temperatures; but it seems certain that the other hypothesis involved in the preceding equations is violated by actions tending to *heat* the air in the higher regions. For at moderate elevations above the surface, where we have air following very strictly the gaseous laws, the rate of decrease of temperature would, according to equation (4), be $\frac{\cdot41 \times T}{1\cdot41\, H}$ per foot, that is to say, $\frac{1^\circ}{329}$ per foot, since $H = 26{,}224 \times \frac{T}{274}$ or 1° Cent. per 329 feet. Now, the actual decrease, according to Mr. Welsh, is 1° Cent. in 530 feet, or not much more than half that according to convective equilibrium.

It seems that radiation, instead of partially accounting for the greater *warmth* of the air below, as commonly supposed, may actually diminish the cooling effect, in going up, which convection produces. In fact, since direct conduction is certainly insensible, we have only convection and radiation to deal with, except when condensations of moisture, &c., have to be taken into account. In fair and cloudless weather, then, the lower and lowest air being on the whole warmer (the lowest being of course at the same temperature as the earth's surface), it is perfectly certain that the upper air must gain heat by radiation from the lower—and that the convective difference of temperature must be diminished by the mutual interradiation.

There are difficulties connected with the radiation of air and earth out into space, and of heat from the sun to air and earth; but I think a full consideration of all the circumstances must explain the smallness of the decrease of temperature which observation shows.

Dr. Joule having suggested that condensation of vapour in upward currents of air might account, to a considerable extent if not perfectly, for the smallness of the lowering of temperature actually found in going up, the Author has added the following investigation, in which the effect of condensation is taken into account.

If a quantity of air, dry or moist, is allowed to expand from bulk v to bulk $v+dv$, it will do an amount of work equal to pdv on the surrounding matter. Now, by the principle established approximately by Dr. Joule, in his experiments on air in 1844,[1] the change of temperature which the mass will experience will be almost exactly equal to what would be produced by keeping it at constant volume, $v+dv$, and removing a quantity of heat equal to the thermal equivalent of pdv. This is expressed by $\frac{1}{J}pdv$, if we adopt the usual notation, J, for the dynamical equivalent of the thermal unit. Now, if t and $t+dt$ denote the primitive and the cooled temperatures, so that $-dt$ expresses the cooling effect (which is positive, dt being negative), the bulk of the vapour, if at saturation in each case, would be $v\frac{ds}{s}$ if s denote the volume of a pound of vapour at saturation at any temperature t, and $s+ds$ its volume at temperature $t+dt$. Hence if, as it will be seen is the case, $v\frac{ds}{s}$ is greater than dv, a portion equal in bulk to $v\frac{ds}{s} - dv$ of the water primitively in vapour, must become condensed. Hence the abstraction of the heat, $\frac{1}{J}pdv$ produces two effects; it cools the mass of air at constant volume from temperature t to temperature $t+dt$, and it condenses a bulk

$$v\frac{ds}{s} - dv$$

of vapour. Hence, if L denote the latent heat of a cubic foot of vapour of water at temperature t, and N the specific heat of one pound of air in constant volume, we have

$$\frac{1}{J}p\,dv = N \times (-dt) + L\left[v\frac{ds}{s} - dv,\right]$$

if we suppose the mass of air considered to weigh 1 lb. (with or without the vapour, which will make but little difference on the whole weight). Hence,

[1] "On the Changes of Temperature produced by the Rarefaction and Condensation of Air," communicated to the Royal Society June 20, 1844, and published in the *Philosophical Magazine*, 1845, first half-year.

$$\frac{dv}{-dt} = \frac{JN + JLv\frac{d\ log\ s}{-dt}}{p + JL}$$

where, for brevity, $d\ log\ s$ is written in place of $\frac{ds}{s}$, $log\ s$ denoting the Napierian logarithm of s.

To find L and $\frac{d\ log\ s}{-dt}$, it is necessary to know the bulk of a pound of steam at different temperatures. Dr. Joule and the Author demonstrated,[2] by experiments on air, and by dynamical reasoning, that

$$L = \frac{J}{t}\frac{dp}{dt}\left[1 - \frac{\lambda}{\gamma}\right]$$

where p denotes the pressure of vapour at saturation at the temperature t, and $\frac{\lambda}{\gamma}$ denotes the rate of the bulk of liquid to vapour. Since $\frac{\lambda}{\gamma}$ is very small, we have $L = \frac{J}{t}\frac{dp}{dt}$ approximately.

It was shown also in the same Paper, that the density of saturated vapour was to be obtained more accurately from this equation, and Regnault's experiments on the latent heat of a stated weight of vapour, than from any direct experiments on the density of vapour made up to that time. This conclusion has been verified by the recent experiments of Messrs. Fairbairn and Tate. With the assistance of some excellent tables in Rankine's "Steam Engine and other Prime Movers," calculated on these principles, the author has obtained the following results :—

Temperature Centigrade, or $t-273{\cdot}7$.	Volume of 1 lb. of air at pressure 2117 lbs. per square foot.	Dynamical value of latent heat of 1 lb. of saturated vapour.	Proportionate diminution of bulk of saturated vapour per 1° Cent. of any wanted temperature.	Augmentation of volume of 1 lb. of moist air required to cool it 1° Cent.	Elevation from earth's surface required to cool moist air by 1° Cent.
			$\frac{d\ log\ s}{-dt}$	$\frac{dv}{-dt}$	$\frac{dx}{-dt}$
$t-273{\cdot}7$	v.	J L			
0°	12·38 cub. ft.	249 ft. lbs.	·0698·	·1905 of a cubic ft.	499 ft.
5	12·61	348	·0671	·2150	551
10	12·83	481	·0644	·2434	611
15	13·06	655	·0617	·2753	678
20	13·29	881	·0592	·3096	751
25	13·52	1171	·0569	·3455	827
30	13·74	1538	·0546	·3800	900
35	13·97	1999	·0524	·3950	932

The last column of this table, headed $\frac{dx}{-dt}$, is calculated from the column headed $\frac{dv}{-dt}$, by the following formula :—

$$\frac{dx}{-dt} = p\left[\frac{dv}{-dt} + \frac{v}{t}\right]$$

which shows the height, dx, that must be reached to get a lowering of temperature, $-dt$, when air saturated with moisture ascends. The pressure, p, is taken as 2117 lbs. per square foot; and the value of $\frac{v}{t}$, which is the name for the different temperatures, is $\frac{12{\cdot}38}{274}$. The results, for temperatures from 0° to 35° Cent., are exhibited in the last column of the table. For the temperatures 0°, 5°, and 10°, they agree very well with the height in which Mr. Welsh found a lowering of temperature of 1° Cent.; and we may conclude that at the times and places of his observations the lowering of temperature upwards was nearly the same as that which air saturated with moisture would experience in ascending.

It is to be remarked, that, except when the air is saturated, and when, therefore, an ascending current will always keep forming cloud, the effect of vapour of water, however near saturation, will be scarcely sensible on the cooling effect of expansion. Hence the law of convective equilibrium of temperature in upward or downward currents of cloudless air must agree very closely with that investigated above, and must give a variation of 1° Cent. in not much more or less than 330 feet.

It appears, therefore, that the explanation suggested by Dr. Joule is correct; and that the condensation of vapour in ascending air is the chief cause of the cooling effect being so much less than that which would be experienced by dry air.

The following extract of a letter from Professor W. Thomson, LL.D., &c., to the President, was also read :—

"About two years ago I wrote to you that a metal bar, insulated so as to be moveable about an axis perpendicular to the plane of a metal ring made up half of copper and half of zinc, the two halves being soldered together, turns from the zinc towards the copper when vitreously electrified, and from the copper towards the zinc when resinously electrified.

"If the copper half and the zinc half of the ring are insulated from one another, and if they are connected by means of wires with two pieces of one metal maintained at any stated difference of potential by proper apparatus for dividing the electro-motive force of the two plates of a Daniell's element into 100 parts, from 60 to 70 of those parts are required to reduce the zinc half ring and the copper half ring to such a state that the moveable bar remains at rest whether it is electrified vitreously or resinously.

"If the copper half ring is oxydised by heat, the amount of electro-motive force then required to neutralise the two halves is much increased. If, after oxydising the copper one day by heat, I leave the apparatus till the next day, the effect is generally diminished, though something of it still remains. After again heating the copper by laying it for some time on a red-hot iron heater and allowing it to cool, I found the effect almost exactly 100 parts. I have no doubt that by making the coat of oxide very complete and thick enough, and by cleaning the zinc perfectly, I shall be able to get considerably above the electro-motive force of a single Daniell's element. I remembered perfectly what you told me a long time ago about heating the coppers of a battery and getting a strong effect for some time equal to that of the Daniell's cell, when I tried the effect of oxydising the copper plate by heat.

"I believe there are also electrical effects of heat itself; so that if one half of a ring of one metal is hot and the other is cold, the needle will show a difference according as it is charged positively or negatively.

"For nearly two years I have felt quite sure that the proper explanation of voltaic action in the common voltaic arrangement is very near Volta's, which fell into discredit because Volta or his followers neglected the principle of conservation of force. I now think it quite certain that two metals dipped in one electrolytic liquid will (when polarisation is done away with) reduce two dry pieces of the same metals, when connected each to each by metallic arcs, to the same potential.

"There cannot be a doubt that the whole thing is simply chemical action at a distance. Zinc and copper connected by a metallic arc attract one another from any distance. So do platinum plates coated with oxygen and hydrogen respectively. I can now tell the amount of the force, and calculate how great a proportion of chemical affinity is used up electrolytically, before two such discs come within

[2] "On the Thermal Effects of Fluids in Motion," Part II., "Theoretical Deductions," Section II., *Transactions* of the Royal Society, June, 1854.

$\frac{1}{1000}$th of an inch of one another, or any less distance down to a limit within which molecular heterogeneousness becomes sensible. This of course will give a definite limit for the sizes of atoms, or rather, as I do not believe in atoms, for the dimensions of molecular structures."

NOTICES OF PATENTS.

940. *Making Bread, and Obtaining Starch from the Materials employed Simultaneously.* H. Anthonissen, Brussels. Dated April 17, 1861. (Not proceeded with.)

For the purpose of making bread according to the instructions contained in the specification, a certain quantity of flour is mixed with cold water to make the dough, this is then transferred from the mixing trough to a washing apparatus of peculiar construction, provided with sieves and perforated diaphragms, in which by the action of a stream of water the starch may be partially removed from the gluten, falling through the meshes of the sieves, on the upper surface of which the constituent last mentioned and the coarser farinaceous particles are retained. The compact glutinous mass is then withdrawn, placed in contact with fermenting materials, and finally made into loaves of bread. The starch is collected from the washings by means of fine hair sieves, drained and dried.

The falsifications here proposed to be carried out in the manufacture of an important article of diet stands curiously in opposition to the common practice of mixing rice, potatoes, or other forms of starch with genuine wheaten flour before making into bread. Inasmuch as macaroni, semolina, &c., contain a considerable quantity of gluten and are well recognised nutritious articles of food, it cannot be positively asserted that evil consequences must follow as the result of any alteration in the relative proportions of starch and gluten in bread; but there are many circumstances which tend to show that a judicious combination of these constituents is easier of digestion than others which differ widely from the proportions usually found naturally associated in the common varieties of wheaten grain.

948. *Increasing the Illuminating Power of Gas.* H. Carstanjen, Cologne. Dated April 18, 1861. (Not proceeded with.)

The employment of benzine is here recommended as an agent suitable for the purpose of increasing the brilliancy of burning jets of coal gas; and an apparatus is described which consists of a small chamber through which the gas in its passage is made to take up a certain quantity of the vapourised material before being consumed at the burner.

The agent here referred to is better known in this country under the name of benzol, and, employed in forms of apparatus very similar if not identical, has already been made the subject of many patent claims.

959. *Electric Telegraph Apparatus.* J. H. Johnson. Lincoln's Inn Fields, London. A Communication. Dated April 18, 1861. (Not proceeded with.)

This proposal refers to a modification in the arrangement of apparatus known as Morse's telegraph instrument, and consists in the employment of an electro-magnet, placed in circuit and traversed by the current of the line, and with its poles set in opposition with the contrary poles of the magnet in the ordinary Morse apparatus. The soft iron bar or armature usually provided is dispensed with under this arrangement.

962. *Obtaining Jellies, Syrups, and Drinks from the Tree Arbutus unedo.* P. Mingand, Paris. Dated April 19, 1861.

Directions are given in the specification for preparing a variety of confectionery products, using alcohol or water for extracting from the fruit the flavouring essence, and in some cases employing likewise the flowers.

Grants of Provisional Protection for Six Months.

2473. William Malam, Skinner Street, London, "Improvements in apparatus for the manufacture of gas."—Petition recorded October 3, 1861.

2986. Hermann Brambach, Lungengasse, Cologne, Germany, "A new mode of manufacturing gas for illuminating purposes."—Petition recorded November 27, 1861.

3103. William Clark, Chancery Lane, London, "Improvements in stoppering bottles and other vessels."—A communication from M. Victor Hippolyte Solon, Boulevart St. Martin, Paris.—Petition recorded December 11, 1861.

3208. William Mattieu Williams, Handsworth, Staffordshire, "An improvement or improvements in treating coal and other bituminous minerals and peat, in order to obtain solid and liquid hydro-carbons therefrom, and in apparatus to be used for that purpose."—Petitions recorded December 21, 1861.

3265. Thomas Pickford, Fenchurch Street, London, "Improvements in the manufacture of manure."

13. William Barker Patrick, Highgate, Middlesex, "Improvements in the manufacture of sugar, and in the apparatus employed therein."

23. Hermann Eschwege, Mincing Lane, London, "Improvements in treating wood and other vegetable spirit."—Petitions recorded January 2, 1862.

29. James Whitton Arundell, Gresham House, Old Broad Street, London, "An improved method and improved apparatus for removing impurities from coal, parts of which invention are applicable for the separation and cleansing of ore and other minerals."—A communication from Mr. Martin Neuerburg, Kalk, Rhenish Prussia.

37. Arthur Warner, Threadneedle Street, London, "Improvements in preparing materials for and in purifying coal gas."—Petitions recorded January 4, 1862.

55. John Stenhouse, Upper Brunswick Terrace, Barnsbury Road, London, "Improvements in rendering certain substances less pervious to air and liquids."

Notices to Proceed.

2240. George Norris, Brondesbury Terrace, Kilburn, London, "Improvements in the manufacture of soap."

2607. James Webster, Birmingham, "Improvements in manufacturing oxygen gas, and obtaining certain other products."—Petition recorded October 19, 1861.

2665. John McCall, Houndsditch, London, and Bevan George Sloper, Walthamstow, Essex, "An improvement in the preservation of articles of food."—Petition recorded October 24, 1861.

2920. John Henry Johnson, Lincoln's Inn Fields, London, "Improvements in the treatment of zinc ores, and in the apparatus employed therein, which improvements are also applicable to the manufacture of phosphorus."—A communication from Adrien Muller, Paris.—Petition recorded November 20, 1861.

3043. William Henry Balmain, St. Helen's, Lancashire, "Improvements in the manufacture of potash and salts of potash."—Petition recorded December 4, 1861.

2196. Patrick Robertson, Sun Court, Cornhill, London, "Improvements in treating yeast, and in the manufacture of ammoniacal salts, and a substitute for animal charcoal."—Petition recorded September 3, 1861.

2229. Charles Felton Kirkman, Palace New Road, Lambeth, Surrey, "Improvements in obtaining manure from sewerage, and in the apparatus employed therein."

2238. Nicholas Doran Proby Maillard, Dublin, "Improvements in the material and preparation of the material and apparatus for making potash, pearlash, and caustic potash for commerce."

2800. William Albert Shepard, Pall Mall, London,

"Improvements in preparing and treating gutta-percha and india-rubber."—Petition recorded November 7, 1861.

2961. Alfred Vincent Newton, Chancery Lane, London, "An improved method of removing and preventing the formation of calcareous and saline deposits in steam boilers."—A communication from Lewis Baird, Cambridge, Massachusetts, U.S.—Petition recorded November 25, 1861.

2997. Henry Wilde, Manchester, "Improvements in magneto-electric telegraphs, and in apparatus connected therewith."—Petition recorded November 28, 1861.

3142. Eduard Claude Barbotte de Beaulieu, Avallon, Yonne, France, "Improvements in apparatus for extracting gold-dust from auriferous sands."

3225. Frango'is Laurent and John Casthelaz, Rue St. Croix de la Bretonnerie, Paris, "Improvements in the manufacture of colouring matters."—Petition recorded December 24, 1861.

2288. Richard Waller, Baker Street, Portman Square, London, "Improvements in machinery and apparatus for manufacturing and refining cane juice and other saccharine substances."

2351. John Oliver, Colchester, William Sinnock, Sylvan Cottage, Woodford, Essex, John Grantham, Nicholas Lane, and Montague Richard Leverson, St. Helen's Place, London, "Improvement in the mode of obtaining certain chemical substances, and in the treatment of vegetable fibre, and in obtaining manurial and other products therefrom."—Petitions recorded September 20, 1861.

2377. Joseph Jacob, Golden Square, London, "Improvements in the mode of, and apparatus for, obtaining and treating hydrogen gas, and the application thereof to various purposes, parts of which improvements are applicable to the manufacture of iron and steel."—A communication from Carl Preisenhammer and Carl Weniger, Zöptau, Austria.—Petitions recorded September 23, 1861.

2597. Charles Denton Abel, Southampton Buildings, Chancery Lane, London, "Improvements in apparatus for the simultaneous manufacture of white lead and vinegar."—A communication from Robert Rowland, New York, U.S.—Petition recorded October 18, 1861.

3208. William Mattieu Williams, Handsworth, Staffordshire, "An improvement or improvements in treating coal and other bituminous minerals and peat, in order to obtain solid and liquid hydro-carbons therefrom, and in apparatus to be used for that purpose."

CORRESPONDENCE.

The Chemical Society.

To the Editor of the Chemical News.

Sir,—"The authors shall not be at liberty to publish their Papers until they shall have appeared in the *Transactions* of the Society." Such is the notice printed on the cover of the Journal of the Chemical Society, which contains papers read at the Society from Dr. Bolley and by Dr. Oppenheim. The communication from the former gentleman was published in *Dingler's Journal*, and copied into the *Centralblatt*, long before it was read at the Chemical Society; and that by the latter was read before the Chemical Society of Paris at their sitting on August 9 last, and was published *in extenso* in their *Bulletin;* an abstract also appeared in the *Comptes-Rendus* of the Academy of Sciences months ago. As a Fellow of the Chemical Society, I am, of course, obliged to any gentleman who sends papers; but after having read them in a foreign journal, I cannot help, when attending the meeting of our Society, having something of the feeling of a guest invited to a second day's dinner. I feel that the Society is to a certain extent snubbed. Our honoured and much-respected President manages better. He sends all his communications to the Academy of Sciences and the Royal Society, and does not treat the Chemical to a *réchauffé*. The Society however, like a scientific Lazarus, has occasionally the opportunity of picking up some crumbs which fall from the great man's table.—I am, &c. F.C.S.

MISCELLANEOUS.

Transparency of Gold.—In describing the transparency of gold in very thin layers, Mr. Makins in his recent work, gives an illustration, which may be new to many of our readers:—"This transparency," he says, "may be elegantly demonstrated, by taking some twenty grains of fine gold, and fusing it in a convenient shallow vessel; this is to be removed from the furnace in a completely fluid state, when, if watched, it will be observed that just upon cooling a crust of solid metal will first suddenly form, through which the light of the internal red-hot mass appears of a beautiful brilliant green colour."

Calcining Sulphur Ores.—Some improvements in furnaces for calcining sulphur ores, which are likely to become of importance in the manufacture of sulphuric acid, as they are said to offer a complete solution of the nuisance difficulty in the Swansea copper works, with the production annually of some 300,000*l.* to 350,000*l.* worth of sulphuric acid, at a merely nominal cost, have been invented by Mr. Peter Spence, of Pendleton Alum Works, Manchester. The inventor has already five furnaces at work in his own business, and four licences just commencing. Taking Dr. Percy's data as his guide, he declares that he could undertake to calcine all copper ores with about half of the present expenditure of fuel, and with the conversion of all the sulphur eliminated into sulphuric acid, the only cost of this acid being the nitrate of soda, which, with his furnace, is only half of that regularly used; and, in addition to the interest of the capital invested in vitriol chambers, no labour would be expended on the acid manufacture.

The Adulteration of Bees'-wax by Japanese wax is detected, according to Hager, by their different behaviour in a concentrated solution of borax, at the boiling point. Bees'-wax is totally insoluble in such a solution, while Japanese wax dissolves, and on cooling forms a milky white, gelatinous mass. From a mixture of the two the latter is dissolved out, carrying with it a portion of the former, while another portion rises and congeals on the surface.

ANSWERS TO CORRESPONDENTS.

*** All *Editorial Communications* are to be addressed to the Editor; and *Advertisements* and *Business Communications* to the Publisher at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

Vol. IV. of the Chemical News, containing a copious Index, is now ready, price 12s., by post, 12s. 8d., handsomely bound in cloth, gold-lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. A few copies of Vols. I. II. and III. can still be had. Vol. V. commenced on January 4, 1862, and will be complete in 26 numbers.

Pure Hydrogen.—A. A.—This may be prepared by passing the gas evolved from zinc and sulphuric acid, through a solution of caustic potash to absorb any free acid, then through a solution of nitrate of silver to absorb any arsenic or sulphur, and lastly through strong oil of vitriol, or over chloride of calcium to free it from moisture.

A. O. Z.—The best plan is to heat them; no other is effectual. The local action in your battery is most likely due to the solution of copper in the acid, and its subsequent precipitation on the zinc.

A would-be Fellow.—We will forward the desired information if you send an address where it will reach you.

THE CHEMICAL NEWS.

VOL. V. No. 115.—February 15, 1862.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On Cryolite,[1] *by M.* H. SAINTE-CLAIRE DEVILLE.

SINCE the discovery and the exportation of an important deposit of this curious mineral on the sea coast of Greenland, important applications have been made of it. Dr. Percy, who first received some, extracted aluminium from it almost at the same time as M. H. Rose and myself, to whom M. Hofmann sent some specimens of cryolite. It is now employed in the three aluminium manufactories, either as a flux or as a substance to be decomposed by sodium.

From its first introduction into Europe, it has been employed at Stettin, if I understand correctly, and certainly at Denmark, as a substance suitable for the preparation of soda and alumino-soda soaps. On that account it was first known in commerce under the name of soda mineral. The method employed in Denmark and Prussia, where it was utilised for the first time, consists in treating it with milk of lime at the boiling point of water. If the amount of lime is properly calculated, and the amount of water sufficient, the mixture is wholly transformed into fluoride of calcium and aluminate of soda. This reaction proceeds without attention and in a complete manner.

From this aluminate of soda a soap has been made which contains an enormous quantity of water owing to the alumina or the stearate of alumina which may be present. The aluminate has likewise been treated with gaseous carbonic acid, which converts it into carbonate of soda and a very dense aluminous precipitate, which was at first taken for alumina, but which, in reality, is a carbonate of soda and alumina.

I have, indeed, received from Copenhagen, some years ago, a specimen of this alumina, which had been dried in the air or by a stove. It was very constant in composition and appeared free from soda, since it had no taste. Its analysis yielded me the following numbers :—

Alumina	44·8
Carbonate of soda . . .	20·1
Carbonate of lime . . .	0·7
Water and carbonic acid . .	34·4
	100·0

Very prolonged washings with boiling, however, ultimately decomposed it, but never entirely, and the quantities of soda and carbonic acid which remained pointed out a true combination between these elements, which water only gradually destroyed.

I. Phosphorus.—Together with the nitrate of cerium,[2] I have likewise found a notable quantity of phosphoric acid, which I have not accurately estimated, on account of the invincible difficulty experienced in separating small quantities of phosphoric acid from an excess of alumina.

In the aluminium factory at Nanterre endeavours have been made to employ this alumina to prepare the double chloride of aluminium and sodium, by mixing it with salt and carbon; but it occasioned great surprise to find among the first products of the reaction of chlorine on the aluminous mixture a large quantity of phosphorus, which condensed and burned at the exterior part of the apparatus.

It is, therefore, seen that phosphorus exists in cryolite, and, unfortunately this element enters into all the products made from it, and always contaminates them in an injurious manner. M. Persoz, who has examined the aluminates of soda obtained from an aluminium factory at Rouen, has also proved the presence of phosphoric acid in it, according to a communication which he has kindly made to me. And, moreover, M. Morin and myself have each proved the presence of phosphorus in considerable quantity in aluminium made exclusively from cryolite and met with in commerce.

This is the process employed :—Dissolve the aluminium in *aqua regia* containing an excess of nitric acid; evaporate the filtered solution in the presence of a great excess of nitric acid, and when the greater portion of the acid is driven off pour the liquid, rather diluted, into M. H. Rose's re-agent, that is to say, into molybdate of ammonia dissolved in nitric acid; heat it a little and estimate the proportion of phosphorus from the quantity of yellow phosphomolybdate precipitated, or from the colour of the liquid.

This is one of the numerous substances, together with marine salt, fluorides, silicium, and zinc, that must be sought for in commercial aluminium. For the details of the analysis I would refer to the small *brochure*[3] which I have published upon this point; but I will take this opportunity to beg those numerous persons who have recently been devoting their attention to investigate the properties of aluminium to make an accurate analysis of the metal upon which they work. It is at present met with in commerce in very variable states of purity, and the discordant results which have been published respecting some of the reactions of aluminium may be due to the different and always injurious substances contained in it. Aluminium prepared from chloride of aluminium contains chiefly iron and chlorides derived from the flux. That which is prepared from cryolite, when this substance is exclusively used in the preparation of the metal, contains silicium, fluorides in rather large quantity, and, as has just been shown, phosphorus. Thus, the results obtained in researches must not be considered as being definitely settled until analysis has proved the purity of the material operated upon.

II. Analysis of Cryolite.—Berzelius has given the composition of cryolite, analysing it by means of

[1] *Ann. de Chim. et de Phys.*, lxi., p. 337.

[2] M. Damour and I have endeavoured to make use of the remarkable insolubility of ceric phosphate in nitric acid to estimate the phosphoric acid united with the alumina. We shall shortly publish the results of our researches on this point.

[3] "De l'Aluminium." Mallet-Bachelier, Paris, 1859.

sulphuric acid. I have repeated his analysis and published numbers perfectly in accordance with his own. It was advisable to re-commence this analysis, employing the Danish method,—that is to say, by means of lime, to estimate its value. This is how I proceeded, operating upon one gramme of powdered cryolite.

I mixed it with the results of the calcination of 1·059 grammes of perfectly pure carbonate of lime. I added a tolerably large quantity of water and boiled it, replacing the water in proportion as it disappeared by evaporation.

The filtered aluminate of soda was evaporated with an excess of nitric acid; the solid product was heated to 200° or 300° and extracted with water containing nitrate of ammonia and a little ammonia, which left behind a dense and nearly anhydrous alumina, which was calcined and weighed. The liquid containing the nitrates, from which traces of lime are precipitated, was evaporated and changed into carbonate by an excess of oxalic acid, a little tartaric acid, and calcination. The product, which was perfectly soluble and left no residue, was changed into chloride of sodium, which was weighed with the usual precautions. I thus obtained:—

Aluminium	12·7
Sodium	31·8
Fluorine (by difference) . .	55·5
	100·0

These figures are the same as those obtained by the other method.

III. Vanadium.—To find the vanadium existing in small quantities in cryolite, I took fifty grammes, which I pulverised and fused with three times their weight of caustic potash and a few grammes of nitre. The mixture was pasty and almost liquid at a red heat, which could not be raised very high for fear of fusing the silver crucible employed. The mass, which was bluish-green, was poured into a cold silver dish: it was dissolved in water, which became of an intense green colour; there remained a slight crystalline and ochreous residue, which will be subsequently examined.

A few drops of alcohol precipitate the manganese and render the liquid colourless; it is then filtered and supersaturated with sulphuretted hydrogen. There is first obtained a slight brown coloration, due to an almost insensible deposit of sulphide of silver or lead; then there deposits cryolite, in the form of a pulverulent, transparent, and apparently gelatinous precipitate[4]; finally, the liquid assumes the red colour of sulphovanadite of potash. Filter, treat with hydrochloric acid to almost exactly saturate the liquid, preserving a slight acid reaction. Heat, collect the heavy brown precipitate on a filter, roast the sulphide, fuse the vanadic acid and weigh it.[5]

These fifty grammes of cryolite gave me fifty-five milligrammes of red oxide of manganese and nine milligrammes of vanadic acid. Sesquioxide of iron was found in the residue, insoluble in water, which was composed of unattacked cryolite, a little sesquioxide of iron, and, lastly, a little silver taken up by the potash from the crucible in which the fusion was effected.

[4] This cryolite contains a little alumina and potassium, which has entered into its composition in the place of an equivalent quantity of sodium transformed into soda.

[5] Tested before the blowpipe, it gives, with phosphorus salt, an intense green colour in the reducing flame and yellow in the oxydising flame. Dissolved in hydrochloric acid, it disengages chlorine, and produces a red solution, which, upon evaporation, changes into the azure blue chloride characteristic of vanadium.

On the Estimation of Tin, by M. H. Rose.

The ordinary method of separating tin from the metals with which it is alloyed, and which consists in attacking it with nitric acid, may yield inaccurate results in the case of some metals the oxides of which are slightly basic. If it is desired to employ this process for a mixture of oxides, these must be previously reduced by hydrogen, an operation which can be readily effected in a small porcelain crucible.

It is known that another method consists in precipitating the oxide of tin (stannic acid) with sulphuric acid. This precipitation only takes place in dilute solutions when the oxide of tin is present in its *a* modification obtained by the decomposition of the bichloride. Care must be taken not to filter the precipitate, except after long standing, especially when the liquid contains hydrochloric acid; and in this case the whole of this acid must be well removed by washing; if any remains, there is a risk of losing some tin, which will be volatilised in the state of chloride at the moment of calcination.

There is no advantage in replacing for the precipitation, as M. Löwenthal has done, the sulphuric acid by a sulphate, or by nitrate of ammonia.

Precipitation by sulphuric acid may lead to erroneous results when the solution contains bodies which are carried down by the oxide of tin and which cannot be extracted from the precipitate by hydrochloric acid. This is particularly the case with phosphoric acid.

If the tin exists in the state of alloy with other metals, it must be attacked with nitric acid as if the separation was to be effected by means of this acid. After the greater portion of the nitric acid has been driven off by evaporation in a porcelain capsule, the mass is moistened with rather concentrated hydrochloric acid, and the mixture left in contact for half-an-hour. Add a certain quantity of water, which dissolves the whole, and precipitate with sulphuric acid.

M. Reynoso proposed some time ago to estimate phosphoric acid by precipitating it with oxide of tin in a nitric solution. There must be employed, to remove the whole of the phosphoric acid from the liquid at least four or five equivalents of oxide of tin, and, if perfect security be desired, there should be eight or ten equivalents. The precipitate has not a constant composition.

The oxide of tin, when strongly calcined, only dissolves in traces in most acids. Concentrated sulphuric acid can dissolve it in the form of a thick syrup; but water precipitates all the oxide from the solution.

The calcined oxide is dissolved by sulphate of potash in fusion, but on dissolving in water all the oxide is seen to separate again.

Native oxide of tin (*cassiterite*) can only be attacked by fusion with alkalies.

Tin from Copper.—The ordinary method of separation by nitric acid furnishes for the oxide of tin a result a little too high, a small quantity of oxide of copper adhering to the oxide of tin. The method, which consists in dissolving the oxidised mass in hydrochloric acid, and precipitating the stannic acid with sulphuric acid, is more rigorous in this case.

The finely pulverised matter may also be fused with a mixture of sulphur and alkaline carbonate. Water dissolves, along with the sulphide of tin, an extremely minute quantity of sulphide of copper.

If the two metals are treated with a current of chlorine, the chloride of copper retains a trace of tin.

Tin from Bismuth.—This cannot be effected by

means of nitric acid. There must be employed fusion with sulphur and an alkaline carbonate, or if the oxides are in solution, sulphide of ammonium.

Tin from Lead.—Here again, fusion with a mixture of sulphur and alkaline carbonate gives the best results. It is easy to ascertain that the oxide of tin retains traces of lead when nitric acid is employed.

Tin from Zinc.—The two methods by nitric acid and by sulphuric acid are applicable. If the metals are in solution they may be separated by means of sulphuretted hydrogen, provided the liquid is sufficiently acid.

Tin from Iron.—When these two metals are oxydised with nitric acid the oxide of tin dissolves in water along with the oxide of iron. Upon treating the mass after evaporation we can, indeed, render the oxide of tin insoluble in nitric acid, but a great portion of the oxide of iron becomes so at the same time. Sulphuric acid likewise precipitates a mixture of the two oxides from a weak solution of the chlorides.

The separation of tin from iron succeeds easily when a current of sulphuretted hydrogen is passed through the hydrochloric solution of the oxides. A mixture of sulphide of tin and sulphur is precipitated, whilst the iron remains in the state of protoxide. The separation may also be effected with sulphide of ammonium. In this case the liquid must be heated. With this precaution the sulphide of iron may be washed with water containing sulphide of ammonium.

The method of fusing the alloy with a mixture of sulphur and alkaline carbonate is not so good as the two preceding methods. Upon extracting with water a green liquid is obtained, containing, with sulphide of tin a small quantity of sulphide of iron.

Tin from Manganese.—This separation cannot be effected with nitric acid Precipitation with sulphuric acid from a hydrochloric solution succeeds perfectly.

Tin from Silver.—For this purpose there may be employed indifferently nitric acid, fusion with sulphur and an alkaline carbonate, or sulphide of ammonium.

Tin from Gold.—When the tin is in excess the finely-divided alloy may be boiled with rather concentrated sulphuric acid, to which hydrochloric acid is added carefully. The tin dissolves in the state of protochloride. Heat to the partial volatilisation of the sulphuric acid; the oxide of tin remains dissolved. Water precipitates it, together with finely-divided gold; but it dissolves again when heated with concentrated hydrochloric acid, water being afterwards added. The gold remains perfectly pure.

When the gold is in considerable proportion in the alloy it must be attacked with aqua regia. From the diluted solution the tin is precipitated with sulphuric acid.

Tin from Magnesia and the Alkaline Earths.—This separation may be effected by calcination with chloride of ammonium. Two calcinations are sufficient to remove all the tin. The first, indeed, only leaves traces of this metal mixed with the chloride of magnesium and the magnesia of which the residue consists.

Estimation of Proto- and Bin-oxide of Tin in a Mixture of these Two Bodies.—The process previously pointed out by M. Rose, consisting of the reduction of bichloride of mercury to the state of protochloride by the protochloride of tin cannot be employed. The protochloride of mercury does not separate perfectly from the liquid.

Both sulphuric acid and ammonia precipitate a mixture of the two oxides. Neither do the volumetric methods proposed for the analysis of a mixture of the two oxides give good results.

Oxide of Tin from Titanic Acid.—If the two oxides are in solution the separation may be effected by means of hydrosulphuric acid or sulphide of ammonium. When a mixture of the oxides is fused with sulphur and carbonate of soda and extracted with water, all the tin is found in the solution, and all the undissolved titanic acid remains mixed with a small quantity of insoluble titanate of soda. To estimate the soda in the titanic acid, this latter must be heated with four or five times its weight of chloride of ammonium, to the complete volatilisation of the chloride. Upon extracting with water, a small quantity of chloride of sodium dissolves and the titanic acid, which remains in a state of purity, may be calcined.—*Poggendorff's Annalen*, vol. cxii., p. 163.

On the Composition of a Carbonaceous Substance existing in Grey Cast Iron, by F. Crace Calvert, *F.R.S., F.C.S.*

Having often noticed that the quantity of carbonaceous mass left in the vessels in which grey cast iron was dissolved, varied with the concentration of the acid used, I began in September, 1858, a series of experiments on the action of very weak acids on cast iron, in the hope of obtaining a quantity of the so-called graphite which it contains, and I believe that I have arrived at results which throw much light upon the chemical composition of this substance, proving it to be composed of iron, carbon, nitrogen, and silicium. This substance occupies exactly the same volume as the cast iron from which it is obtained, and is sufficiently soft to be easily penetrated by a blade. I shall now describe the method of experimenting.

Cubes, of one centimètre in dimension, of Staffordshire cold-blast cast iron were placed in cork bottles with eighty times their volume of the following weak acid solutions:—

Sulphuric acid,	1 alkalimeter	contained	10	grs. of SO_3	anhydrous.
Nitric ,,	1 ,,		13·5 ,,	NO_5	,,
Hydrochloric	1 ,,		9·12 ,,	HCl	,,
Acetic ,,	1 ,,		12 75 ,,	$C_8H_3O_3$	,,
Oxalic ,,	1 ,,		9·00 ,,	C_2O_3	,,
Tartaric ,,	1 ,,		33·00 ,,	$C_8H_4O_{10}$	,,
Gallic ,,	1 ,,		10·00 ,,	cryst. acid.	

The foregoing figures represent the equivalent quantity of anhydrous acid as compared with 10 grains of anhydrous sulphuric acid per alkalimeter.

Besides the above, phosphoric, carbonic, oleic acid, tannin, and acid peat-water were also used.

After three months of contact, I found that, although the external appearance of the cubes was not changed in any of the vessels, still those in contact with the weak sulphuric, hydrochloric, and acetic acid solutions, especially the latter, had become so soft externally that the blade could penetrate three or four millimètres into the cubes. I therefore removed the solutions from the vessels, and replaced them by an equal bulk of each weak acid solution, and continued to do so every month for two years. I then found that the cubes in contact with the acetic acid ceased to yield iron to the acid, although they were still of the original size. They had, therefore, become transformed into the carbonaceous substance before mentioned. These are the results of the action of the various weak acid solutions on the centimètre cubes of grey cast iron, after two years:—

Acetic acid . .	Complete.
Hydrochloric acid .	Nearly complete.
Sulphuric acid . .	Ditto ditto.
Nitric acid . .	Action less complete than above.
Phosphoric acid .	No similar action.
Oxalic acid . .	Ditto.
Tartaric acid . .	Ditto.
Tannin . . .	Very slight action.
Carbonic acid . .	Ditto.
Gallic acid . .	Ditto.
Oleic acid. . .	Ditto.
Acid peat-water .	No similar action.

The action of acetic acid on grey cast iron is most interesting, for instead of ceasing when saturated with oxide of iron, as is the case with other acids, its action is continuous if the precaution is taken to close the mouth of the vessel with an ordinary cork. Thus I have had cubes of cast iron in contact with the same quantity of acetic acid for two years, and the chemical action still existed when the contents of the bottles were examined. This action of acetic acid appears, therefore, to be analogous to that which it has on lead.

To examine the chemical composition of the cubes transformed by the action of acetic acid, they were reduced to fine powder in an agate mortar, and well washed with boiled water slightly acidulated with acetic acid. The powder was then dried at 115° C., in a dry atmosphere of carbonic acid or hydrogen, according to the nature of the body to be determined in the mass. The carbonaceous substance so prepared presented the following properties and composition:—

The cubes of grey cast iron, which originally weighed 15·324 grammes, weighed only 3·482 at the end of the two years, and their specific gravity was reduced from 7·858 to 2·751. Their composition was as follows:—

	Composition of the original cubes.	Composition of the carbonaceous substance.
Iron . . .	95·413	79·960
Carbon. . .	2·900	11·020
Nitrogen . .	0·790	2·590
Silicium . .	0·478	6·070
Phosphorus . .	0·132	0·052
Sulphur . .	0·179	0·096
Lost . . .	0·108	0·205
	100·000	100·000

These results led to the following remarks.—

Nitrogen.—That the largest part of the nitrogen originally existing in the cast iron remains in the graphitoid substance, and only a small portion is transformed into ammonia. These facts tend to prove that the nitrogen of cast iron exists in it under two states, namely, that one portion is combined with the carbon, whilst the other is in a condition to unite with the hydrogen liberated from the water, and thus to form ammonia. This method of ascertaining the amount of nitrogen in cast iron, by determining the quantity of nitrogen in the state of ammonia, and that existing in the carbonaceous mass, appears to be the very best process known, for I obtained 0·790 from the same cast iron which only yielded me 0·100 by the process lately published by M. Fremy. For ordinary analyses of cast iron, the slow action of acetic acid may be advantageously replaced by that of hydrochloric acid, taking care to use freshly distilled acid, water free from ammonia, and placing the whole in a flask provided with cork and tube, so as to exclude the possibility of the ammonia existing in the atmosphere from interfering with the experiment. By this means the amount of nitrogen existing in cast iron can be easily and speedily ascertained.

Silicium.—I have ascertained by direct experiments that it is silicium, and not silica, that enters into the composition of the carbonaceous mass. Amongst other experiments I may state that I took 5·96 grains of the carbonaceous substance dried at 115° C. in a current of carbonic acid, and after having placed it in a small platinum dish, the whole was introduced into a porcelain tube, and submitted for several hours at a red heat to a current of pure and dry oxygen. I then found that the 5·96 grains had increased to 7·39 grains, or nearly the theoretical amount: for 5·96 grains of substance would lose 0·664 of carbon and the remaining 0·338 of silicium would become 0·718 of silica, whilst the 4·672 of iron would become 6·702 of peroxide of iron, or both added together 7·420, being within 0·13 grain of the weight actually found. Now, if the iron in the mass had been in the state of protoxide, and the silicium in the state of silica, the 5·96 grains employed would, after the loss of carbon, have decreased to 5·551 grains, leaving, therefore, no doubt that the carbonaceous substance contained metallic iron and silicium. Finding, however, that the quantity of silicium in the carbonaceous substance, though high, did not represent the whole of the silicium contained in the cast iron employed, I passed the hydrogen, liberated by the action of weak acids on cast iron, through fuming nitric acid, and found, on the evaporation of this acid, a white deposit of silica. All acids which give off hydrogen when in contact with cast iron, also give rise to the gas discovered by Wöhler, namely, silicide of hydrogen.

Carbon.—Like silicium, the quantity of carbon found in the carbonaceous compound does not represent the whole of the carbon pre-existing in the cast iron employed, as carburetted hydrogens are given off during the slow action of acetic acid on cast iron.[1]

Iron.—As shown by the above analysis, the carbonaceous compound contains 79·960 of metallic iron, even when the acetic acid has ceased to act upon it. I have made several experiments to satisfy my mind that the carbonaceous mass contains metallic iron, and not oxide of iron. Thus, I passed hydrogen, at a dull-red heat, over some of the carbonaceous substance previously dried at 115°, and obtained no water, and the experiment related under the head of silicium confirms this conclusion. All grey cast irons appear to yield the same relative proportions of carbon and iron; but as cast iron becomes harder and whiter, the amount of carbon decreases, and in fact, nearly disappears in Welsh white cinder iron, being replaced by silicium. The relative amount of carbon and iron in the carbonaceous substance corresponds to 4C and 6Fe, or are the same that I have found in some cast iron which had been saturated with carbon, by melting No. 1 cast iron in presence of a large excess of coke on a cupola, and called technically "keechy."

I do not, however, believe that the carbonaceous substance obtained by me is simply composed of 4C and 6Fe, but that the nitrogen and silicium found in it must likewise enter into its composition. But, in the present state of my researches, it would be premature to attempt to assign any definite composition to this substance. As I am, however, still pursuing the subject, I shall be happy, if I arrive at any conclusion, to communicate the same to the Society.

[1] I am now engaged in preparing a sufficient quantity of these hydrocarbons to enable me to submit them to a careful examination.

When the *graphitoid* substance prepared by the above process is exposed to the atmosphere, it absorbs oxygen with rapidity, and the temperature of the mass rises rapidly, protoxide of iron being first formed, which is converted into sesqui-oxide; but when this mass is placed in distilled water, a chemical action ensues similar to that described by M. Kuhlmann, namely, a portion of the carbon is converted into carbonic acid by the oxygen of the sesqui-oxide of iron, and the carbonic acid thus produced unites with protoxide of iron to form carbonate of protoxide of iron. The atmospheric action on the carbonaceous substance above described, explains the difference of composition which I have found to exist between the body obtained by me, and that of a soft graphitoid mass which was found to replace a mass of iron buried for many years amongst coal cinders, and which had the following composition :—

Mean of Several Analyses.

Peroxide of iron	66·61
Carbon	12·03
Silica	18·13
Sulphur	0·79
Phosphorus	traces
Lime	2·14
	99·70

Nitrogen was also present, but its amount was not determined. I am aware that the latter curious and interesting change in cast iron has been several times noticed. Thus, Dr. Henry published a short notice on this subject in the "Annals of Philosophy." Mr. R. Mallet also described a similar substance in the *Transactions of the British Association*. Mr. Warington has also noticed the conversion of cast iron into a soft black substance, which occurred in a brewery where it was in contact with sour beer. Lastly, Mr. E. W. Binney records, in the eleventh volume of the "Memoirs of the Manchester Philosophical Society," a description of a similar substance found by him at the emptying of a deep coal-pit in which the cast iron had remained submerged for a few years, and which yielded to Dr. Angus Smith the following composition :—

Iron and bases	38·8
Carbon	40·0
Silica	19·7
	98·5

In conclusion, the conversion of the guns of the *Royal George*, at Spithead, is so well known, that I merely refer to it to express the same opinion as Mr. Binney,—that it strongly behoves those who have charge of light-houses, now so often erected in the sea upon cast iron pillars, to carefully and frequently inspect such structures, for day by day their support must be weakened by the action of the salt water.

On the Action of Oxygen on Ammonia in Contact with Oxides, by M. Schönbein.

Berzelius has stated that what was regarded as a solution of oxide of copper in ammonia is, in truth, only a solution obtained by the aid of an ammoniacal salt; he has proved that pure oxide of copper is insoluble in pure ammonia, but that if but a single drop of an ammoniacal salt, carbonate, for instance, is added and the mixture stirred, a beautiful blue colour will appear, sometimes dark enough to make the liquid opaque.

When M. Peligot published his simple and elegant process for preparing Schweitzer's liquid, he determined that, in this particular case, the solution of the copper was not due to atmospheric carbonic acid, but to the spontaneous production of nitrite of copper, an important fact with reference to the natural formation of nitrates. It is this fact which M. Schönbein has just confirmed. The formation of nitrous acid is determined by the production of ozone. Other metals besides copper give a similar reaction.—*Journal für Praktische Chemie.*

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Six Lectures on Light[1] *(adapted to a Juvenile Auditory), by* John Tyndall, *Esq., F.R.S., Professor of Natural Philosophy in the Royal Institution.*

Lecture VI. (Jan. 7, 1862.)

NOTES TO THE LECTURE :—

The colours of thin films are produced by interference—The light falling on the film is partially reflected at its front surface; but a portion of the light enters the film and is reflected at its back surface —If the film be of a suitable thickness, these two beams will clash and destroy each other—By a different thickness, the two beams may be made to combine and help each other—To cause two red rays to coincide requires a thicker film than that necessary for the coincidence of the blue rays—Hence, a thickness which destroys one colour developes another. If the thickness of the film be uniform, the colour will be uniform; if the thickness vary, the colour will vary. It is to the varying thickness of its film that the varying splendours of a soap bubble are due.

A beam of ordinary light must be figured as performing its vibrations in all directions—By reflection at a particular angle these vibrations are reduced to a common plane: a beam, with its vibrations thus circumstanced, is said to be *polarized*—Thus light reflected from a plate of glass at an angle of 35½ degrees, is polarized—A portion of the beam which passes through the plate of glass is also polarized—By adding a second plate of glass, a still further portion of the transmitted light is polarized; and by using a large number of plates, almost all the light they transmit may be polarized—On passing through certain crystals, a beam of light is split into two, one of which is more refracted than the other. This is called *double refraction*—When, for example, the image of the coal points of the electric lamp is on the screen, the interposition of a piece of Iceland spar produces two images instead of one—The two beams emergent from the spar are polarized at right angles to each other—Thus light is polarized in three ways; by reflection, by ordinary refraction, and by double refraction.

A polarized beam has two sides: when one of these sides strikes upon a glass surface at the proper angle, the beam is reflected; but when the other side strikes the same glass surface, the beam is not reflected—Thus, preserving the angle of incidence, perfectly constant, and causing the mirror to move round the ray, in one position we have a maximum reflection, in the other a minimum; the quantity of light reflected in intermediate positions being intermediate between those two.

A beam of light, on passing through a plate of tourmaline, cut parallel to the axis of the crystal, is polarized—This beam can pass through a second plate of tourmaline if its axis be parallel to that of the first—But when the axes are crossed at right angles, the beam which passes through the front plate is cut off by the one behind—By the interference of the rays polarized by crystals, splendid colours are produced.

I think I ought to warn you at the outset that this is going to be a very hard lecture, but you have been so good and so attentive during the last five lectures, that I am quite persuaded I can calculate upon your attention during this concluding hour. I said in the notes of our last lecture that the laws of the reflection of waves were just the same as the laws of the reflection of light. You have only to observe a boat passing along, say, the Regent's Canal. When the water is very smooth the boat produces a series of ripples as it goes along. The observations cannot be made upon a windy day, because then the surface is not calm; when the boat moves along it produces a series of ripples in front, and those ripples

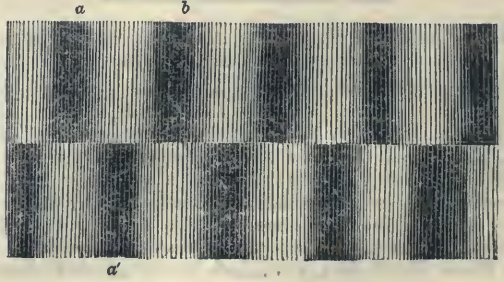
[1] Reported verbatim by special permission.

go along and strike against the side of the canal; the moment they strike the side of the canal you will find that they produce a series of waves crossing the original ones, and this reflection of the waves is exactly the same as we have found to be the case for light, the angle of incidence is equal to the angle of reflection; the reflected waves fall as much on one side of the perpendicular as the direct waves are on the other side of the perpendicular, and thus you see the whole surface of the canal chased, reduced to a beautiful mosaic owing to the reflection of the waves in this way. I have often stood with joy and pleasure upon the sea coast of the Isle of Wight, down at Ventnor, and while a storm has been raging, have watched the waves come in higher and higher, and I have seen them heel up against the shore obliquely, and instantly another wave was reflected back crossing the first, showing that the reflection of the wave was exactly the same as the reflection of light. In connection with this point, I want, in passing, just to make one experiment to show you the production of the beautiful figures that are formed by the reflection of waves. I might have taken mercury which gives a very pretty image because it is a particularly bright and beautiful metal, and reflects the light in intense quantity, but instead of that I intend to make a cheap experiment and simply to take water. I will put some water into this tray, and we will throw the beam of light upon the water so as to illuminate it, and the water in the tray will reflect the light upon the screen, and there I trust, if you are all perfectly still, (mind that is the condition, if you are not perfectly still you will shake the tray and produce waves when we do not want to produce them.) If you are, as I have said, perfectly still, you will see an image of the surface of the water quite clearly upon the screen, and then I will agitate the tray, and you will see the beauty of the waves that are produced. There is nothing more beautiful than these wave motions, and in order to get them in a fine manner, I have here, you see, a little glass tube drawn at the end to a fine point, and those who have very good eyes will see little drops of water falling from the point. I will cause these drops to fall upon the water in the tray, and then I shall obtain a series of waves and you will see what beautiful figures we shall have if I am skilful enough to make it aright; the water will do its duty if I do mine, of that I am perfectly persuaded. In the former lecture I made an experiment of this kind with a square tray, and you had thus square waves reflected from the straight sides of the vessel, but here the waves are oval. Look how they cross each other and produce this beautiful change. I drop the water on the centre and you see how the waves coil and encircle each other. Thus you see that science makes beauty cheap. I might go on for any length of time producing these figures. I will change the point of dropping, I will go along the diameter of this tray, and here you see the varying figures, the different shape of the figures depending upon the point on which the drop falls. See how beautifully these run into each other. This, as I have said, is a very cheap experiment, and shows beauty in a very simple form.

Now I come to one of the difficulties, and here I want your attention. I spoke a little during the last lecture of sound, the sound of my voice for instance, and I made an experiment, which, I am perfectly certain pleased you all, to show you that the sound passed from my voice through the air to a little flame, and caused that flame to produce a very beautiful note, to sing a most melodious song. There is a passage from the outer air into the brain, that passage is stopped at a certain place by a thin membrane which covers the chamber something like a drum, and hence this particular membrane and the chamber that it covers are called the "drum" of the ear. When I speak, vibrations of the air enter my ear and strike against this drum and put it in motion. I draw this fiddle bow against a tuning fork; at the present moment the drum of your ear would be seen bending in and out. So if you take two watches, one ticking nearly in the same time as the other, supposing one produced 100 ticks while the other produced 101; they start together, then the two ticks unite to give an additional impulse to the ear; at the 101st tick they also coincide, there you will have the two ticks going again and driving, as it were, the drum of the ear inwards, and pressing the drum of the ear outwards. As I have said, the drum of the ear is pressed inwards first, and then outwards by every sound you can conceive; I am sure the simplest of my philosophic hearers can understand the possibility of this, that one watch may be in the act of pushing the *tympanum* in (I have used a learned word, which I did not intend to do, this membrane is sometimes called the tympanic membrane) one watch may actually be in the act of pushing this membrane in when the other is in the act of pushing it out. And what would be the consequence? the one would neutralise the other, and by the union of both the sounds we should have silence. Take them separately and you produce a sound, take them both together, and in virtue of their action upon the drum of the ear the one would neutralise the other. Thus you have often heard the beats of organ pipes, I am sure you have many times, and these beats are simply due to the fact that at certain times the sounds of the pipes coincide and produce a loud sound, at certain other times they go in opposite directions and produce comparative silence. Here are two organ pipes, when I blow strongly into them you will find the sound goes up and down, and you have this effect that we call "beats." This is produced, as I have said, by combination,—at certain particular times the sounds coincide and produce a strong pushing of the membrane in, at other times one pushes it in and the other out, so that we have sound and silence alternately instead of a continuous sound. Let me try whether I can do it; of course my lungs are not sufficiently strong to make it a loud sound, but if I had a strong pair of bellows you would have the sound very loud. You hear the beating, and that is produced by what we call the interference of the sound.

Now, just let me say one or two words more upon this point, and then I will have done with it. When this tuning fork, or any other sound-vibrating body, is caused to sound in air, it vibrates thus. First of all, when it comes out it crowds the air together,—it pushes the air together, when it goes in it pulls the air asunder again. What is the consequence? Why, that in a line of sound the air is thus circumstanced. At first the particles are crowded closely together, and I draw the lines closely together; then by degrees you have them wider apart owing to this action we have been speaking of, and then again they are crowded together, again widely apart and so on.

And thus we philosophers can actually look at those things that quite defeat the ordinary eye. We can see, and you can see I am sure, the air between this tuning fork and yourselves, in a state of condensation and expansion, just as I have represented on the board. But now imagine two sounds to start from different points,—just let me define one point, the distance from this point of condensation (*a*) to this one (*b*) on our diagram is sometimes called a wave,—it is called the length of the wave, these are sound waves in the air.



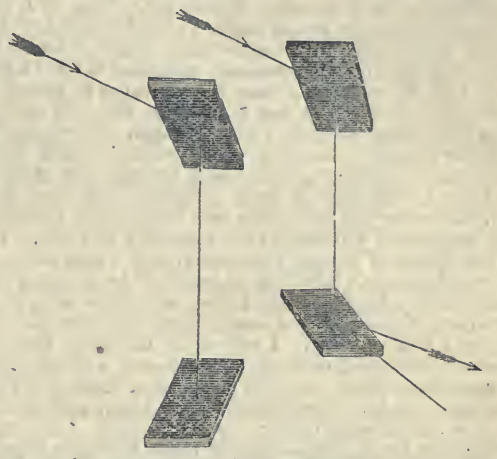
Now conceive that at one and the same moment this sound starts from here (*a*) another sound starts from here (*a'*) so as to produce a condensation underneath this expansion.

Then you see the one sound will actually blot out the other along the whole line, and thus we shall have a case of interference of sound. And this is always the case when one sound is half a wave length behind the other,—the one sound destroys the other.

Now do not be daunted, but give me your patience for one or two minutes more, whilst I pass on to light. A similar thing occurs with light. Light has this peculiarity, that whilst all the rays of sound, the particles of air between me and you, or between this tuning fork and you—when they are thrown into motion,—oscillate backwards and forwards, to and fro, in this way all along the line. The particles of the substance causing the waves of light do not oscillate so, they oscillate transversely across the line of the ray. That is the difference between sound and light. Let me take the case of a very thin film of glass, or of a soap bubble, or of varnish such as Mr. De la Rue uses to illuminate his paper; supposing I allow the ray of light to fall upon that film, a portion is reflected at the first surface and a portion goes through and is reflected at the second surface, and if the thickness be suitable these two rays will interfere with each other just like the two sounds we have been speaking of and will blot each other out and the consequence is by a film of a certain thickness you can blot out the rays of light as we have supposed the rays of sound to be blotted out.

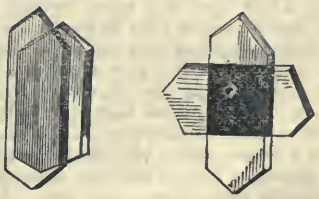
One word more, the waves of light differ in length, and you will see in the notes of our last lecture that the waves of red are longer than the waves of violet; the consequence is that the thickness, the necessary difference in the part travelled over by these two rays is greater in the case of the red than in the case of the violet, so that when we have an extinction of violet we have the red in all its glory, and when we have an extinction of the red we have the violet in all its glory. And this is the reason why we see these beautiful colours in these thin films. I won't trouble you with more than this, it is as much as you can bear, and I would not presume upon you too much.

Let me go on to another portion of the subject. You see I have here a piece of black glass, and it has a reflecting surface. If I cause a ray of light, falling upon a glass, to be reflected from it towards the ceiling, it is very curious but that ray of light before it strikes that reflector is vibrating transversely in all directions, but when it strikes the black glass and is reflected up, all these vibrations are performed in one way, they are all reduced to one common direction. Have you ever seen a snake going along a flat surface? A snake would represent a polarized beam, it wriggles along on the flat surface and all its oscillations or vibrations as you may call them that describe sinuosity are confined to the flat surface. A serpent, however, wriggling along with vertical folds would represent also a polarized beam. The snake's mode of progression is horizontal, and the serpent's is vertical. I want you to have a clear image in your minds, that this ray of light which is reflected from the surface, goes on oscillating in one direction only. Well now, I will show you next, that if I obtain a ray of light and cause it to be reflected upwards, and try to reflect it with another reflector, I can reflect it when I allow this beam of light to fall thus, but if I allow the side of the ray (for a ray, as it were, has two different sides), if I allow the side of the ray to strike this one, I cannot reflect it at all; it is quenched. Let me show you this experiment, and that will be better than any description. Here I will place this reflector in front of the lamp, and we will get a clear image of the disc upon the ceiling. Now, I say, that that beam that thus goes up to the ceiling is totally different in quality from the beam coming from the lamp. Let me show you this. If I take this second reflector and hold it in this direction, you see I can get the image clearly reflected upon the gallery. If I hold it so as to cross the ray, as



you see at the present time, it is extinguished. So you see when the side of the ray strikes the reflector it is not reflected, but when its face side strikes the reflector it is reflected. This is called a *polarized* ray of light; the ray has two sides. When one of these strikes against the reflector it is reflected; when the other strikes it is not reflected, it is quenched.

Now, let me pass on to another point. We have here two little crystals, and those crystals are among the most wonderful things in Nature; they are what we call Tourmaline crystals. I will show you these upon the screen. This is a crystal, I should say, about an inch in length, and less than a quarter of an inch in width. Now, these crystals possess this peculiarity; if a beam is polarized so that it vibrates horizontally, so that its vibrations are parallel, running along the axis of the crystals it can get through it; but if I hold the crystals vertically and let a beam of this kind fall upon it horizontally, it is completely cut off. I will show you this. If I hold two of them together with their axes in the same direction, the light from the lamp passes through both, but if I cross them and look at the lamp through, I find at the space where they cross, the light permitted to pass by the one is completely cut off by the other. Let me see whether it is not possible to be shown to you. There is the image of one of our crystals. I will bring it to a clear focus on the screen. I now introduce the other one so as to allow it to fall over the other, and there you see the crystals are transparent, but I will turn them cross ways, and look, the centre rays where they cross are cut off. I turn them



so as to render them parallel, and the light goes through both. I turn them crossways and they are black. Now, both of these rays, which we get through these Tourmalines, are polarized. When once the ray has passed through the Tourmalines, it is compelled to carry on its vibrations all in one way, and that is the meaning of the term "a polarized ray."

How now shall I deal with this subject? It is very difficult to render clear. Supposing I allow the light that has passed through these two Tourmalines to fall on a

reflecting surface. If I had the Tourmalines crossed, what would be the effect? One allows the rays to vibrate in one direction, and the other in another. Then if I hold this reflector at the proper angle, I shall be able to reflect one of the Tourmalines so as to remain bright, whereas the light from the other Tourmalines, according to what I have said, will be completely quenched, because it cannot be reflected. I hope you can follow me, for it is not the beautiful things which you may see upon the screen, but it is the beautiful thoughts which underlie these things that ought to interest you and will interest you most by and bye: only have a little patience with me now. Mr. Anderson will hold those two Tourmalines in front of the lamp and I will subject our reasoning to a test. This is the beauty of natural philosophy, that a man cannot go on wandering in a labyrinth of error; if he is wrong he is soon checked by Nature, and that, as I have said, is the grandeur of philosophy in comparison with other modes of thought and research. I will allow the beam from the Tourmalines to fall thus upon the reflector, and will reflect it upon the ceiling; you will find one of them black and the other bright. According to what I have said it ought to be so, let us see whether it is not the case. I am happy to see that by an accident one of these is longer than the other. You can remark that one is long and narrow, and the other is wide. Will anyone tell me, will my golden-haired friend in front tell me, which is the longest, the black one or the bright one?

A Voice: The black.

Professor Tyndall: Quite right; now bear that in mind. I will turn the reflector so, and thus I will project the image upon the screen. You saw that formerly I allowed the one side of the beam to fall upon that mirror, now I will allow the other side of the beam to fall upon the mirror, and let me see whether we get the same result, which is the black, is it now the long one?

A Voice: No, the other is.

Professor Tyndall: The other, to be sure, and thus we can verify the conclusion we previously arrived at.

I have now to make one or two more experiments upon a point of very great importance. I have here a piece of crystal—one of those wonderful edifices that are built up by Nature herself where she takes the atoms of matter and puts them together so as to build up forms of the most exquisite beauty; this, then, is one of those crystals found in Iceland, which, because they are particularly beautiful and transparent are called Iceland spar. Looking through that spar you see all things double, perfectly double. That spar, when a single ray of light falls upon it, has the power of splitting it into two,—dividing it into two equal beams, they are both refracted, but one is refracted more than the other, and the consequence is that you have a splitting of the ray into two rays. I have here a piece of such a crystal cut out from a thin transparent specimen of Iceland spar; I will send our beam through this crystal, and you will find we can in that way split the single beam into two, so that instead of one image on the screen you will have two, and I will try to show you the track of those two beams through the air of the room. Here I place my piece of spar, and see how beautifully they come up: just look at those two images. Take that spar away and you have a single image, put the spar in and you see what takes place. That wondrous substance has the power of splitting these beams into two. And if I turn this round so as to bring one over the other, many of you can see the track of these beams through the room. As I turn those round and round, I think many present will see two beautiful cylinders of light passing through the room, showing you the splitting of the ray into two parts. You see that immediately it emerges from the spar the splitting of the ray takes place. One peculiarity of those two rays that we get from the spar, is that they are totally different from ordinary light. Now, what have I to do next? I want to investigate those two beams—I want to examine whether those two beams that issue from the spar are like ordinary light? They are not; those beams are both polarized, the rays of one are oscillating horizontally, and the rays of the other are oscillating vertically. Now, I only wish I had you for a few hours after all these lectures, that I might make you all undergo an examination, but inasmuch as I cannot have an examination, I will ask you a question. I am sure, from what you have now learned you will be able to tell me, from an experiment I am going to make, the manner in which this invisible thing which causes light is vibrating. I think I may almost appeal to you, Sir, [addressing one of the little boys,] to decide this highly philosophical and important point. Well now, let me see how I am to attack it. I will first of all produce those two images. Now, supposing I put a plate of Tourmaline in front of the lamp, I shall have two images of that plate of Tourmaline on the screen—an image for each beam. But we know that a plate of Tourmaline allows only rays that oscillate in a certain direction to pass through it,—that if they go in the same direction as the axis they can get through, whereas, if they go perpendicularly to the axis they cannot get through; so that in these two images of the Tourmaline upon the screen you will see one bright, answering to the ray that oscillates along the axis, and the other black, answering to the ray that oscillates perpendicularly to the axis. Let us see whether we cannot do that. Now, I say that one of those beams—I do not know which—oscillates horizontally, and the other will oscillate vertically. I now place my Tourmaline in front of the lamp,—the one image is black, and the other is bright. Now, let each young gentleman give himself time to reflect, and I ask you first to fix your attention on that right-hand beam. May I ask you, Sir, in what directions the vibrations are moving: are they horizontal or vertical?

A Voice: Vertical.

Professor Tyndall: They are vertical,—they vibrate across the axis,—and thus they are intercepted, and the image is perfectly black. Now, let us go on with our reasoning. We will test it still further. I have here a reflector,—and I think, in order to enable you to distinguish one beam from the other, I will produce a little magic, a little necromancy: I will colour those two discs for you by that wonderful property of interference. I will throw a bit of transparent crystal into the path of those beams, and that will give you two beautifully-coloured discs instead of the white ones. Here is the crystal; you will find it competent to colour those two discs, and, what is more, to colour them differently. The two colours I am going to produce will be complementary colours, and you all know what complementary colours are,—they are those which, when blended together, will produce white. If I turn this, you see I change my colour. How beautiful they are! The right-hand colour is now yellow, and the other is blue;—these are complementary colours: they produce white, although if you mix Prussian blue with gamboge you produce green. I will cause these to come a little nearer to each other; I will cause them to be as much contrasted as possible. You see, as I move this round I get a succession of colours. Here are red and green, blue and yellow, green and red again,—all complementary colours.

Well now, gentlemen, let me go on with my reasoning. You see in our reasonings we build up our argument, as it were, by those glorious facts; our logic, as it were, makes those facts upon which it hangs itself. It is not a mere heaving up of ideas, but it is really that we take hold of the glorious facts of Nature, and we follow them and trace their connexion, and we, as it were, get up to the very intelligence that built up Nature, the very intelligence that actually pronounced the fiat that endowed Nature with these glorious laws; it is our privilege, and we are entrusted with the exercise of that power to make ourselves acquainted with these things, and to see the intelligence,

and the law, and the order, and the beauty, and the grandeur that underlies all those facts. Well, now, I have caused those images to be coloured. We have already seen that the two beams are polarized. I will cause the two beams to be reflected from this surface. Will both be reflected?

A Voice: No.

Professor Tyndall: No, one only will be reflected, and in order to be enabled to tell which, you will find one red and the other green; you will find, when I reflect one upon the ceiling, a different one will be reflected from the one thrown upon the screen. I think this will be best shown if I turn the apparatus; I can thus reflect one beam upon the screen, and the other fairly upon the ceiling. Which is now most reflected above?

A Voice: The green.

Professor Tyndall: The green is most reflected; and if I could catch the proper angle, I could actually quench the other altogether. There is the green; it is perfectly different. That is the under portion of the ray. Now I reflect the side of the ray; which is now most vivid?

A Voice: The red.

Professor Tyndall: So that you see the polarization of the ray still continues here; we have one ray upflected downwards, and the other ray reflected sideways. Thus you see, when once we get hold of a truth of Nature, a million courses of reasoning, a million lines of logic, will converge upon that truth, and no two will ever contradict each other. You may rely upon this, that when we have once seized hold of a truth, every additional experiment will only render that more and more firm.

Well now, I have shown you two colours apart. I want now to show you these two colours actually blending together, overlapping each other, so as to satisfy you that white is really the result. I want to show you the intermediate colour. For that purpose I must use an instrument which acts like two Tourmaline crystals. By bringing them parallel, I can cause the beam to go through, and by causing them to cross I can cut off the beam. It is a polarizing apparatus, as it is called. I have a yellow and a blue disc overlapping each other, but in the centre part there is no trace of green,—it is white. Here again, I have red and green producing white where they overlap. Here again is the blue and yellow with the white. It is not, perhaps, absolutely wrong to say that blue and yellow produce green, but still if we take pure blue and yellow light, and mix them together as we have here, we produce white. As I turn the ins rument you have the different complementary colours overlapping each other and producing white. I have here another apparatus, a still harder one, dreadfully difficult to manage, but I must try it. We are not to be daunted by difficulties, so I must try whether I cannot get a beam through this or not, and if I can, I must show you some effects produced upon crystals. I think it might be worth while just to show you the two coal points actually coloured in complementary colours upon the screen. We will see whether we can colour our coal points, as it is a very pretty experiment; we will see whether we cannot build up the coal points of these complementary colours. There, we have our pair of complementary colours, and if I turn this round, you get other colours. There, you have the left hand red, and the other green, and so on as I move the instrument.

Well now, let us try to get a beam through this complicated instrument. I have here some bits of gypsum, which is found in the quarries at Montmartre, near Paris; it is as transparent as glass, and yet if I send a beam through it in the apparatus—I have simply split a thin bit off with my penknife—and interpose one of these crystals, you will find that I have very beautiful colours produced. I first of all cross the two Tourmalines, and the light is completely quenched. I then take some sulphate of lime and put it in between those two, and you will find that it has the power of restoring the light in a measure, and showing you these splendid colours. Thus, you see, we get these beautiful colours where there is, properly speaking, no colour at all. You have seen these varying colours, in one place it was green, in another place it was red, it is entirely dependent upon the thickness of the piece of crystal. One thickness gives me red, another thickness gives me blue. Now, here I have a piece of crystal cut from this selenite, and polished so as to form a little wedge, the edge is extremely thin, and it gradually thickens to the back. What is the consequence? As each particular thickness gives me a particular colour, you will find the colours arranged in parallel bars from the thin edge upwards. Let me see whether it is not the case. Here you see the colours showing the peculiar thickness which produces them. Here I have another piece of selenite in the shape of a lens, hollowed out in the centre so as to make the centre thinnest, and the thing gradually thickens. What is the consequence? You ought to have a series of rings because it thickens on all sides from the centre outwards. As I turn it the middle of the circle is light, and it gets darker towards the edges. This is produced by a perfectly transparent body. These are all pieces of crystals, the particles of these crystals are, as I have said, built together in the most wonderful manner by Nature herself. I have here a piece of glass, you see that when I introduce it in front of the lamp it is not at all crystalline, there is no power to act upon the light that passes through it, but if I put this piece of glass into a press and squeeze it up in the centre, in fact, bend it, it will have crystalline qualities. I will first then introduce this between the two parts of the instrument, and you will find it has no power at all, either to darken or to lighten the screen. It has no power similar to that possessed by the gypsum, no power whatever of the kind; but when I now squeeze it you will find, from the strain upon the particles, it becomes like a crystalline body.

Now I want to show you that by means of this polarized light I could actually tell you the part of that glass that has been subjected to pressure and the part that has been pulled. There is part of it in a state of pressure, and part in a state of strain, and by means of this polarized light I could tell you which part is in a state of pressure, and which is in a state of strain. In fact, we have this much power over these things—that if we take a weight and compress with it, a small cube of glass, by properly examining the cube you could tell by the colour with mathematical certainty the exact amount to the pound weight, of the pressure that was exerted upon the cube of glass.

Now the time is going, I am sorry to say, or I might also cause these changes by heat. For instance, here are two pieces of glass crossing each other; they have been thrust into the fire and allowed to cool, and they have been cooled in a manner so as to throw them into a state of strain similar to that in which I have drawn these pieces of glass by means of the press, and produce the same beautiful effects. Now I will take a piece of glass and heat it before you. I believe this piece of glass has no power at all for polarizing the light. But the moment I heat it its particles are drawn into a state of strain, and the glass has the power of a crystalline body. If I take a sheet of glass and heat the corner of it, you will find I have the same power. At the present time that sheet of glass has no power at all to restore the light; but if I now heat one corner, what is the consequence? You see at once this thin sheet of glass is thrown into a peculiar state of strain, and allows the light to pass,—and it acts as a crystalline body, and allows the rays to pass. Here again is a very beautiful experiment that I made for the first time myself in preparing this Lecture yesterday. I took a hot poker and placed it on a plate of glass of this kind, in the middle. The hot poker disseminated its heat, and its heat

passed away round about the point of contact, and the consequence was, it made the glass to expand and forced the particles—crowded them together so as to produce a beautiful black cross upon the sheet of glass. And thus I might go on with experiments of all kinds by means of the gypsum and by means of the selenite crystal I first showed you. I might cut flowers out on films of different thicknesses, and might thus produce the most beautiful bouquets, merely by a piece of crystal, there being in this no colour at all. Here, for instance, I have something which is intended for a rose, but it is not a very good one. No, I find it is a tulip; see the varying colours! There the rays of blue predominate; here, as I go round, you see a yellowish hue predominates. Here, then, we have another: that is a red rose with green leaves. As I turn the instrument round thus, it makes a monster—the leaves red and the rose blue. I must not now detain you longer.

Thus, I have gone through all these glorious facts, my boys, and we have learned, I trust, something of this thing we call Light. But I will say over and over again, that you must not be content—those who pursue the subject must not be content with admiring those beautiful experiments, you must try to get at the law and order and philosophy that underlies them; that is the grand point. There are two classes of people in this world with regard to science. One class—a highly cultured class—fine fellows, noble men, generous souls, of great knowledge—men of literature; but, unfortunately, they say they have not time at all for science—they are very ignorant of science—they know nothing at all about it. These men I respect; of them there is some hope. But there is another class of men who see in science simply what it produces in the market. These are the men that will shout and yell about the electric telegraph, and about the steam-engine—anything that brings country butter to town is good and great, anything by which you can send messages, any thing that you can bring into the market and get money for will be appreciated by these men, and they will stand by science on that account. Boys, that is not science. I cannot point you to a single great discovery in the world, in the history of science made by these men. They are not the men who make discoveries. It is those men who pursue the thing for the love of the thing—whether it be natural philosophy or any other branch of science. It is those men who create these people of the market by thousands. Why, there is Volta, of Italy, and Oersted, of Copenhagen, and Mr. Faraday, of the Royal Institution; these three men have created ten thousand of your self-styled practical men. All honour to practical men as long as they do not limit to their ends the aims of philosophy. But let us pursue science for its own sake; let us not pursue it merely because it has a certain market value. I trust that when you become men, and become philosophers, you will bear this in mind, and take care to seek to draw all these men of high culture over to our side. The really great practical men sympathise with us, but the market men are incorrigible, we cannot do much with them; but with the other men you can do much. They have high and noble sympathies, and I have no doubt that by-and-bye we shall lay hold of these men, and that they, as well as you, will see that science is a glorious thing, and a noble subject for the human intellect to be engaged upon.

CHEMICAL SOCIETY.

Thursday, February 6, 1862.

Dr. E. Frankland, *F.R.S., in the Chair.*

Mr. Prestwich, F.G.S., was admitted a Fellow of the Society.

A Paper, by Mr. Adie, "*On Ground-Ice*," was read by the Secretary. He commenced by referring to Dr. Frankland's explanation of the formation of ground-ice, with which he did not altogether agree. His observations had been made in a district about eleven miles north of Liverpool, the surrounding country being flat, and the ditches flowing slowly. Two nights' frost was sufficient to cause a fringe of ice to form along the edges of the ditch; this afterwards becoming detached floated down the stream until some obstacle detained it, a considerable quantity having been found to collect at a bend of the stream, the weeds at this point being covered with ground-ice. Since the temperature at the surface of a piece of water was often as much as ten degrees lower than at a short distance below the surface, it was necessary that the water should be shallow. He had seen a statement that ground-ice occurs chiefly at the beginning of a frost, and that a quick, sharp frost generally produced only surface ice. He then alluded to the observations of Dr. Parkerson, who considered that the reason why streams in a flat country are most favourable for the production of ground-ice was that they are free from an under-ground supply of water, and that it had been observed that a frozen fog was a precursor of its production; also, that when the frost was not severe, so that a slight thaw took place during the day, large quantities of ice that had formed on trees during the night, fell during the thaw that followed, and, this being repeated each night, a large quantity of ice was precipitated on to the ground or into any stream that might be near.

Dr. Frankland said that, on occasion of a previous paper by Mr. Adie, he had proposed a solution of the phenomenon; it had struck him that since the ground-ice is always found on rough surfaces, this roughness, and the agitation produced in the water flowing over the obstacles, might assist the congelation, the same effect being observed to take place with solutions of salts, as, for instance, if a rough stick be plunged into a strong solution of alum; the solidification taking place at a temperature very slightly above that at which it ordinarily occurred, it was necessary that the water should be shallow, or else the bottom of the stream would not be sufficiently cooled. He thought that Mr. Adie's observations about the formation taking place at the bend of the stream confirmed this view. Mr. Adie had said that ground-ice was retained under trees, but he thought that the reason of its being found in such situations was that water, although it allows luminous heat to pass through it, resists the passage of non-luminous heat; and that, in consequence, the bottom of the water might be colder in a shaded part.

Mr. Prestwich said, that it had been observed by M. Le Clair, that the formation of ground-ice took place at places where shadows fell on the water, and where the bed was rough, and that crystals were first formed at the side of the stones not exposed to the stream, the ice gradually extending until the stones were covered; this ice thus formed afterwards rose and floated away, carrying stones with it. He thought this fact was rather interesting to the geologist, as affording an explanation of some geological changes.

Dr. Smith remarked, that in a river which flowed along smoothly until it came to a bridge, at which point there were ripples, he had found ground-ice covering the stones at this part.

Mr. Crace Calvert said, he had observed that ice, which had been rubbed by sliding, melted quicker than that at other parts of a frozen surface, and that, in order to ascertain whether the friction made any difference, he had taken two pieces of ice and weighed them, and then after exposing one piece to severe friction, while the other was left to itself, again taken the weight, and he had invariably found that a larger portion of the piece that had been exposed to friction was melted than of the other piece.

The next Paper was by Dr. H. Bence Jones, "*On Crystalline Xanthin.*" A case had come under his notice of a school-boy, who suffered from incessant sickness and

pain in the stomach, the urine being bloody at first, and afterwards a complete stoppage taking place; at length a violent perspiration relieved him, but a cold brought on a fresh attack; albumen was found in the water passed during the complaint, and also a crystalline deposit resembling uric acid, but redissolving by heat. The hydrochloric solution of this crystalline matter gave on evaporation six-sided crystalline plates, the aqueous solution had a feebly acid reaction; in fact, all the properties of this substance corresponded with those of the substance called xanthin, or sometimes xanthic acid; deposits of urates, however, were afterwards found, the urine having a high specific gravity.

Dr. Frankland remarked, that an examination of the nature of the deposits occurring in urine during different diseases was very interesting to the physiologist.

Mr. A. H. Church then made a verbal communication "*On Silica.*" He first drew attention to some fossils found in some triassic red conglomerates, which contained more than ninety per cent. of silica. These fossils were originally corals, but had been altered by infiltration of water holding silica in solution. On acting on a specimen with hydrochloric acid, the remaining carbonate of lime was removed and the silicious crust left behind. He had endeavoured to produce this substitution artificially, by filtering a solution of silica, containing also carbonic acid, through a piece of coral, and the process succeeded completely. A weak solution, containing about one-half per cent. of silica answered best. All the silica was removed during the filtration, and a large quantity of carbonate of lime entered into solution. The operation also succeeded with shells, although not to the same extent. He had observed during the evaporation of a solution of silica that a number of concentric rings were found after a time in the solution, presenting a curious appearance. These rings were produced by crusts which formed all round the edge of the solution and afterwards fell into the liquid. He had found that the maximum strength of a solution which did not deposit a portion on keeping was that containing six-tenths of a per cent. of silica; a stronger solution always gelatinised after a time. A solution of chloride of barium or of calcium produced no change when added to this weak solution; but if to the stronger solutions a small piece of carbonate of lime, or of baryta, or of strontia were added, the whole immediately gelatinised, even one milligramme being sufficient to produce the effect. He had some fossils with him in which a concentric arrangement of the silica was visible; it had also been observed in shells and in carbonate of lime deposited from a solution containing gum or sugar.

A Member said that when a solution of silica was placed on limestones it formed a definite compound with the lime, and that skin immersed in such a solution became tanned; also, that when the solution was boiled in flasks it did not change, but in an open vessel it gelatinised in a short time.

The next paper was by Mr. Bloxam, "*On Arsenic in Sulphuric Acid.*" He had some time ago brought forward an electrolytic test for arsenic, and had found that no samples of hydrochloric or sulphuric acid were free from this impurity. The quantity was sometimes very small, but still it would be satisfactory in medico-legal inquiries to possess an acid containing no traces of the metal. Various methods for the purification of sulphuric acid had been tried,—for instance, distillation with chloride of sodium, both concentrated and dilute acid being employed; also, passing hydrochloric acid gas through a boiling solution of sulphuric acid, electrolysing, fractional distillation, distillation with oxydising agents. None of these processes, however, were successful. He next tried oxydising sulphurous acid by passing it over pumice-stone which had been coated with platinum black, but the pumice-stone itself yielded up arsenic. Nitric oxide was next employed as an oxydising agent, the sulphurous acid being prepared from sulphuric acid and copper. Mercury was employed in some of the experiments for the preparation of the sulphurous acid, on account of the arsenic present in copper. As a last resource, crystallised sulphite of soda was employed as a source of sulphurous acid; the nitric oxide being prepared from sulphate of iron, nitric acid, and sulphuric acid. This last process was the only one that furnished an acid free from arsenic. To discover the source of the arsenic, he obtained a specimen of sulphur from a manufacturer of acid which was guaranteed as free from arsenic. This specimen when burnt into sulphurous acid was found to contain arsenic, and even the residue left after combustion contained traces of the metal. In preparing an acid free from arsenic he had found that it was necessary to conduct the operation at as low a temperature as possible.

Dr. Frankland said it would be very satisfactory if sulphuric acid free from arsenic could be prepared in any quantity.

A Member mentioned a modification of Pelouze's method of determining sulphur in iron pyrites, recently published in the Chemical News, which was as follows: Melt the ore with chlorate of potash, chloride of sodium, and carbonate of soda, and determine the excess of carbonate remaining after the experiment. This precaution was, however, necessary; if arsenic were present in the pyrites the arsenic acid formed would neutralise the carbonate, and consequently the calculated per-centage of sulphur would be too high. To rectify this, however, all that was necessary was to use a standard solution of hydrochloric acid for the determination of the alkalinity, and afterwards to precipitate the sulphuric acid by means of chloride of barium, and in this way to determine the sulphur; the amount of arsenic could then be easily calculated.

Mr. Dugald Campbell inquired what was the smallest amount of arsenic that could be detected by Mr. Bloxam's method, for by his own method, which would detect a very small quantity, he had found samples of sulphuric acid which were free from arsenic, but that it was always present in samples of hydrochloric acid, and that it was more important to obtain this latter acid pure. He also stated that he had found arsenic in common salt.

Mr. Bloxam said that he had distilled common salt with sulphuric acid, and had found arsenic in the distillate, but that he obtained none from chloride of ammonium and sulphuric acid.

Mr. F. A. Abel then read a paper "*On a Deposit occurring in Teak Wood.*" He had observed in many specimens of this wood which had come under his notice, deposits of a white substance intersecting the wood, in the form of layers converging towards the centre or heart of the tree. These layers which were sometimes soft and pulverulent, and at other times so hard as to blunt the cutting edges of the saws employed, were often several feet in length, six or eight inches in breadth, and from one to three-eighths of an inch in thickness; they had evidently been deposited in flaws or cracks which had formed in the living tree. Cavities occasioned by knots in the wood were frequently filled with the same white deposits, as were also perforations of considerable size and length constantly found in teak wood, which were produced by a large caterpillar. This white deposit exhibited occasionally a distinct striated crystalline structure, at other times it consisted of conglomerates of small acicular crystals; it was readily soluble in dilute acids with separation of a small quantity of organic colouring matter, a very minute quantity of carbonic acid being evolved.

The analysis of an average specimen of this deposit, dried *in vacuo*, furnished the following per-centage results:—

Lime	34·04
Magnesia	1·86
Ammonia	1·12
Phosphoric acid	43·35

Carbonic acid	trace
Water and a minute proportion of organic matter	19·54

From the relations which the amounts of magnesia and ammonia bear to each other, it was concluded that they were present in the form of the ordinary ammonio-magnesian phosphate, the amount of which would be 11·43 per cent. Deducting the phosphoric acid contained in this salt, the remaining phosphoric acid, the lime, and the water were nearly in the proportion in which they exist in the phosphate of the formula 2 CaO, HO, PO_5+2 Aq. He thought it possible, however, that the deposit might consist of variable mixtures of amorphous and crystalline diphosphate of lime.

Dr. Frankland asked whether the deposit appeared to be diffused through the sound parts of the wood, and if in this case it would at all account for its durable properties.

Mr. Abel replied that although it was diffused through the wood to some extent, he did not think it was the cause of its durability, although other hard woods were found to contain a similar deposit.

The next paper was by Dr. J. Smith, of Sydney, "*On the Composition of the Water of Certain Boiling Springs in New Zealand.*" Some of these boiling springs were intermittent, many of them having formed terraces of silicious matter. He had made an analysis of the water of one of those in which the supply was copious. The water had a pale blue colour, and descended in cascades; it was in a state of violent ebullition. The portion collected became milky after a time, and acquired a faint smell of sulphuretted hydrogen; its specific gravity was 1·00205, and it had a sediment of vegetable matter in it. The following substances were found in the water, the amounts being calculated in grains per gallon:—

Silica	42·40
Soda	16·00
Chloride of Sodium	113·57
Chloride of Potassium	6·67
Sulphate of Potash	5·28
Carbonate of Lime	1·66
Alumina	0·32
Total Residue	189·12
Volatile matter driven off by heat	4 00

The amount of carbonic acid present was small; no boracic acid was found. Other samples from different springs differed a little in composition. The water formed white incrustations on reeds or other plants, the deposit being spongy in some cases, while in others it was dense; it was sometimes tinged with pink, presenting a very beautiful appearance. The water was met with of every temperature up to a few degrees above the boiling point of pure water. When solid bodies were immersed in the water they became petrified, and some plants growing near seemed to have been petrified by the splashes while still living, even the flowers being preserved in some cases. The waters differed slightly in the same locality; for while some were alkaline, others were slightly acid.

Dr. Frankland remarked that it was interesting to find the same changes going on at the Antipodes as on our own side of the globe, these ice springs being analogous to those found in Iceland.

NOTICES OF PATENTS.

983. *Manufacturing Oxygen Gas, and obtaining certain other Products.* J. Webster, Birmingham. Dated April 20, 1861. (Not proceeded with.)

This invention consists in heating mixtures of nitrate of soda or other nitrate with crude peroxide of iron, for the purpose of obtaining oxygen gas, compounds of nitrogen, and the base of the saline material employed.

The method of preparing oxygen here suggested would not be so advantageous as that proposed by M. Deville for obtaining this gas by the decomposition of sulphuric acid by heat.

988. *Bleaching and Refining Oils.* A. V. Newton, Chancery Lane, London. A Communication. Dated April 20, 1861. (Not proceeded with.)

Brown binoxide of lead, either alone or in combination with other materials, is proposed to be employed for the purpose of bleaching and refining oils and fatty substances.

The efficacy of binoxide of lead as a decolorising agent is indisputable, especially when employed together with an acid; but there would always be a tendency for the fatty matter to become contaminated with lead by union with the protoxide.

1006. *Sulphuric Acid.* P. Ward, Bristol. Dated April 23, 1861.

For the purpose of accelerating the formation of sulphuric acid from the mixed gases in the leaden chambers, the inventor employs fragments of sheet glass, or other materials upon which the acid has no solvent action, exposing a large extent of surface, which is kept constantly moist by retaining a film of the acid.

Spongy platinum, and even pumice-stone coated with the reduced metal, are known to facilitate the combination of various gases, and there is no doubt that inasmuch as fragments of glass are found to lend assistance in the union of oxygen and nitrogen, they will exert the same power in favour of the more rapid production, in their presence, of sulphuric acid from the mixed sulphurous and nitrous gases.

1008. *Purification of Coal Gas.* T. Richardson, Newcastle-on-Tyne. Dated April 23, 1861. (Not proceeded with.)

This proposition consists in reducing the crude oxides of iron, left as secondary products when pyrites ores are used in the manufacture of sulphuric acid, by the action of carbonic oxide, hydrogen, or other deoxidising gas employed at a high temperature; and in utilising the resulting material in the purification of coal gas.

The efficacy of the proposed material would be considerably augmented by allowing the reduced iron to become rusted by the action of a moist atmosphere, an improvement which could not, however, be carried out without infringing the patent rights of Mr. Hills, whose claim to the employment for these purposes of the hydrated oxides of iron has recently been proved in a Court of Law.

1019. *Artificial Manures.* C. Stevens, Charing Cross, London. A Communication. Dated April 24, 1861.

With the object of preparing a manure which shall contain a variety of fertilising ingredients, a mixture is compounded of the materials, and in the proportions, undermentioned:—

Woollen rags	100 parts.
Soda crystals	90 ,,
Lime (slaked)	30 ,,
Sulphate of iron	1 ,,
Sulphate of magnesia	2 ,,
Sulphate of ammonia	10 ,,
Superphosphate of lime	35 ,,
Water	200 ,,

1084. *Treatment of certain Ores Containing Metals.* R. Laing, Ince, near Wigan, and J. Swindells, Wigan. Dated May 1, 1861.

For the purpose of extracting copper and other metals from their ores, the patentees prefer to employ acids in the

gaseous state, and to economise for this application the spent gases evolved from the sulphuric acid chambers, and sundry other products, particularly the waste hydrochloric acid generated in the manufacture of "salt-cake" in alkali works. Soluble compounds of copper or other metal are produced which may be extracted from the residual ore by washing.

1072. *Certain New Chemical Combinations, and for the Application thereof to Fixing Aniline and Pigment Colours in Printing and Dyeing, and other Industrial Purposes.* J. Meyer, Berlin. A Communication. Dated April 30, 1861.

This invention consists in the combination of certain organic substances, such as fibrine, albumen, gelatine, animal tissues, and other similar materials with the oxides or salts of tungsten and molybdenum.

It is very probable that the tungstate of soda, in conjunction with any one of the animal matters enumerated, will operate as a mordant and be susceptible of the same applications as the stannate of soda.

1087. *Colouring Matters Derived from Naphthaline, and Application of the same to the Dyeing and Printing of Fabrics.* F. Z. Roussin, Paris. Dated May 1, 1861. (Not proceeded with.)

The inventor prepares in the first place nitro-naphthalase by the action of nitric acid on crude naphthaline. This product is then dissolved in hydrochloric acid and submitted to the reducing influence of nascent hydrogen, &c., by contact with tin (or pewter), in which reaction the base naphthalidine is formed. The hydrochlorate of this last named substance is then finally brought into contact with nitrite of potash or hyponitric acid, either of which leads to the production of a red colouring matter, tolerably permanent, and applicable in the dyeing processes.

In the earlier stages of this method of preparation, compounds are formed which were described many years ago by Zinin. The naphthaline products have lately been made the subject of renewed investigation, and promise to furnish a valuable series of colouring matters.

1092. *Fixing Colours in Connection with the Printing and Dyeing of Woollen Fabrics and Yarns.* R. T. Pattison, Daldorch House, Ayr. Dated May 1, 1861. (Not proceeded with.)

This invention refers to the employment of the alkaline earths or soluble salts, without being combined with tannin or other organic materials, for fixing certain colours derived from the products of coal tar, upon yarns and woven fabrics in the processes of printing and dyeing.

1099. *Stearine.* E. De Bassano and A. Brudenn, Brussels. Dated May 2, 1861. (Not proceeded with.)

In the process of distilling the crude fatty acids as a means of purification, the inventors recommend the addition of powdered wood or animal charcoal, lamp black, or coal, for the purpose of retaining certain empyreumatic substances which are apt to distill over and contaminate the product.

1109. *Manufacture of Paper and Cardboard from a Fibrous Vegetable Matter,* M. A. F. Mennons, Paris. A Communication. Dated May 3, 1861.

This improvement consists in manufacturing paper from the concretions of vegetable matter known as "pommes de mer," or marine apples, and which, formed by the rolling action of the sea on a certain variety of alga (*Zostera marina*), are found in abundance on the shores of the Mediterranean.

1113. *Electric Telegraphs.* O. Rowland, Wellington Road, Kentish Town, Dated May 3, 1861. (Not proceeded with.)

The suspended conducting wires of the electric telegraphs whether of iron or steel, are, according to this invention, protected from the atmosphere by being coated with antimonial lead, or with alloys of tin and lead, or the latter metal with addition of antimony.

The expense attending the application of these metallic coatings would be greater than is the case with zinc, but the ordinary galvanizing seems but ill-fitted to protect the iron wire from the effects of atmospheric exposure.

CORRESPONDENCE.

Mounting Microscopic Objects.

To the Editor of the Chemical News.

Sir,—Perhaps some of your subscribers may know, and, if so, will be kind enough to inform me, how I may prepare a solution of silicate of potash or soda fit for mounting microscopic objects. I have found it, in many respects, suitable for the purpose, as the exuding portion becomes semi-solid in a few minutes, when it may be removed with a knife; and the varnish adheres without any of the difficulty and loss of time so often experienced with preservative solutions, and more especially glycerine; nor is there any danger of its running in. But I was disappointed to find that, even when the edges of the thin glass cover were varnished, it always became milky or crystallised, sometimes in a few hours, sometimes not for a week.

It is recommended in the "Micrographical Dictionary" on account of its low, refractive power; but the author also states that his specimens became opaque, which he attributes to excess of alkali.

I should not venture to ask for so much of your valuable space, did I not believe that a receipt for a silicate which will remain transparent will be a great boon to microscopists. I am, &c.

A. H.

Fusibility of Oxide of Cadmium.

To the Editor of the Chemical News.

Sir,—All chemical books agree that oxide of cadmium is infusible. We have found it to be otherwise. Some nitrate of cadmium being precipitated with carbonate of soda, the resulting carbonate of cadmium was thoroughly washed with hot water, and dried. The white powder, after being well ground, was put into a clay crucible and strongly heated in a fierce coke furnace. In a few minutes the usual conversion into brown oxide took place. In about half-an-hour, however, a red-hot mass was formed. This, when cold, was of a blackish colour, speckled on the surface with brown; and could only be got out of the crucible by breaking the latter, portions of which adhered with great tenacity.

Fused oxide of cadmium is very heavy, and yields, on grinding, a brownish-grey powder, not so readily soluble in acids in the cold as the ordinary brown oxide.

We are, &c.

Thomas Salter, F.C.S.
Henry Matthews, F.C.S.

February 11.

Chemical Notices from Foreign Sources.

I. ORGANIC CHEMISTRY.

Kreatinine in Urine.—Dr. C. Neubauer has demonstrated (*Ann. der Chem. und Pharm.*, Bd. cxix., s. 27,) that Kreatinine is a normal ingredient of urine, always present in about the same proportion as uric acid. He separated it by forming a double compound with chloride of zinc.

Temperatures at which Starches Dissolve.—Lippman (*Journ. für Prakt. Chem.*, Bd. clxxxiii., s. 51,) has determined the temperatures at which the granules of various starches burst, and paste is formed. We extract a few of the most common. The column A gives the temperature at which the granules begin to swell; B, that at which paste commences to form; C, that at which paste is completely formed. The degrees are, of course, Centigrade.

	A	B	C
Rye starch	45·0	50·0	55·0
Maize	50·0	55·0	62·5
Potato	46·25	58·75	62·5
Rice	53·75	58·75	61·21
Arrowroot (*Arum Maculatum*)	50·0	58·75	62·5
Tapioca	—	62·5	68·75
Wheat starch	50·0	65·5	67·5
Arrowroot (*Marantha Arundinacea*)	66·25	66·25	70·0
Sago	—	66·25	70·0

Amylaceous Matter in Fruits.—It is asserted by Pelouze and Fremy that starch cannot be detected in green fruits, either by means of the microscope or by iodine. M. Payen shows (*Comptes-Rendus*, t. liii. p. 813) that it can be easily recognised by iodine in the following way:—He takes a thin slice off a growing pear, apple, or quince, plunges it under water to avoid the action of the air, and to wash away soluble matters, and when the washing is complete, puts it into a weak alcoholic solution of iodine. In an hour or two an intense blue colouration is produced. He also recognised starch granules by the microscope. One curious fact observed was, that as the fruit ripened the starch first disappeared from the neighbourhood of the peduncle.

Gases given off by Plants under the Influence of Light.—M. Boussingault has discovered (*Comptes-Rendus*, t. liii., p. 862) that under the influence of direct sunlight the leaves of aquatic plants give off a notable proportion of carbonic oxide and carburetted hydrogen. He thinks that this emanation of carbonic oxide may be one of the causes of the unhealthiness of marshy districts. The fact he points out is important, and the subject will, no doubt, receive further investigation.

Ethyl and Methyl Compounds of Tellurium.—Our readers will find in the *Chemisches Centralblatt*, No. 58, 1861, a long account of the above compounds extracted from an inaugural dissertation by Dr. Max Heeren. Experimenters may be glad to be informed that the most remarkable property of the tellurium methyl compounds is their intolerable and persistent odour. A small quantity, either of tellurium-methyl, or of a salt, allowed to get on the finger, soon communicates a smell to the whole body, and in a few days is perceptible in the breath. The stench is so lasting that the unfortunate chemist is shut out from society for several months.

MISCELLANEOUS.

Chemical Society.—The next meeting of this Society will take place on Thursday the 20th inst.

Royal Institution.—The following Lectures will be delivered in the ensuing week:—Tuesday, February 18, at three o'clock, John Marshall, Esq., "On the Physiology of the Senses." Thursday, February 20, at three o'clock, Professor Tyndall, "On Heat." Friday, February 21, at eight o'clock, James Fergusson, Esq., "On the Site of the Holy Sepulchre at Jerusalem." Saturday, February 22, at three o'clock, Rev. A. J. D'Orsey, "On the English Language."

Borax in Milk.—Professor Kletzinsky states that borax is often employed, to prevent milk from turning sour and to impart to it more consistence so as to make it appear more cream-like. This addition is detected in the ashes by boiling them with alcohol acidulated with sulphuric acid; the presence of boracic acid is ascertained from the brown colour of curcuma paper and from the green flame of the burning alcohol.—*Polyt. Centralblatt*, 1861, 224.

To recognize Grape Sugar beside Cane Sugar.—O. Schmidt employs triacetate of lead and ammonia, which produce with both sugars white precipitates, which after a while, particularly when heated, assumes a red colour in the presence of grape sugar, but remains unaltered by cane sugar; a small quantity of the former mixed with a large proportion of the latter may thus be recognized by the red tint of the precipitate.—*Ann. der Chem. und Pharm.* cxix. 102.

Influence of Silicic Acid on Fermentation.—J. C. Leuchs states that silicic acid precipitated from water glass, produces fermentation in saccharine solutions, particularly after the addition of some tartaric acid, and generates the odour of beer yeast, afterwards of fruits, and finally of ether; in very dilute solutions the odour of putrid yeast appears. Silicic acid does not lose this property by boiling with water or by repeated employment for fermenting and subsequent washing with water. A solution of sugar, containing alcohol and tartaric acid, fermented briskly with silicic acid, from which the gas was evolved, and amid the separation of a yeasty foam.—*Portfolio.—Dingler's Polyt. Journ.* clxi. 400.

Protosulphate of Manganese, free from iron, is prepared by Delffs, by treating black oxide of manganese with washed sulphurous acid gas, which does not take up a trace of iron. Other bases besides iron must be removed by treating previously with dilute nitric acid.—*Zeitschr. f. Ch. und Pharm.* iii.

Test for Gaseous Sulphurous Acid.—Hugo Schiff employs paper moistened with solution of protonitrate of mercury which instantly assumes a gray colour from reduced mercury; it is requisite to test with lead paper for sulphuretted hydrogen. Both gases are not present at the same time, as they decompose each other.—*Dingler's Journal.*

Death of M. Biot.—We have to record the decease of Mons. Jean Baptiste Biot, the eminent French chemist, and the Father of the Academy of Sciences at Paris, which happened on Tuesday week, at the age of eighty-eight. Born in 1774, he early devoted himself to the study of chemistry and natural science, and owing to his great proficiency, was elected into the French Academy so far back as 1803, when less than thirty years of age. The Father of Modern Science in Paris, and beloved by all who knew him, M. Biot's life has been singularly uneventful; only a year ago he was described as being still as devoted to his favourite pursuits as ever, and as having recently contributed to "Les Annales de Chemie," an introduction to some "Researches in Mechanical Chemistry, in which Polarized Light is employed as a re-agent by way of an Auxiliary." He has also experimented on various solutions, and in the "Annales" given accurate and interesting tables of results. M. Biot's death will cause in Parisian society a void which will not readily be filled up.

ANSWERS TO CORRESPONDENTS.

*** All *Editorial Communications* are to be addressed to the Editor; and *Advertisements* and *Business Communications* to the Publisher at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

Obliged Correspondent.—We will give the desired information in our next number.

J. Walker.—We shall always be pleased to receive accounts of new facts, but we cannot undertake to insert mere discussion. The matter had better drop.

Hart.—We do not know the address. Apply to Mr. Ladd, whose address will be found in our advertising columns.

F.R.S.—The process was published in our second volume. You have probably not seen it, or you would have been saved some trouble.

THE CHEMICAL NEWS.

VOL. V. No. 116.—February 22, 1862.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Adulteration of Butter with Animal Fats, by EDWARD BALLARD, *M.D. Lond., M.R.C.P., Medical Officer of Health for Islington, and Parochial Analyst under the Adulteration of Food Act.*

[FOURTH COMMUNICATION.]

IN my last communication I detailed the principal phenomena of solidification of a sample of butter, beef dripping, mutton dripping, and lard. But as our object is to ascertain in what way pure butter may be distinguished from the adulterated, I cannot do wrong in adding briefly the results of similar experiments upon other samples of butter.

Fifth Experiment.—Pure butter (purchased). The physical characters were detailed in my last paper. 20 grains left 1·8 grains of residue when heated for an hour with ether at 65°.

On melting in the tube an abundance of air-bubbles rose slowly and formed an abundant froth on the top. (I ought not to have omitted this among the characteristics also of the butter which was the subject of the first experiment.)

At 171° the liquid was cloudy from the presence of a strongly-marked generally-diffused flocculence. Thermometer only obscurely visible, but here and there, between the flocculi, the mercurial column could be seen. These flocculi rapidly subsided and very perfectly, so that in

25 minutes and at 120° they had formed a dense deposit at the bottom of the tube about the bulb, while all above was bright and clear. After

83 minutes and at 78° the bright clearness of the liquid was somewhat dimmed; and after

104 minutes and at 72° it was decidedly more cloudy, and minute points had begun to appear in it, but the markings on thermometer were still quite legible. After

115 minutes and at 70° these opaque points had so much increased in abundance and size, that the readings on thermometer were becoming obscure. After

120 minutes and at 69° they were still legible; but after

125 minutes and at 68° were not only illegible, but scarcely visible. After

137 minutes and at 66° the thermometer itself was invisible except in a narrow line at the top of the liquid, the presence of which showed a certain gravitation of the opaque points. After

155 minutes and at 64° the loaded tube could be raised one inch out of the water, but then slipped. After

165 minutes and at 63° it could be raised no higher, and was now left. When examined again, at the expiration of

210 minutes, and at 59°, it could be raised completely out of the water and oscillated.

Sixth Experiment.—Butter, newly churned and unsalted, procured from a farm near Hatfield, of a pale straw-colour and sweet butyraceous odour and taste, brittle at 40°. When melted in the beaker with boiling water and stirred, the liquid came up very frothy, and very delicately cellulated, and opaque-looking at the edge. When cold and dried it broke down on pressure into a fine mealiness of pure butyraceous taste. Residue from treatment with ʒj. of ether for an hour at 65°, 4 grains. Raised in the tube to

169°, an abundant froth arose occuping the extent of about ¼ inch. The liquid was bright and clear, only a small amount of flocculi scattered in it, but the markings on the thermometer were quite clearly legible. After

18 minutes and at 127° the flocculi had mostly fallen to the bottom of the tube. After

21 minutes and at 121° the whole of the liquor was somewhat dimmed, the lower part more distinctly so than the upper. After

41 minutes and at 109° this dimness had increased to a decided milkiness, but the markings were still legible. After

68 minutes and at 88° this had increased so much that the markings were illegible, although the mercurial column and numbers were still visible. The milkiness was *not* punctiform. After

90 minutes and at 78° the markings on thermometer were scarcely visible, but a trifling clear line had appeared as to the top of the liquid. After

106 minutes and at 73° points were observed in the clouded liquid which, after

116 minutes and at 72° rendered both the numbers and mercurial column invisible. After

121 minutes and at 71° the thermometer was obscurely visible only at the upper part, and was sufficiently fixed to permit the loaded tube to be moved about in the water by means of it. After

131 minutes and at 69°, the thermometer being for the last five minutes obscured from view, the loaded tube could be raised one inch out of the water; and after

156 minutes and at 65° it could be raised completely out and oscillated without slipping.

Seventh Experiment.—Butter newly churned and unsalted, procured from another farm near Hatfield, of a pale straw-colour, and sweet butyraceous odour and taste. Crumbly at 40°. When melted with boiling water in the beaker and stirred, the liquid came up very frothy and finely cellulated, a very few larger cellulations interspersed; edge of melted liquid opaque-looking. When the cake was removed and dried it broke down into *very fine* grains, one or two rather larger grains being here and there visible. Taste purely butyraceous. Residue from ʒj ether, 5·3 grains. Raised in tube to

170° the liquid was so opaque that the thermometer could scarcely be seen. The opacity was occasioned by small opaque flocculi and flocculent points. An abundant froth rose to the surface. After

20 minutes and at 125° the flocculi had aggregated very decidedly; the thermometer was more visible, and the commencing formation of a clear line at the top indicated commencing gravitation. After

83 minutes and at 78° the "clear" layer had nearly attained the depth of half an inch, and now became slightly dimmed. After

104 minutes and at 72° fine points appeared in the "clear" layer. After

116 minutes and at 70° these had become much larger and more numerous, but the markings on thermometer were still legible. After

126 minutes and at 68° they were illegible, but with mercurial column still visible. After

131 minutes and at 67° they were all invisible. After

145 minutes and at 65° the thermometer, having been for some time invisible, was not yet sufficiently fixed to enable the loaded tube to be raised to the surface of the water. It was now left until the lapse of

181 minutes, when at 61° it could be raised out of water, and oscillated without slipping.

Eighth Experiment.—Butter newly churned and unsalted, procured from a third farm near Hatfield, was of a much paler straw colour, but of sweet butyraceous odour and taste; firmer and less brittle than the last two samples at 40°. When melted with boiling water in the beaker and stirred, it came up very finely cellular indeed, and opaque-looking at the edge. When cold and dried it broke down into a very fine mealiness, of pure butyraceous taste. Residue from 3j. of ether at 65° 3·2 grains. Raised to

170° it was very frothy on the surface and liquid was cloudy from flocculent points and some large flocculi, but the mercurial column and figures on thermometer were visible, though the latter could not be read. After

5 minutes and at 158° the solid matter in the liquid had become distinctly flocculent, and a clear line was beginning to form at the top; and after

18 minutes at 128° the flocculi were large and scattered in a bright clear liquid, while the markings on thermometer could be read clearly. After

36 minutes and at 105° the flocculi had become almost all deposited about the bulb, leaving the liquid nearly free from them. After

76 minutes and at 81° liquid slightly dimmed. After

89 minutes and at 77° points were visible in the clear layer, and after

102 minutes and at 74° these had increased sufficiently to render the markings on thermometer scarcely legible. After

107 minutes and at 73° they and mercurial column were quite invisible. After

114 minutes and at 72° the thermometer was invisible, but not yet fixed. After

193 minutes and at 62½° the loaded tube could only now be raised one inch out of water, and then slipped. After

216 minutes and at 60½° no more complete fixing had taken place. It was now left.

It will be well to summarise the results of the experiments upon these five samples of pure butter. First: in all of them there were two kinds of solid matter, the one flocculent, appearing either as flocculi from the first or becoming so very early in the cooling. These flocculi took different appearances, were of different amounts, and deposited more or less differently, and with different degrees of rapidity in the several samples. In fact, they are accidental ingredients of butter, consisting of particles of caseine, which element of the butter is not entirely separated by the previous process of washing. In order to determine this, the etherial residue in the three last samples was boiled with ether, and the following result was obtained:—

	Grs. of residue.		Grs. of caseine.	Grs. of washed butter.
With that giving	4	at 65° .	·2	in 20
"	5·3	at 65° .	·6	in 20
"	3·2	at 65° .	·4	in 20

It cannot, therefore, be said that the residue from ether at 65° consists of this impurity, as the latter only existed in it to a very small amount, and the residue was not even proportional to the amount of caseine it contained. But the proportion of caseine in the several samples of butter did agree with the degree of flocculence observed in the newly melted samples. Second: the other solid matter which appeared was the margarine of the butter, which deposited at a certain temperature from its solution in the liquid elements of the butter. Its first deposition was marked by the earliest indication of dimming in the clear liquid, and more strongly by the first formation of opaque points in it. The "richness" of butter is partly dependent on these liquid elements, and is hence so far proportional to the lowness of the temperature at which the margarine deposits from it. Third, the temperature at which the various changes in appearance of the melted butter took place appears in the following table:—

Experiment.	Clear liquid dimmed.	Opaque points.	Thermometer invisible.	Raised 1 inch out of water.	Completely raised and oscillated.
First	—	75°	69°	68°	55°, and probably higher
Fifth	78°	72°	66°	64°	59°
Sixth	121°	73°	70°	69°	65°
Seventh ..	78°	72°	66°	—	61°
Eighth ..	81°	77°	72°	62½°	below 60°

The highest temperature at which dimming of the clear liquid occurred was 121°, which is close upon what is regarded as the melting point of margarine (118°). In the other cases it occurred at much lower temperatures. The highest point at which the opacity became punctiform was 77°. The highest point at which the thermometer became invisible in the "clear" layer was 72°. The highest temperature at which it was so fixed as to allow of the loaded tube being raised so that the surface of the butter in it stood one inch above the level of the water in the beaker was 69°. The highest temperature at which it became so firmly fixed as to allow of its being raised completely out of the water and oscillated without slipping was 65°. The shortest time which elapsed before the final result was attained was 156 minutes, but mostly it was much longer.

I proceed now to the results of similar experiments with butter (that used in first experiment) adulterated with beef fat, mutton fat, and lard.

Ninth Experiment.—Butter *adulterated* with one-fourth of beef dripping (*e* of Second Communication), residue from 3j. ether 4·0 grs. Raised to

170° the liquid presented little frothiness; a trifling milkiness was observable through which the markings on thermometer were quite legible. After

25 minutes and at 120° the milkiness was in the form of very fine points. After

60 minutes and at 90° these points had become more distinct, but not flocculent; the upper markings on thermometer were slightly clearer than the lower ones. After

80 minutes and at 80° the markings were more obscure, but still legible; opaque points larger, more distinct and generally diffused. After

85 minutes and at 79° the markings were illegible; bulb and lower part of stem of thermometer more obscured than the rest. After

88 minutes and at 78° numbers and mercurial column invisible. After

96 minutes and at 76° thermometer wholly invisible, but not yet fixed. After

111 minutes and at 73° the loaded tube could be raised nearly above the water. After

115 minutes and at 71° it could be completely raised out of the water, and oscillated without slipping.

In an experiment in which the same butter, but a different sample of dripping, was used, the points of the original cloudiness aggregated into flocculi at 140°, which gravitated as in pure butter, leaving a narrow, clear, bright layer, which began to become cloudy at 75°. The loaded tube could be raised out of water, and oscillated without slipping at 72°.

Tenth Experiment.—Butter adulterated with one-fourth of mutton dripping (*f* of Second Communication), residue with ʒj. ether 4·0 grs. Raised to

171° only trifling frothiness; liquid turbid from generally diffused minute points; thermometer only just visible. After

13 minutes and at 135° points aggregating into an universal fine flocculence; thermometer slightly more distinct. After

39 minutes and at 105° a clear layer had formed at top of liquid by subsidence of the flocculi, to the extent of about $\frac{1}{5}$th inch. After

99 minutes and at 78° this clear layer had arrived at a depth of nearly $\frac{2}{3}$rds inch, and the clear bright liquid was beginning to become slightly dimmed. After

108 minutes and at 77° fine opaque points began to appear in it. After

115 minutes and at 75½° they had increased so much as to render the markings on thermometer illegible, though still visible. After

123 minutes and at 74° the stem of thermometer was itself invisible; and after

143 minutes and at 72° the loaded tube could be raised out of the water and oscillated, even before the top portion had become absolutely impervious to light.

This specimen was re-melted on the following day, and on cooling gave similar results.

Eleventh Experiment.—Butter adulterated with one-fourth of lard (*g* of Second Communication), residue from ʒj. ether 2·0 grs. Raised to

173° there was very little frothiness; liquid turbid from presence of fine points; thermometer visible, but not the markings on it. After

6 minutes and at 157° the turbidity was assuming the form of a fine flocculence, and the thermometer was now distinctly visible. After

41 minutes and at 105° the fine flocculence had gravitated sufficiently to form a narrow clear layer at top of the liquid; and the mercurial column was visible in places between the flocculi. After

125 minutes and at 76°, during which time the subsidence of the flocculi had been going on, the clear bright layer had reached nearly 1 inch in depth, and at this temperature began to become somewhat dimmed. After

132 minutes and at 75° the mistiness was observed to be in fine points; the markings on thermometer still legible, but mistily. After

141 minutes and at 74° the stem of thermometer in upper part still obscurely visible, and the loaded tube could be raised by the thermometer to the level of the water; at 73° the stem was invisible; and after

148 minutes and at 73° the loaded tube could be raised 1 inch above the level of the water; and after

168 minutes and at 70° it could be supported completely out of the water, and oscillated without slipping.

Another experiment was made with butter adulterated with an equal weight of beef fat (*d* of Second Communication). Flocculi formed and deposited, leaving a clear bright liquid, which after

85 minutes and at 79° was so opaque with minute opaque points, that the markings on the thermometer were scarcely legible. After

88 minutes and at 78° it had so rapidly increased, that only the stem of thermometer, at the upper part of tube (above deposited flocculi), was obscurely visible; and after

96 minutes and at 76° the thermometer being lost to view, the loaded tube could be raised about 1½ inch above level of water; and after

116 minutes and at 71° it could be supported completely out of the water, and oscillated without slipping.

From all these observations it appears that the consistency of a sample of butter may be used as one of the tests of its adulteration with foreign fats.

1. These samples of adulterated butter were all much less frothy when melted than the pure samples.

2. The lowest temperature at which the "clear" liquid became dimmed was 75°. This, then, is useless as a test.

3. The lowest temperature at which the opacity became punctiform was 75°. This, then, is only a test of purity when the points appear at lower temperatures, such as 72° or 73°.

4. The lowest point at which the thermometer became invisible in "clear" layer was 73°. This is 1° higher than the highest temperature at which it became lost to view in the pure butter examined, and 7° higher than the lowest. The obscuring of the thermometer at much lower temperatures, then, may be assumed to be an indication of purity, and, at higher temperatures, of adulteration.

5. The lowest temperature at which the loaded tube could be raised so as to bring the level of the butter one inch above that of water in the beaker was 73°. This is 4° higher than in any sample of pure butter examined, and it is to be regarded as a test of adulteration when the temperature at which the tube can be thus raised is 73° or upwards.

6. The lowest temperature at which the loaded tube could be raised and supported thoroughly out of the water was 71°. This is 6° higher than that at which any sample of pure butter examined could be similarly supported, and hence we may probably conclude that when butter is thus supported at 71° or upwards it is adulterated with at least one-fourth of some foreign fat.

7. The greatest number of degrees requisite for the temperature to fall from the appearance of opaque points to the final complete fixation of the thermometer was, in samples adulterated with one-fourth of mutton fat, beef fat, or lard, 5°. (The ninth experiment is exceptional, inasmuch as the clouding was punctiform almost from the first, no clear layer being formed at all.) In the sample adulterated with an equal weight of beef fat it was 8°; but here the points appeared at 79°.

The smallest number of degrees of fall in temperature between the appearance of opaque points and complete fixation in the unadulterated samples was 8°; but in

this sample the points did not appear till 73°. In other samples the fall requisite varied from 11° to 20°.

8. The greatest length of time which elapsed before complete fixation of the thermometer was 168 minutes. This, then, is only a test of purity when the length of time is considerably in excess of this; and of impurity, when considerably short of it. [The temperature of Laboratory varied from 45° to 60°.]

Islington Parochial Laboratory, January 28.

On the Preparation of Silicium, by J. Robbins.

A short time ago, requiring rather a large quantity of pure silicium, I made some according to the method usually described in chemical works,—viz., by heating together potassium with the double fluoride of silicium and potassium. Sodium at the present time being comparatively cheap, I thought it worth a trial to see if it could not be substituted for the more expensive metal, potassium. The experiment was, therefore, made, using the former precisely in the same manner as the latter. The reaction with sodium appeared just as energetic, and the fused mass indicated a favourable result; but, after washing in hot and cold water, and drying, I obtained a light grey powder totally different from that obtained by potassium.

Deville has published a process by which sodium may be advantageously substituted for potassium by introducing, at the same time, aluminium; by this process the product is obtained in a crystalline form, and is, I believe, the one generally adopted in France for obtaining *diamant de silicon* (crystallised silicium). The aluminium and sodium are ordered to be cut up into small pieces, and well incorporated with the fluoride of silicium and potassium. On applying heat, combination takes place, the fused aluminium is separated, digested in hydrochloric acid, and washed, the residue being pure crystallised silicium.

Deville being so much interested in aluminium, and always having it at hand, naturally gives it the preference, although he says zinc may be substituted for it and employed in the same manner, first reducing the zinc to a fine powder, and intimately blending it with the double fluoride. In consequence of the high price of aluminium, I selected zinc for my experiment, but, in procuring crystals, did not meet with the success I anticipated; at the end of the process the zinc, instead of being fused into a button, remained disseminated throughout the mass; after digesting in hydrochloric acid very good amorphous silicium was obtained. It then struck me that a better plan would be to place the sodium and zinc powder together, and surround them completely with the double fluoride, as probably the two metals would first combine together, and afterwards re-act on the powder. Whether this theory be correct or not, in practice it answered perfectly. I proceeded as follows:—A crucible was about two-thirds filled with fluoride of silicium and potassium, a hole was made in the centre pressing the powder against the side of the vessel; zinc, in fine powder, was then thrown in, on which was placed the sodium; the remaining zinc was added so as to completely surround the sodium. The crucible was then nearly filled with the double fluoride. On application of heat combination takes place energetically, but without loss of sodium, if well covered with the powder. The zinc is found fused into a mass at the bottom filled with acicular crystals of pure silicium, which are readily separated by hydrochloric acid. The proportions employed were, sodium 1 part, zinc 4 parts, fluoride of silicium, and potassium a sufficient quantity. An excess of the latter is of no consequence, as it can be readily separated at the end of the process, and may be used in a second operation.

On the Preparation of Silicium, by Captain Caron.

A mixture is made of

Dried silico-fluoride of potassium . .	300
Granulated zinc	400
Sodium, in small pieces	80

These proportions are not absolutely necessary, but they seem to give the best yield of silicium. The mixture thus made is projected into a crucible, which, along with its cover, is red-hot. The reaction is brisk, although, when the cover is not sufficiently hot, it is often necessary to press the mixture with a clay pipe. When the whole is liquid, the crucible is removed and allowed to cool. It is necessary to execute the operation as rapidly as possible, otherwise the crucible is liable to be perforated, and part of the zinc and silicium lost.

The cooled crucible is broken, to extract the ingot of zinc, which will have settled down well if the operation has been successful; the crystallised silicium is almost entirely at the upper part of the zinc. The pieces of the crucible and scoriæ adhering to the regulus are removed, and the latter is melted at as low a temperature as possible, so that the zinc is liquid while the silicium is solid. The zinc is run out and granulated, and can be used for another operation; crystals of silicium remain in the crucible surrounded by a little zinc. This residue is treated with concentrated hydrochloric acid, which removes zinc and iron, and crystallised silicium is left still containing a little lead (if the zinc was not quite pure), and always a little protoxide of silicium. The lead is removed by boiling with strong nitric acid and washing, and the protoxide of silicium, as well as any of the mass of the crucible, are removed by treatment with hydro-fluosilicic acid. The pure silicium which remains is washed with water and dried.

To melt this silicium, it is mixed with silico-fluoride of potassium and placed in a double crucible, having been previously covered with a thick layer of coarsely-powdered glass. It is next heated to the melting-point of iron for some time, and is then immersed, while hot, in cold water, in order to render the glass more friable. The crucible is then carefully broken, and the globule of silicium is found surrounded by glass, which is easily removed either by a hammer or by means of a sharp-pointed steel. To purify it completely, it must be boiled for some time with concentrated hydro-fluoric acid, which completely removes any slag, provided it is not in the centre of the regulus.

The only acid which attacks melted or crystallised silicium is nitro-fluoric acid.—*Phil. Mag. and Ann. de Chim. et de Phys.*

On the Analyses of Aluminous Minerals, or Bauxite,[1] *by M.* H. Sainte-Claire Deville.

These substances may contain silica, titanium, corundum, alumina, phosphoric acid, sesquioxide of iron, vanadic acid, carbonate of lime, and water.

[1] *Annales de Chim. et de Phys.*, lxi. p. 318.

Water and Calcareous Matter.—The water is estimated by heating the substance to dull redness (it is entirely driven off at this temperature), and the limestone by cold digestion with weak hydrochloric acid.

Silica.—The substance freed from lime, either after calcination or hydrated, is treated with sulphuric acid of a density of 1·6°, evaporate until the sulphuric acid begins to evolve vapours. If the action is not perfect, add a little water and evaporate again in the presence of a sufficient excess of acid. The alumina, iron, and titanium dissolve, whilst the silica remains with a little corundum. Separate with warm water, and a prolonged digestion with pure water—for it must be remembered that, according to a very curious observation of M. Persoz, sulphate of alumina heated with an excess of concentrated sulphuric acid only dissolves with difficulty in pure water; ebullition must also be avoided, as this might precipitate a little titanium. The silica being well washed, ignited, and weighed, is treated with dilute hydrofluoric acid, which instantly dissolves it, leaving a little corundum, which is to be also weighed.

Titanium.—The titanium may be readily separated from its sulphuric solution by precipitating it with the iron and alumina by means of ammonia, and evaporating the whole to dryness in a platinum dish; extract with water acidulated with nitric acid, which dissolves the iron and alumina, and leaves the titanium; then weigh. The evaporation ought to be performed on a sand-bath in a large platinum dish, and followed by a calcination at 300° or 400°, which renders the titanium insoluble.

Alumina and Iron.—The liquid then contains alumina, iron, sulphuric acid, nitric acid, and ammonia. Transfer it to a glass flask in which a tolerable quantity of aqua regia is poured, boil till the complete cessation of nitrous vapours, and evaporate until it is quite certain that there is no more aqua regia present. The ammonia will then have disappeared, and there will remain only sulphuric acid, nitric acid (which must be carefully kept in excess), alumina, and iron; transfer to a platinum crucible, evaporate to dryness, and ignite until all the sulphuric acid has been driven off by the heat. Then weigh the mixture of alumina and iron.

Alumina.—This mixture is weighed, either entire or partially, in a platinum boat contained in a glass tube (see *Ann. de Chim. et de Phys.* Third Series, xxxviii. p. 21), and treated with hydrogen and gaseous hydrochloric acid in a platinum tube heated by gas or in a porcelain tube, according to the method described in the above memoir. Pure alumina remains, which must be weighed; this will suffice, knowing the weight of the mixture of iron and alumina, to calculate the proportion of iron volatilised by the hydrochloric acid and, consequently, of the sesquioxide of iron, which enters into the composition of the mineral.

Abridged Method.—If the titanium can be neglected, it is sufficient to treat with hydrogen and gaseous hydrochloric acid the dried mineral after removal of the carbonate of lime. The quantity of the sesquioxide of iron which it contains is thus determined. Knowing from the attack with sulphuric acid the quantity of silica, the difference gives nearly the proportion of the alumina to the titanium.

Phosphoric Acid. — The determination of this element, which exists in very minute proportion in the mineral, is almost impossible, on account of the large quantity of alumina with which it is mixed. The method which I have found the most exact is to reduce the oxide of iron in a charcoal crucible with a very silicious and alkaline flux, and to analyse the cast iron obtained for phosphorus. An example of this is given further on.

Vanadic Acid. — I have already described the method of extracting the vanadium from the mineral itself; it is by this process, imperfect though it be, and operating on 100 grammes of material, that I have determined its proportions.

All my analyses give a sum equal to 100 upon adding together the per-centage results, even when one of the substances has not been determined by difference; this is due to the process adopted, which consists in determining in the same platinum tube first the water, then the iron, and, lastly, the silica, alumina, and titanium united. Such a method cannot give any loss. On another portion of the substance the silica and titanium is determined, and, consequently, the alumina, since the total of these substances is already known *by one weighing*. This method is very accurate, on account of the numerous controls which it presents; and it has this advantage, that the minute errors, be they more or less in the estimation of the silica and titanium, of which the proportion is very small, are transferred to the dominant substance, the alumina of which the quantity is such that the relative error resulting from the faults of analysis is absolutely insensible. I recommend young men who are practising analysis to employ by preference processes which give with accuracy the proportion of substances present in small quantity, when they are of importance, even at the risk of transferring to the dominant substances the small errors arising from the first determinations.

Aluminous Minerals Containing Iron.—Among bauxites there are some which contain a large proportion of iron, and which have been tried to be smelted in blast furnaces. The refractory nature of the slag arising from the presence of a large quantity of alumina, has occasioned the search for these minerals to be abandoned. On the other hand, I learn that, in certain works near Berry, a small proportion of these Southern minerals is added to the charge. The improvement of the cast iron which is observed is due, undoubtedly, to the presence of vanadium in the aluminous materials. Moreover, Captain Caron has found that one of the best iron minerals of Cher contains considerable quantities of vanadium as determined by the methods which I have described. It will, therefore, be very useful to detect the presence of this element in either ether, in cast iron, or in its minerals. For that it is merely necessary to treat at a red heat by caustic soda the minerals, or the cast iron (which has been transformed into oxide by aqua regia, evaporation, and calcination) to dissolve the alkali in water, to filter and super-saturate the liquid with sulphuretted hydrogen. The violet-red colour which the solution assumes when it contains sulphovanadite of soda indicates approximately the proportion of vanadium contained in the substances analysed. The sulphide of vanadium may also be precipitated, and the vanadic acid weighed by the methods already described. M. Wöhler's process succeeds equally well.

I have made some iron assays with these aluminous minerals, and have been obliged to operate at extremely high temperatures, in order to fuse the aluminous scoria when I have not increased the proportions of flux. This is how I proceeded:—

Knowing the quantity of sesquioxide of iron contained in the mineral, I first mix it with the quantity of charcoal necessary to reduce the oxide of iron, and then introduce 5 per cent. more charcoal. I then add for every 4 parts of the mineral 2 parts of pure white sand

and 1 part of powdered quick-lime. The intimate mixture is then introduced into a crucible of retort carbon (see *Ann. de Chem. et de Phys.* Third Series, xlvi. p. 149) of a decided and regular conical shape, enclosed in a good earthen crucible; between the two crucibles is placed a mixture of sand and very finely-powdered coal. Heat the whole in a wind furnace (fed if possible with retort carbon, broken into fragments, or with good coke) during four or five hours. The intensity of the heat may be diminished by adding to the above-named flux eight or ten parts of white glass.

When no glass has been added there is found a hard, compact, and crystalline slag, resembling in a remarkable manner the most crystalline porphyry; an ingot of cast iron, sometimes almost ductile, and very frequently crystals of great beauty adhering to the ingot. This is the nitrocarbide of titanium, which has taken its nitrogen from the reducing atmosphere of the furnace, the gas from which, in spite of all these coverings, penetrates freely to the interior of the carbon crucible. It may be assumed when any substance is heated in an apparatus of this kind, as well as in the best carbon crucibles, that the contents will, according to its nature, absorb as much nitrogen as is necessary to saturate it completely. I have obtained in these experiments the small, regular crystals of blast-furnace titanium (the nitro-carbide of Wöhler), of rather large dimensions, and of a very beautiful copper colour. Frequently, the brass-coloured nitride is observed in fibres interlaced in the ingot of iron.

On the Volumetric Estimation of Nitric Acid, by M. C. D. BRAUN.

THE author modifies M. Pelouze's process for the volumetric estimation of nitric acid in the following way:—Instead of estimating, by means of permanganate, the amount of protoxide of iron on which the nitric acid has not acted, he determines the peroxide formed by means of iodide of potassium and hyposulphite of soda. The perchloride of iron decomposes, as is well known, the iodide of potassium, and sets the iodine at liberty, even in the cold, but more completely when the reaction is assisted by a gentle heat. The free iodine may be easily estimated by means of a solution of hyposulphite of soda, after having added a small quantity of starch paste.

The normal solution of hyposulphite of soda is prepared either by dissolving a known weight of the salt in a similarly known weight of water, or by standardising with a solution of hyposulphite, of unknown strength, the iodine set at liberty by perchloride of iron, prepared from a known weight of metallic iron.—*Journal für Praktische Chemie*, lxxxi. 421.

TECHNICAL CHEMISTRY.

On the Application of Charcoal to the Ventilation of Sewers, by H. LETHEBY, *M.B., M.A., Ph.D., Medical Officer of Health, and* W. HAYWOOD, *M. Inst. C.E., F.R.I.B.A., Engineer and Surveyor.*

WE give the following abstract of a Report on the above subject just presented to the Commissioners of Sewers of the City of London:—

The experiment was suggested by the facts detailed in the Report of the Medical Officer on the Ventilation of Sewers, and on Sewer Gases, in 1858, wherein he described the powerful oxydising effect of charcoal as determined by the investigations of Lowitz, Saussure, Thenard, and others, at the beginning of the present century, as well as by the recent inquiries and practical results obtained by Dr. Stenhouse. All of these tend to prove that charcoal has the power of absorbing and oxydising the miasms of organic decomposition, when, with atmospheric air, they are passed over it. In commenting on these facts, it was remarked that in common wood charcoal there was evidently a powerful means of destroying the foul gases of sewers; and that the practical application of it was fortunately a question of but little embarrassment; for, to use the words of the Report, "let the sewers be ventilated as they may, either by open gratings in the streets, or by the rain-water pipes in the houses, or by the pillars of the gas-lamps, or by tubes carried up at the landlord's expense, from the drains of every house, or by especial shafts in the public streets,—in fact, let the gases go out of the sewers how they will, and where they will, you have but to place a small box containing a few pennyworths of charcoal in the course of the draft, and the purification of the air will be complete. As far as we know, the strength and the endurance of this power is almost unlimited, so that when once the air filter has been set up, it will last continuously for years. Its action also upon the draft cannot be particularly injurious; and I have no doubt that the temperature of the sewers, and the agencies which are now at work in circulating the air, and ventilating them, will be sufficient to keep up a current of foul air through the filters; and if these were multiplied to a large extent, the friction of the gases upon the charcoal would be reduced to an insignificant amount."

Acting on this recommendation, the Engineer was instructed to report on the practical capabilities of the suggestion; and he advised in his Report of November 22, 1858, that the proper mode of ascertaining the cost and effect would be to select "a limited district; adapt the existing ventilators in the first place; add to their number, and perfect the system to such an extent as may be practicable; and watch, and record the results before adopting it for the whole City."

The district experimented upon is in the eastern portion of the City of London. It includes a space bounded by Bishopsgate Street on the west, from Cornhill to Widegate Street; by Middlesex Street and Somerset Street, on the east, to the City boundary; and by the Minories and then by Leadenhall Street to Cornhill on the south; the whole of the main thoroughfares above-named being included in the area. It comprises a space of about fifty-nine acres, with about 1700 houses, and about 14,000 inhabitants.

This district was selected for various reasons: 1st, because the sewers have but a slight fall, and the currents in them are sluggish; 2nd, the area is densely populated, and has more than an average proportion of resident poor in it; 3rd, the thoroughfares are mostly narrow, and are, therefore, disagreeably affected by the sewer gases which issue from the ventilators; 4th, the district affords comparatively good means of isolation from other sewers.

The total length of sewers in the district is 25,587 feet. Upon this length there are 104 air-shafts, 265 gullies, 15 flushing-shafts, 4 tanks, and 26 side entrances.

The number and condition of the house drains entering the sewers are, upon the average, the same as those of other sewers within the City.

The whole of these sewers were isolated as far as

possible, so as to prevent air currents passing through them either from or to the adjacent sewers; and, as we have said, the general arrangement of the sewers of the district admitted of such isolation being effected with considerable completeness, so that the sewers were dependent upon their own conditions for ventilation.

There were two varieties of mechanical arrangements adopted for applying the charcoal; one was that patented by Messrs. Bean and Burge,[1] which consisted of one large sieve with compartments, the other was an adaptation of our own, and consisted of a series of trays for holding the charcoal, and were so constructed as to be capable of being readily removed from the frames into which they fitted.

Wood charcoal was employed, broken into pieces of the size of a filbert. It was packed closely, but without compression, upon the various trays; and each tray held about $1\frac{1}{12}$ lbs of charcoal, making altogether $6\frac{1}{2}$ lbs., distributed over the six trays of each air filter.

The experiment was commenced on July 14, 1860, and is still in operation. It has, therefore, been continued for a period of rather more than eighteen months.

The points to which we have directed our attention are the following:—

1st. The deodorising power of the charcoal.

2nd. The length of time that the same charge of charcoal will continue to deodorise the sewer gases.

3rd. The effect the air filters have on the ventilation and temperature of the sewers.

4th. The exact cost of the experiment, so as to obtain data from which to estimate the probable expense of the process if it were applied to the whole City, or even to the Metropolis.

First. The deodorising power of the charcoal has been satisfactorily proved to be complete. Not only have there been no complaints from the public of stenches from the ventilating gratings, but we have ascertained, by actual observation that the odour of the sewer gases is not perceptible when they have traversed the charcoal. This, indeed, might have been predicated from the extensive laboratory experiments, and the other practical inquiries to which we have alluded.

Charcoal from the ventilators has been submitted to chemical examination after having been in action for from nine to twenty months, and when heated with water it yields abundance of alkaline nitrate, showing that some of the organic miasmata have undergone complete oxydation. But besides these compounds, others are present,—namely, peculiar alkaline salts, which indicate the fixation not only of ammonia, but also of other volatile nitrogenous bodies which are peculiar to organic decomposition. The nature of these compounds has yet to be determined; for all that can be said of them is, that they have a remarkably bad odour, compounded of urine, sewage, bad meat, ammonia, and stale tobacco; attempts have been made to isolate them, but without success. This, however, is not surprising when we consider that chemists have hitherto failed to separate and identify the miasms of organic corruption.

Guntz, for example, tried to collect the volatile matters of putrid flesh, but he only obtained a stinking alkaline liquor that he could not analyse. Moscati condensed the organic vapour from the miasms of the pestilential rice fields of Tuscany, but the solution which he procured defied investigation, notwithstanding that it was so highly charged with organic matter as to putrefy quickly. Rigaud did the same with the atmosphere of the marshes of Languedoc, and Boussingault with the air of some of the worst districts of Paris. We have ourselves sought for the putrid miasms which infect the over-crowded dwellings of the poor; and although the liquid obtained in each case is so charged with alkaline organic compounds as to blacken sulphuric acid, and reduce the salts of gold and silver, yet the nature of the miasms is still unknown. Graham thinks they are not actual vapours, but organic molecules, like the pollen of flowers, floating in the air. This was also the opinion of the late Dr. Wilson, of Edinburgh; and both of these chemists have adduced very cogent reasons for believing that the infection or disease-producing molecule is not volatile in a strict chemical sense, but is merely diffused by suspension in the atmosphere. This may perhaps be true of the actual morbific agent, but there are also organic vapours of an alkaline nature present in these miasms, which are the invariable products of putrefaction. As far back as the year 1849, the researches of Dr. Stenhouse into the products of the decomposition of nitrogenous organic matters by heat, by acids, by alkalies, and by spontaneous decay, demonstrated that "whenever ammonia is generated in large quantities from complex substances (either animal or vegetable), it is always accompanied by the formation of a larger or smaller amount of volatile organic bases." Many of these bases he collected and obtained in the form of an oily liquid, but it was so difficult to separate them that he was unable to determine their exact composition. More recently, Dr. Crace Calvert has examined the volatile alkalies from putrefying flesh, and has found that they are very remarkable substances, containing carbon, nitrogen, hydrogen, phosphorus, and sulphur. Dr. Odling has also collected the alkaline emanations from sewage, and has ascertained that they are more complex than is usually supposed; for in addition to nitrogen and hydrogen, the constituents of ammonia, they contain carbon. Let them, however, be what they may, either physically suspended organic molecules, or complex volatile alkalies; and be the morbific agent either the one or the other, there is in charcoal a perfect means of arresting and oxydising all the noxious compounds contained in these gases. This is demonstrated not merely by their absence in the sewer air which has passed the charcoal, but also by the presence of the alkalies and the changed molecules in the charcoal itself.

Second. As to the duration of the powers of deodorisation, we have hardly sufficient proof.

The charcoal appears to lose much of its power when saturated with water; and as the position in which the trays containing it are placed is such that leakage of water into them in times of rain is, to some extent, all but impossible; and as, moreover, the atmosphere of the sewers is always very moist, the charcoal becomes so wet as to require removal long before it has failed as a deodoriser. Upon an average the sieves have been recharged about once in three months. Those which have been in very wet situations have been re-filled much more frequently, and those in dry situations less.

If the situation of the ventilators could be so arranged as to keep the charcoal dry, we are of opinion that it would not require renewal more frequently than once in a year, perhaps not so often; but, under existing circumstances, many of them require to be changed not less often than once a month. Secure situations might, however, be obtained by enclosing the charcoal in the lower compartments of the lamp-posts, and by other means which have been suggested in our previous reports, or by placing the ventilators against the sides

[1] See Chemical News, vol. i. p. 50.

of the houses, as in Philpot Lane. And here we may remark, that no apprehension need be felt of the escape of effluvium, for the experience of the last two years has proved, beyond all doubt, that this is impossible. We make prominent allusion to this fact because there was great opposition on the part of occupiers of houses when it was desired that the charcoal sieves should be placed against the outer walls of their premises; and we were, therefore, compelled, in arranging the experiment, to fix the filters in a much more expensive and inconvenient situation than we wished to do.

Third. The effect of the application of charcoal upon the general ventilation of the sewers is a point of the highest importance, and is, unfortunately, one upon which, after the most careful consideration of the facts elicited by the experiment, we are unable to pronounce a very positive opinion.

The necessity for sewer ventilation has already been the subject of separate reports by each of us, and, therefore, it would be superfluous to dwell now at any length upon the subject. It is merely needful here to hold in remembrance that, whilst diminishing the escape of effluvium into the streets, it is essential that the dilution of the gases generated in the sewers should not be so lessened by the diminished supply of air as to render the atmosphere of the sewers dangerous to the men who work in them; and it is also equally essential that the effluvium should not find its way more freely or in a more concentrated state into the interior of houses, by the house drains and their inlets, than it already does. These two considerations deserve attention, and may be best examined separately.

As regards the sewers, we must first state that observations made within them by means of delicate anemometers and other means, have not exhibited any perceptible currents of air through the ventilators fitted up with the charcoal sieves; and also that observations of temperature made contemporaneously in sewers with and without the charcoal boxes, have tended to show that those with the charcoal sieves were less influenced by the external atmosphere than those without them, for they were cooler in the summer months and warmer in the winter; from which it might be inferred that the currents of air were somewhat interfered with, and that the ventilation of the sewers was not so perfect as in other cases; but it must, at the same time, be stated, that the differences in respect of temperature were extremely slight.

On the other hand, there appears to have been no appreciable effect on the condition of the air within the sewers, as far as the experience and observations of the workmen have gone; for no complaints of a worse atmosphere have been made by them.

Chemical and physical considerations indeed would lead us to the conclusion that, although the coarsely-granulated charcoal may offer some slight resistance to the passage of an actual current of air, it cannot affect the diffusive power of gases, which is so largely concerned in maintaining the chemical equilibrium of the atmosphere; in fact, experiment has shown that media much denser than charcoal, will permit of the free interchange of air, and of the quick dilution of noxious effluvia. Again, the chemical condition of the internal atmosphere of the sewers, as far as analyses have gone, show a proportion of 79·96 per cent. of nitrogen, 19·51 per cent. of oxygen, and 0·53 per cent. of carbonic acid, with traces of ammonia, marsh gas, and sulphuretted hydrogen. There is, therefore, abundance of oxygen to maintain the functions of life.

We believe, therefore we are justified in the conclusion, that the danger to the workmen in the sewers has not been materially increased by the application of charcoal to the ventilators. How far they would obstruct the passage of a large quantity of coal gas escaping into the sewers is yet a matter for observation. This gas is at all times dangerous, and is most frequently the cause of sewer accidents by explosion; fortunately, however, the presence of it is betrayed by its odour long before it reaches a dangerous proportion, and, with due precaution, the risk of its exploding may be avoided.

And here, while discussing the accidents which are incidental to the presence of foul gases in the sewers, it may not be out of place to speak a little more fully concerning the safety of those who are exposed to them.

The deadly and morbific effect of concentrated sewers' gases is, unfortunately, a matter of common experience. Again and again it has happened that these gases have killed with the energy of the most deadly poisons. In France, the fatal effects of sewer and cesspool miasms have been frequently observed; and the writings of Portal, Gériel, Labouire, Parmentier, Alibert, Dupuytren, Cade de Vaux, Hallé, Geraud, Parent-Duchâtelet, Chevalier, D'Arcet, Orfila, Devergie, and others abound with instances of the fatal *mephitismés*, caused by the gases from the *fosses d'aisances*. In this country such accidents are of much rarer occurrence; for over a period of sixty years there have been but five such accidents in the sewers of this metropolis. These, however, were the cause of eighteen deaths. One of these accidents occurred about a year ago in the sewer of Fleet Lane, and it was the first upon record within the sewers of this city. We have but little knowledge of the immediate cause of that accident, for the four men who were killed were the only persons in the sewer, and they seem to have died instantly; but the inquiries which were instituted at the time suggested the necessity for some protection against a similar disaster.

The experience of our present investigations, as well as the results of laboratory experiments, point to the advantage which may be derived from the use of an apparatus in the form of a respirator, containing charcoal. Dr. Stenhouse has already contrived such an apparatus; and we believe that it might be resorted to by the men in times of danger, as when there is an odour of foul gas, and would afford them an opportunity of quitting the dangerous locality and reaching a place of safety. It is very probable that under the worst circumstances, when the air is charged with a large proportion of sulphuretted hydrogen, there is still enough oxygen to maintain the functions of respiration: the danger, indeed, is not from the absence of atmospheric oxygen, but from the presence of sulphuretted hydrogen, and this the charcoal of the respirator would destroy. It is desirable, therefore, that each of the sewer men be provided with a respirator of this construction, and be advised to use it when the air of the sewer is expected to be unusually offensive, and when they are disturbing large deposits of mud. There will be, doubtless, difficulty in compelling men to use these precautions; but if they are provided for their use, the Commission will have the satisfaction, should an accident occur again, of knowing that they have done the utmost in their power to prevent it.

The second part of this branch of the inquiry is as to the effect the introduction of the system has had upon the sanitary condition of the houses, as regards the escape of sewer gases into them from the drains. Upon this head our information is of a negative character. All

that can be said is, that we have had no evidence of increase of effluvium in the interior of dwellings; nor do the mortality or sickness returns indicate an unusual amount of disease of a pythogenic character. Unfortunately, as has been pointed out by your Engineer, in his Report on the Ventilation of Sewers, there are evidences that from most of the house drains, even in the houses of the better classes, where the inlets are supposed to be properly protected, effluvium periodically escapes into the dwellings and is the cause of mischief; but that such escape has been in excess in the district alluded to, we can discover no proof.

As to the effects of the diluted gases when they find their way into dwellings, little need now be said; for the Medical Officer has already discussed the subject in his Report of 1858, on Sewage and Sewer Gases. Abundant evidence is there produced of the disease-producing agency of the diluted effluvium; and the sad experience of the last few months is sufficient to show how those gases may exert their action in places where they are hardly suspected to exist. It can no longer be doubted, after the experiments of Dr. Barker, and the clinical observations of Dr. Murchison, that these gases are concerned in producing a form of continued fever, which is very aptly termed from the cause, *pythogenic*. This fever is often seen in the filthy haunts of poverty, where corruption is abundant; and its ravages are in proportion to the defective state of the drains and sewers. Twelve years ago, when Mr. Simon reported to you on the matter, he remarked that the constant prevalence of fever in certain courts and alleys of this city told, without reference to the Surveyor's Plan of the Sewers, that they were then destitute of the means of perfect drainage; and the occurrence of typhoid or gastric fever, even in the houses of the rich, is a matter for grave suspicion as to the state of the cesspools or the house drains and their appliances.

These considerations force upon our minds the conclusion, that wherever there is an escape of gases from the private drains or the public sewers, it is of the utmost importance that those gases should be destroyed as far as practicable; and we are of opinion that by the application of these charcoal filters a remedy may in numerous cases be found.

Fourth. With respect to the cost of the process:—The expenditure incurred in fitting up the 104 ventilating shafts was 918*l.* 18s. 5d., which is at the rate of about 8*l.* 16s. 8d. per ventilator. An experiment is necessarily more costly than an established system, and a much less sum than this may be taken as a fair average of the probable expense of extending the process even to the whole of the City, where, as may be gathered from what we have before said, the difficulties in the adaptation of the existing ventilators are considerable, and the cost, therefore, large. The charges named will not, therefore, serve for estimating the cost of adapting the shafts in districts which are less dense, and in streets which are less encumbered. Still less will it furnish an indication of the cost of the system when applied to new sewers, which may hereafter be constructed with the especial view to adopting it. In the latter case, the cost, under well-considered arrangements, would be very moderate.

The total expense of reparation and renewal of damaged trays, frames, and coverings, averaged 16s. 6d. per ventilator per annum. The cost of supervision, supplying, and changing the charcoal, was 8s. 9d. per ventilator, making together 1*l.* 5s. 3d. per ventilator per annum. The very large cost for reparation was mainly attributable to the repeated breakages of one class of apparatus used, which is not found to be capable of standing the severe traffic of the City thoroughfares. This item might, therefore, at a future day, be considerably lessened; but if the process should be still applied in the manner herein detailed, much annual expense will always have to be incurred. We are confident, however, that, with the experience already obtained, a more economical and effective mode of applying the process at the sides of the public way, or otherwise as suggested, might be adopted; and in that case the expense of reparation might probably be reduced by two-thirds, and that of maintenance would be very considerably reduced likewise.

Our general conclusions from these experiments, and from the consideration of collateral evidence, are, that dry charcoal, in the presence of atmospheric air, is a powerful means of destroying the mephitic gases and vapours of sewers and house drains. That the charcoal filters may be used with efficacy in the course of the air channels from the drains and closets of houses, as well as in the ventilation of the public sewers; that in applying the charcoal, those contrivances should be used which offer the least resistance to the free passage of the air; that the situation of the filters is best when the charcoal is protected from wet and from dirt, and is easily accessible; that from the ascertained efficacy of charcoal in destroying the dangerous emanations from sewers, the system may be generally applied with great advantage; and that from the experience derived from this extensive practical inquiry, we are satisfied that the expense of the system might be considerably reduced below that indicated by the cost of the experiment.

PHYSICAL SCIENCE.

On the Molecular Dissymmetry of Natural Organic Products, by L. Pasteur, *Member of the Chemical Society of Paris.*

[INTRODUCTION.]

These Lectures were prepared on the invitation of the Council of the Chemical Society of Paris. The researches of which they present a rapid summary have occupied me during ten consecutive years. I had often thought of collecting them, of minutely reviewing the details, and adding to them all the developments necessary to the co-ordination of the different memoirs in which they appeared for the first time. But there is, in the life of every man devoted to the experimental sciences, an age when the value of time is inestimable; this rapid age in which the spirit of invention flourishes, in which every year ought to be marked by an advance. To stop, then, voluntarily at things acquired, is a restraint and a danger, which the pleasure and even utility of seeing our ideas expand in accordance with our desires too well compensate.

As much as I have recoiled before the laborious task of collecting, of perfecting, my researches on the molecular dissymmetry of natural organic products, in the same degree I have yielded eagerly to the prayer of many Members of the Chemical Society to publish the two Lectures in which I was charged to set forth the principal results at which I had arrived. Few studies have been better received at the time of their successive appearance, and, nevertheless, I have many proofs that they were scarcely known.

I hope, then, that this publication may be of some

use. But should there be nothing beyond the interest which these Lectures have excited in the distinguished Assembly before which they were delivered, I shall be sufficiently recompensed for my labours.

The Chemical Society has been gratified by the eagerness with which many young men of learning have taken seats among the audience. It has especially marked the sympathetic encouragement given to its labours by the presence of Messrs. Balard, Claude Bernard, Delafosse, Frémy, Serret, Members of the Academy of Sciences.

May I be permitted, moreover, to especially thank, in my name and in the name of the Society, our illustrious President, M. Dumas, whose lofty and benevolent patronage has always served science almost as much as his immortal works.

I. At the close of the year 1808 Malus announced that light reflected from all bodies, whether opaque or diaphanous, acquired very extraordinary new properties, which essentially distinguished it from light transmitted directly from luminous bodies.

The modification which light undergoes in the act of reflection Malus denominated *polarisation*. Later, this was designated under the name of *plane of polarisation* of the ray—the plane reflection itself,—that is to say, the plane passing by the incident ray and the normal to the reflecting surface.

Malus did not limit his discoveries on polarised light to this. It had long been known that a ray of direct light always separated into two white fasciculi, of the same intensity, in its passage through a rhomboid of carbonate of lime. The flame of a candle, observed by the aid of such rhomboid, is always double, and the two images have the same brightness.

Huygens and Newton had observed that light which has passed through a crystal of Iceland spar no longer comports itself like direct light. So, looking through a new rhomboid at one or the other of the two images of the candle we have just mentioned,—1. There will not always be bifurcation of the ray; 2. When there is bifurcation, the two new images will not have the same intensity. Light which has traversed a bi-refrangent crystal is different, then, from natural or direct light. This admitted, Malus proved that the modification impressed upon light by double refraction was identical with that which reflection produces from the surface of opaque or diaphanous bodies; in other words, that the two rays, ordinary and extraordinary, given by a bi-refrangent crystal are polarised rays.

Malus established so clearly from the beginning these fruitful discoveries, with so much care and so much precision in his facts and in his language, that one might believe, in reading his memoirs, that they were prepared yesterday. But he could not pursue his work: premature death carried him off in 1812, at the age of thirty-seven years. Happily for science, two celebrated physicists,—at that time young and full of activity,—MM. Biot and Arago, received his legacy, and were not slow to distinguish themselves by brilliant discoveries in the new route which Malus had just opened to science.

In 1811, Arago observed that when a polarized ray traversed normally a plate of rock crystal cut perpendicularly to its axis, the ray, on issuing from the plate, if analysed by the aid of a rhomboid Iceland spar, gives constantly two images in every position of the rhomboid; and further, these two images are coloured with the complementary tints. When the thickness of the spar does not permit an entire separation of the two fasciculi, the image is white where they are in part superposed.

This experiment presents a double anomaly to the ordinary laws of bi-refrangent crystals. Every other crystal, with an axis cut normally to this axis, would have furnished two white images in the place of being coloured, and, in two rectangular positions of the rhomboid analyser, the images would be reduced to a single one.

The conclusion of Arago was, that the results of the preceding experiment are precisely those which should follow, if we suppose that the differently coloured rays of the incident white fasciculus are, on issuing from the plate of the quartz, polarized in different planes.

Arago does not recur to these brilliant phenomena, the laws of which M. Biot presented since 1813, carefully isolating them from those among which Arago appeared to confound them.

M. Biot formed the polarized ray successively with each of the simple rays of the spectrum, and found that the plane of primitive polarization was deviated at an angle proportional to the thickness of the plate; that this angle is different for each simple colour, and goes on increasing with the refrangibility according to a determined law. M. Biot also made the very curious observation, that of the plates obtained from different species of quartz, there were some which deviated the planes of polarization to the right, and some to the left following the same laws.

But the most remarkable discovery of M. Biot in this kind of phenomena is, without contradiction, that of the deviation imparted to planes of polarization by a great number of natural organic products, essence of turpentine, solutions of sugar, of camphor, of tartaric acid, etc. The first announcement of this fact is found in the bulletin of the Philomathic Society for December, 1815.

For the understanding of this Lecture, we should especially notice the existence of the rotary property in tartaric acid, and its absence in paratartaric or racemic acid, an acid which is isomeric with tartaric acid.

There exist, then, liquid organic products or solutions in water which possess the rotary property, and in this respect remind us of the solid crystallized quartz. But it is essential to note here, that this analogy to quartz is entirely apparent. There was in both cases deviation of the plane of polarization, but the characters of the phenomenon were very different.

Quartz deviates, but it must be crystallized. Dissolved or solid, and not crystallized, it has no action. Not only must it be crystallized, but it must be cut in plates perpendicular to the axis. From the moment the plate is a little inclined in the direction of the ray, the action abates and then ceases.

Sugar deviates (and what I say of sugar is true of all other organic products), but the sugar must be either dissolved, or solid and amorphous like barley-sugar. In the crystalline state it is impossible to discover any action.

The tube containing the solution of sugar may be inclined. The deviation does not change for the same thickness. Further, by violently agitating the liquid by the assistance of a clockwork movement the phenomenon remains the same.

Thus M. Biot concluded from the beginning, strictly that the action exerted by organic bodies with a molecular action peculiar to their ultimate particles dependent upon their individual constitution. In quartz the phenomenon results from the mode of aggregation of the crystalline particles.

These are the physical precedents, if I may so express myself, of the researches with which I propose to entertain you. The mineralogical precedents will follow.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

PHARMACEUTICAL SOCIETY.

Wednesday, February 5.

Mr. P. SQUIRE, *President, in the Chair.*

A specimen of *Citrate of Lithia* was exhibited by Messrs. Blake, Sandford, and Co. They had prepared the iodide, tartrate, and citrate of lithia. The first was a very deliquescent salt; the second was dry and stable; the third was an efflorescent salt, soluble in water, and almost tasteless. As citric acid had been recommended for the same diseases lithia was prescribed for, they thought the combination of the two would be useful.

A packet of James's Powder which had recently been found in a medicine chest that formerly belonged to George IV. when Prince of Wales, was exhibited by Messrs. Bourdar and Co. It bore the original signature of "J. Newbery," which, the President said, proved that it must be at least seventy years old.

The PRESIDENT then said, it had been reported to the Pharmacopœia Council that tinctures were in use made with methylated spirit. He had tried and failed to obtain specimens; but he was informed that a methylated tincture of opium was in use as a veterinary medicine. He hoped the subject would soon be brought before the Society, and that measures would be taken to prevent the use of such tinctures, which he considered were highly objectionable.

A Paper, "*On the Nutritive Value of Dika Bread*," by Mr. J. ATTFIELD, Demonstrator of Chemistry at St. Bartholomew's Hospital, was then read. From several points of resemblance to coffee and cocoa, Mr. Attfield had hoped to find caffeine, or a homologue, in this African food. No such principle was present, but the result of the examination in other respects was thought of sufficient interest to merit recording. In addition to 5·0 per cent. of water and 4·4 of ash, 65·5 of a solid fat had been separated. A specimen of the latter was exhibited to the meeting. It is moderately hard, has a slight odour, and fused at about 90° F. That it was actually derived from the nut, and not added in the subsequent making up of the food, was proved by examining some of the kernels picked out from the food, which yielded 68·4 per cent. of the fat. This large quantity of fat, coupled with the statements of African travellers, that enormous quantities of the Dika fruit annually fall and rot at the foot of the mangifera trees in the Sierra Leone district, naturally suggests economic applications. By the aid of heat nearly all the fat can be pressed out of the fruit, and possibly its collection and exportation found mutually advantageous to the African and European. Not containing any caffeine, the action of Dika bread upon the human system must be essentially different from cocoa, coffee, tea, &c. Indeed, looking at the large amount of fat it contains, Mr. Attfield regarded it as one of those foods whose chief office is directly to contribute to the development of animal heat, and not to form tissue. It more nearly resembles a sort of chocolate made in Spain, by roasting, grinding, and caking the root of the *Cyperus esculenta*, or a similar food used in North Carolina, and prepared from the earth-nut, or *Arachis hypogea*. The amount of nitrogen in Dika bread is below that in coffee or in cocoa. Coffee, according to Stenhouse, contains 2·5 to 3·0 per cent. A specimen of good cocoa nibs yielded Mr. Attfield 2·37 per cent.; while in Dika bread he found only 1·51 per cent. or in the picked out and cleaned kernels 1·53 per cent. The mean per centage (1·52) corresponds to 9·5 per cent. of vegetable albumen or casein; and it is to such matters must be ascribed whatever amount of flesh-forming power Dika bread may possess. Mr. Attfield gave the following as the proximate analysis of the bread:—

Nitrogenous matter	9·5
Fat	65.5
Water	5·0
Ash (phosphate, silica, potash and iron)	4·4
Other non-azotized constituents	15·6
	100·0

The fat is said by Oudemans, of Utrecht, to consist of laurinic and chiefly myristinic acids. In reply to the Chairman Mr. Attfield said it was not known how the bread was obtained. The shape of the loaves was due to the baskets in which they were packed.

Mr. ATTFIELD then read another Paper, the first of an intended series, "*On the Mineral Constituents of Plants.*" After pointing out the unsatisfactory nature of our knowledge of the mineral constituents of plants as derived from analyses of the ashes of vegetables, alluding especially to nitrate of potash as being (though frequently present) generally overlooked in such an examination, the author described two new sources of information on the subject. First, the microscopical and chemical examination of the crystalline growths which frequently occur on vegetable extracts used in medicine; and secondly, the production of the salts in actual crystals, from an infusion or decoction after all the uncrystallizable matter has been separated by Professor Graham's dialytic process. Twelve specimens of saline efflorescences or extracts were exhibited *in situ*, and an equal number shown under the microscope. Mr. Attfield gave detailed information of the specimens. One specimen of extract of belladonna had crystals of chloride of potassium $\frac{1}{20}$th of an inch square. Another specimen of the same extract had chloride of potassium and nitrate of potash. A sample of extract of hemlock had chloride of potassium. Compound extract of sarsaparilla also gave cubes, but more commonly, slender needles of the same salt. Extract of henbane showed crystals of nitrate of potash varying from $\frac{1}{100}$th to $\frac{1}{30}$th of an inch in short diameter, and from $\frac{1}{20}$th to $\frac{1}{8}$th in the long. Extract of colchicum and also of stramonium were studded with prisms and cubes of chloride of potassium. One specimen of extract of stramonium was remarkable as having sulphate of soda. An estimation of the soda in the extract indicated 15·8 per cent. of the salt, but an examination of fresh seeds showed that most of the acid was combined with lime and potash. Extract of aconite had crystals of chloride of potassium. Extract of lettuce had nitrate of potash, which had already been observed in the juice. Mr. Attfield then noticed incidentally the efflorescences on the pill masses containing soap. When Castile soap was used these consisted of sulphate of soda. Common soda soap gives rise to an efflorescence of carbonate of soda, and soft soap to chloride of potassium. In the foregoing cases, seven of the efflorescences or extracts consisted of chloride of potassium,—a salt which might be expected to be present in most vegetable juices. Four specimens were nitrate of potash,—a salt which has been considered as only occasionally present, but which Mr. Attfield believes to occur frequently. Sulphate of soda was found unexpectedly in one instance. In conclusion, Mr. Attfield invited all pharmaceutists who may notice saline efflorescences on extracts to forward samples to him, which he will be happy to examine, and communicate the results to the Society at a future meeting. He wished to be forwarded with the samples a statement of the amount of raw material used in making the extract, the age of the sample, and also whether common or distilled water was used in its preparation. In separating the bulk of saline matters in extracts, Mr. Attfield skill-

fully availed himself of Prof. Graham's diffusion apparatus, and so obtained very unexpected results. A portion of concentrated henbane juice in this way yielded 25 per cent. of salts, while an equal portion evaporated and incinerated yielded only 16·2 per cent. Eleven parts of the former consisted of nitrate of potash, but in the latter not a trace of this salt could be found, and 7·9 parts of carbonate of potash was found. Mr. Attfield expressed an opinion that most of the commercial potashes existed in the plant as nitrate, the truth of which opinion he hopes to demonstrate in a future paper. The amount of chlorides in the diffusate and the ash similarly differed. The former gave 5 per cent., the latter only 1·12. Sulphates differed in the converse order, but this he considered owing to insufficient dialysation.

Professor Redwood said that Mr. Attfield had wisely abstained from extensive generalization on the subject. The results were interesting, but should not be built on too much. It was much to doubted whether this new process, showed more accurately than the old, how the salts were combined in the plant, as, in the case of water, the salt got out is not necessarily the salt contained. The salts might be changed in the process of evaporation. There might be greater changes in incinerating, and how the components are arranged we have no means of determining. He thought that carbonates might be derived from nitrates in incinerating, and it was equally probable that the nitrate was the result of oxidation. He did not feel certain that nitric acid existed in the fresh juice. Nitrates in water were the result of decomposing organic matter oxidised, and extracts contained exactly the nitrogenous matter to oxidise. The nature of the crystalline efflorescences, he thought, depended on age. According to old investigators the great bulk of saline matter in extracts consisted of chlorides. The ashes of plants contained both sulphate and chloride of potassium. Why did the chloride come out and not the sulphate? Mr. Attfield, the Professor thought, was only on the threshold of the inquiry as to what the saline matters consisted of.

Mr. Attfield said that the nitrates in water were formed under very different circumstances to those which obtained in extracts; in the case of water there was a minimum of organic matter and a maximum of oxygen; in an extract there was a maximum of organic matter and a minimum of oxygen. The crystals of nitrates were equally diffused throughout the extract out of contact with the oxygen of the air. Sulphate of potash was not found in an extract, but was found in an ash. He thought that sulphate of lime was changed into sulphate of potash in the combustion. He had obtained nitrate of potash from henbane leaves, which had not been gathered many days.

Mr. Deane recommended that Mr. Attfield should examine extracts of various ages, and also the chlorophyll separated in making green extracts. He had obtained a large crop of crystals on evaporating an alcoholic extract of chlorophyll.

Mr. Wood enquired whether Mr. Attfield had ever found salts of ammonia. Bousingault had found them.

Mr. Whipple said he had invariably found that the saline efflorescences on extracts prepared exactly according to the Pharmacopœia consisted of carbonate of ammonia.

Mr. Attfield replied that he had never found a salt of ammonia. He then proceeded to read a third paper entitled "*Poisons not always Poisons*," a notice of which we defer.

New Use for Ozone.—Gorup Besanez says (*Ann. der Chem. und Pharm.*, Bd. cxviii. s. 232) that ozone may be employed with safety to remove the yellowness from old engravings and prints. It should not be allowed to act long on copper-plate engravings, or the delicate lines may suffer.

NOTICES OF BOOKS.

Metallurgy. By John Percy, M.D., F.R.S. London: John Murray, Albemarle Street.

The volume which made its appearance under this title at Christmas last is the first part of a most comprehensive treatise on the art of extracting metals from their ores, and adapting them to various purposes of manufacture.

The chief characteristics of the work before us may be shortly described as consisting of a profusely illustrated volume of some six hundred pages octavo, abounding with details of metallurgical processes and analytical examinations, both of raw materials and manufactured products, ores, metals, alloys, slags, &c. As an introduction to the special consideration of the metals themselves, a full account is given, in the opening chapters, of the fuels employed in smelting, and of the materials ordinarily used in the construction of crucibles and furnaces. The author has treated, in the first place, but of two metals, copper and zinc, with their alloys, and proposes to complete in one more volume the description of the remaining metals, the materials for which are stated to be nearly all collected; and will, it is hoped, be ready for publication before the end of the present year.

Proceeding to a more detailed examination of the contents of this work, and omitting further notice of the introductory chapter, excepting the bare mention that this part relates to general considerations on the metallurgical processes, Dr. Percy commences with a very full account of the various descriptions of coal, coke, and other fuels employed in smelting, which are treated of under their two-fold capacity of reducing agents and sources of heat in the furnace. A vast number of analyses are given of coals from all parts of the world, exhibiting very wide ranges in regard to the proportions of the several elementary components, and indicating the primary cause of the extreme difficulty sometimes encountered in attempting an accurate or specific classification of any given sample under one or other of the general subdivisions into which the ordinary kinds of coal are separable. The case of the Torbane Hill mineral, which was decided in the Edinburgh Law Courts some short time since, may be quoted as furnishing a remarkable illustration of the difficulty sometimes experienced in attempting an accurate definition regarding the nature of coal. Evidence in support of this mineral bearing the character of bituminous coal was opposed by the assertion that it did not form coke on being heated in a close vessel; when it transpired that by very careful manipulation coke might actually be produced from the combustible mineral in question. In like manner several discrepancies are noticeable in the descriptions accompanying samples of coal sent to Dr. Percy for chemical analysis, which must be accounted for by the relative sense in which the characters of "caking" and "free-burning," as applied to coal, are to be interpreted. On this point the author remarks, that "the coals of South Staffordshire are practically non-caking coals; that is, the powder of these coals, when heated in the usual way, does not yield a coherent coke, but only a very slightly fritted and crumbling mass. Yet, if it be *rapidly* exposed to a *high* temperature, such as a bright red heat, in a *close* vessel, a good solid coke may be obtained."

An inspection of the columns indicating the per centage amount of carbon will alone afford abundant evidence of the great diversity which must exist in the several kinds of coal submitted to comparative examination in these analytical tables; and likewise the proportion of mineral matter left in the form of residual ash appears to range between the wide limits of one and eleven per cent. in the case of coal from neighbouring localities in Hungary, and in a particular description of French coal to amount to nearly twenty per cent.

Next in order to coal appear the comprehensive details relative to the preparation of carbon fuel in the shape of wood-charcoal and coke. It has been thought by some critical writers who have publicly expressed their opinions upon this treatise, that the preliminary subjects, especially this portion, have monopolized too much space in the present volume, to the exclusion of information more immediately referring to the metals themselves. This view of the subject restricts, however, the broad definition which the word "metallurgy" is generally intended to convey, and would, in the present instance, have curtailed a most interesting and important branch of the general subject, and deprived the student of much valuable aid towards the understanding of the succeeding chapter upon the construction of furnaces. In preference to losing the advantage of this portion of the work we should much rather hail the advent of a *third* volume, in the event of the two now contemplated being ultimately found insufficient to permit of the full treatment demanded in a standard volume, although the work might in a measure lose its claim and character as a convenient handbook, and become, like Gmelin's "Chemistry," an indispensible standard work of reference. Phillips's "Metallurgy" would then serve a useful purpose in supplying the facts more generalized and condensed, and would be in the form of an introductory treatise to the larger and more comprehensive encyclopædia of Dr. Percy.

(To be continued.)

NOTICES OF PATENTS.

Grants of Provisional Protection for Six Months.

2677. Thomas Richardson, Newcastle-upon-Tyne, and Robert Irvine, Hurlet, Renfrewshire, "Improvements in treating bones and gelatine."—Petition recorded October 25, 1861.

2855. William Henry Balmain and John Kean, St. Helen's, Lancashire, "Improvements in the manufacture of flowers of sulphur and roll and other forms of sulphur."—Petition recorded November 13, 1861.

2974. David Ker, West of England Soapworks, Plymouth, Devonshire, "An improvement in the manufacture of soap."—Petition recorded November 26, 1861.

25. Gilbert Stracy, Rackheath Hall, near Norwich, Norfolk, "Improvements in the manufacture of artificial uel."

76. Henry Darvill, New Windsor, Berkshire, "An improved method of hardening chalk for building purposes."

84. Lauchlan Mackvidy, Greenock, Renfrewshire, "Improvements in apparatuses for reburning animal charcoal."

Notices to Proceed.

2383. Charles Watt, Graham Street, and John Watt, Lorrimer Street, Walworth, Surrey, and Thomas Snaith Haviside, Cornhill, London, "An improved mode or method of bleaching palm oil."—Petition recorded September 24, 1861.

2396. Thomas Richardson, Newcastle-upon-Tyne, "Improvements in the manufacture of muriate of iron for the purification of coal gas."—Petitions recorded September 25, 1861.

2761. George Evans, Gloucester Terrace, Portman Square, London, "Improvements in treating peat to render it useful as fuel, and for illuminating and metallurgical purposes."—Petition recorded November 2, 1861.

2974. David Ker, West of England Soapworks, Plymouth, Devonshire, "An improvement in the manufacture of soap."—Petition recorded November 26, 1861.

3112. Marc Antoine François Mennons, Furnival's Inn, London, "An improved means of defecating and purifying cane and other saccharine juices"—A communication from Louis Marie Amand Achille de Courson de la Villeneuve, Caen (Calvados), France.—Petition recorded December 12, 1861.

Chemical Notices from Foreign Sources.

I. ANALYTICAL CHEMISTRY.

Detection of Rape in other Oils.—F. Schneider gives (*Polytechn. Centralblatt*, 1861, s. 1229) the following process for the detection of rape oil when it is used to adulterate other fatty oils. He dissolves one volume of the suspected oil in two volumes of ether, and adds to the mixture from twenty to thirty drops of a saturated solution of nitrate of silver in spirit of wine. The whole is now well shaken together and set aside, out of the light, to rest. If rape oil be present, the lowest layer soon becomes brownish and at last a deep black. If only a very small amount be present, it will turn a brownish black in the course of twelve hours. The reaction depends upon the sulphur in the rape oil; and founded on the presence of this element. Mailho gives the following process for detecting the falsifications by any oils from the *Cruciferæ:* Dissolve 2 grammes of caustic potash in 20 grammes of water, add to this 25 or 30 grammes of the suspected oil and boil for some minutes. Pour the soap on a moistened filter, and test the filtrate with acetate of lead, or nitroprusside of sodium. The latter test is best applied on a watch glass placed on white paper.

Detection of Rosin Oil in Fatty Oils.—Jungst publishes (*Dingler's Polytech. Journal*, Bd. clxi. s. 307) the method founded on the solubility of rosin oil in spirit of wine, sp. gr. 0·83. He mixes one volume of suspected oil and ten volumes of spirit, shakes them well together for a quarter of an hour, and then allows them to settle. The alcoholic layer is then poured off and evaporated, and the rosin oil, if any is present, is recognized by its colour and smell. But as a hundred volumes of spirit only take up five volumes of rosin oil, a proportionately large amount must be used in making a quantitative estimation. The Author directs the following method:—In a burette graduated in cubic centimètres place 20 c.c. of the oil to be tested, and fill up with spirit to 200. Shake together for 15 minutes; allow it to rest 24 hours, multiply the loss of volume in the oil by 5, and you have the percentage of oil dissolved by the spirit—which, however, is not necessarily rosin oil. Our readers will see in what cases this process is not applicable.

New Method of Detecting Small Quantities of Nitric Acid.—Professor Schulze communicates the following method to the *Chemisches Centralblatt*, No. 42, 1861. It is founded on the fact that nitric acid in a strongly alkaline solution, is, by the action of finely-divided zinc, or sodium, or aluminium amalgam, transformed into ammonia. When zinc is used, only about half the nitrogen is evolved as ammonia, but with sodium amalgam the change is complete. When manures, &c., are examined, the original ammonia and that formed by the action of the alkali, is first got rid of by long boiling, and then the zinc or amalgam is dropped into the residual liquid, and the mixture again heated. According to the author and Dr. Knop, this process will detect very minute quantities of nitric acid.

II. TECHNICAL CHEMISTRY.

New Method of Separating Iodine.—Dr. Luchs points out (*Wittstein's Vierteljahressch*, Bd. x. s. 537) that bichromate of potash may be advantageously employed in obtaining iodine. He dissolves 6½ pounds of the saline residue in 12½ pounds of water, and pours in 6 pounds of oil of vitriol. To this mixture he adds 1¾ pounds of the bichromate of potash in fine powder. After well stirring the mixture, the whole of the iodine falls to the bottom of the vessel in rough crystals. Some iodine occasionally remains dissolved in the green supernatant liquor, and

this may be separated by repeated distillations. The author suggests the separation of the chrome oxide and sulphate of potash from the liquor to reduce the expense of working the process.

Phosphates as Manures.—M. Thénard, continuing the experiments we noticed at page 341, vol. iii., has come to the conclusion that the best way of using fossil phosphates as manure is with an excess of ammonia (*Comptes-Rendus*, t. liii., p. 1019). The experiments (which he does not yet detail) are calculated to throw new light, he says, on the most rational method of employing phosphates in agriculture. Another valuable paper on the action of phosphates is published by M. Ville (*Comptes-Rendus*, t. liii., p. 833). His experiments show that phosphites and hypophosphites have no influence on vegetation, and that phosphoric is the only acid of phosphorus which is assimilated by plants. In the same paper, M. Ville shows that nitrates are far more valuable as manures than nitrites.

MISCELLANEOUS.

New Thermometer for General Purposes.—We have had submitted to us for inspection by Messrs. Spencer, Browning and Co. an improved thermometer, the peculiarity of which is the Indelible Porcelain Scale with which it is furnished. As a material to form the scale of a thermometer, porcelain possesses immense advantages over metal, wood, ivory, or any other substance usually employed for the purpose. Metal scales quickly corrode, and become discoloured, so that the divisions in a very short space of time are almost illegible. Wood, ivory, &c., are easily stained and frequently warp, breaking the tubes which are attached to them. Porcelain is entirely free from these objections, while when constructed on a proper principle, on the score of accuracy it cannot be equalled. In these instruments the scale is graduated and divided to the tube, as in an ordinary good thermometer, whilst by the old plan of printing on enamelled copper the tubes were generally fitted to the scales after the divisions were printed, and were thus incorrect. Besides this they were also liable to the great disadvantage, that any extreme change of temperature caused the enamel to crack, and separate itself in flakes from the copper, thus rendering the instrument useless. Finally, the degree of expansibility of glass and porcelain being almost exactly the same, the thermometer is necessarily much more accurate than is the case when the scale is made of metal. In durability the Porcelain Scale Thermometer is superior to any other, and will therefore be preferred by the general public for ordinary employment, but more particularly, when required for baths or general out-door purposes, while to brewers, chemists, photographers, dyers, engineers, and proprietors of bathing establishments it presents advantages that will render it indispensable. For general maritime use it is unequalled.

Hill's Patent for the Purification of Coal Gas.—The claim of Mr. Hills for the employment of the hydrated oxides of iron, and especially the important feature of this invention regarding the means of renovating the purifying materials by simple exposure to air, continues to be the subject of legal proceedings. Apart from the case in hand, we have on several occasions called attention to the identity of the terms of Mr. Hills' specification with those of others taken out long subsequently to the sealing of the original patent; and have remarked no less than three or four instances in which patentees have been vainly striving to establish their individual claims to the same invention, but with dates obviously disadvantageous to their cause. We append the judgment pronounced by Sir W. P. Wood in the Vice-Chancellor's Court, on February 14, as embodying the most recent conclusions with regard to the interpretation of this patent claim. Judgment was given in this case, which came before the Court upon a motion for an injunction to restrain the defendants from manufacturing and purifying gas according to the invention of the plaintiff, comprised in his patent of November, 1849, as modified by the subsequent disclaimer and memorandum of alterations; and also from using renovated and reoxidised purifying materials, according to or in imitation of the same invention for purifying gas, and from infringing the plaintiff's patent. The patents of the plaintiff have been the subject of much litigation, and the validity of his invention has been recently established by the Lord Chancellor in the case of "Hills *v.* Evans," which was argued at great length in December last. Mr. Rolt, Mr. Grove, and Mr. Marten appeared for the plaintiff upon the present motion; Sir Hugh Cairns and Mr. Druce for the defendants. The Vice-Chancellor said that the plaintiff sought to restrain the defendants from proceeding to purify gas in the manner described in the bill, in infringement of the patent the validity of which was now secured to him by the judgment of the Lord Chancellor in "Hill *v.* Evans." The question was, in reality, whether a substance which had been used by the defendants was a substance the use of which constituted an infringement of the plaintiff's patent. The invention claimed by the plaintiff was the purification of coal gas "by passing it through precipitated or hydrated oxides of iron." Considerable doubt had arisen upon these words as to whether they were sufficiently large to include the native oxides elaborated by the laws of nature, as opposed to what might be termed the artificial oxides produced by the hand of man. It was admitted that the validity of the patent had been established, but it was said that it had been established on the ground that natural oxides were not included in it. His Honour, after referring to the judgment of Baron Bramwell in the Court of Exchequer, and to his own judgment upon a former occasion, said that the view of the Lord Chancellor in "Hills *v.* Evans" was much more favourable to the plaintiff than that taken in either of those judgments. His Lordship, instead of contrasting artificial oxides with native oxides, preferred to take "precipitated" as opposed to "hydrated" oxides. In this view every precipitated oxide, whether natural or artificial, would be included in the patent, and the plaintiff would no longer be embarrassed by the use of the term "hydrated." Upon the first branch of the case, he should direct an issue to determine whether the material used by the defendants was a precipitated oxide of iron or not. Upon the second point which had been argued, he was of opinion that the plaintiff was entitled to an injunction restraining the defendants from renovating or reoxidizing the purifying materials used by them by means of exposure to the air, so that they might be used over and over again. It was arranged that the issue should be tried before this Court, without sending the plaintiff to a Court of Law.

ANSWERS TO CORRESPONDENTS.

Permanganate of Potash.—J. M. C.—We have received from Mr. Condy a specimen of pure permanganate of potash in beautiful crystals. Upon examination the salt appears to be free from impurities, and will be found very useful in the preparation of standard solutions and for general laboratory purposes. We cannot do better than to advise our correspondent to get some of this salt.

Combustion of Turpentine in Chlorine Gas.—Obliged Correspondent.—The precautions necessary to ensure success are to have the turpentine dry and the chlorine unmixed with air. A piece of filtering paper must then be saturated with the turpentine and suspended in the centre of a jar of gas, when it will immediately inflame.

Diamant.—Boil olive oil, litharge and water together until they unite into a mass of soft, solid consistence. Roll out on wet marble slabs and macerate in cold water. The process is a tedious one, and requires to be pursued with strict reference to many precautions, which would take up too much of our space to give in detail.

THE CHEMICAL NEWS.

VOL. V. No. 117.—March 1, 1862.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Products Resulting from the Simultaneous Action of Air and Ammonia on Copper, by M. E. PELIGOT.

IN a previous communication I called the attention of the Academy to the phenomena resulting from the contact of metallic copper with air and ammonia. I showed that the solution of copper thus obtained possessed in a high degree the property of dissolving cellulose, silk, and several other organic substances which resist the action of ordinary solvents. This liquid has advantageously taken the place of the ammoniacal hyposulphate of copper, used by M. Schweitzer, of Zurich, to whom is due the discovery of this curious phenomenon.

In the paper on this subject, which I read to the Academy at the end of the year 1858, I mentioned the existence in this solution of a salt of copper, produced by an oxygen acid of nitrogen which I thought was nitric acid, resulting from the combustion of ammonia by atmospheric oxygen in presence of copper. I afterwards found that the acid produced is nitrous acid, by saturating with pure nitric acid the sky-blue liquid so rapidly obtained; this gives, by the addition of nitrate of silver, a crystalline precipitate of nitrite of silver.

This production of nitrous acid had, unknown to me, been previously proved by M. Schönbein, who at the time published in the German journals a paper in which he shows that by passing ammonia over spongy platinum, nitrite of ammonia is produced. M. Schönbein shows that the same salt is produced in presence of copper. "Copper," he says, "at ordinary temperatures exposed to ammonia, fixes oxygen, and forms nitrite of ammonia. If fifty grammes of finely divided copper are introduced into a flask filled with oxygen, or atmospheric air, the mass becomes heated, white vapours are given off, which are nothing else than nitrite of ammonia; for a strip of starch paper with iodide of potassium, and previously acidulated, plunged into the flask, will immediately turn blue. . . . The blue solution of copper produced, contains not only oxide of copper, but nitrite of ammonia."

M. Schönbein has not separated from this solution the product, the formation of which he has pointed out. The knowledge of his labours has not deterred me from pursuing the more complete study of these curious phenomena of oxidation—a study I had before begun.

The process which I have found to succeed best for obtaining large quantities of the ammoniacal solution of copper, which is, so to speak, the raw material necessary for this research, consists in introducing into large flasks, of from 12 to 15 litres capacity, from 15 to 20 grammes of copper, and from 60 to 80 cubic centimètres of concentrated ammonia. The metal proceeding from the reduction of a salt of copper by iron or zinc, is thrown against the damp sides of the vessel, to which it adheres under the form of a thin layer. In a few minutes the flask becomes heated, and filled with thick white fumes, which being condensed on a cold wet body, give all the characteristics of nitrite of ammonia. When the reaction seems finished, change, by the aid of a bellows, the atmosphere of the flask, which will then contain nothing but nitrogen; repeat this several times at intervals, taking care to renew equally the points of contact of the metal and the products of its oxidation, with the ammoniacal liquid and the air. Invert and drain the flasks, and wash them out several times with liquid ammonia. Independently of the blue solution obtained, there remains in the water and in the ammonia an insoluble product of a variable colour, being sometimes olive-green, sometimes brown or yellow. It is a mixture of the two oxides of copper and of unattacked metal. The blue liquid contains only a fourth or fifth part of the copper employed.

The presence of an ammoniacal salt singularly accelerates this reaction. By using liquid ammonia, previously saturated with sal-ammoniac, the whole of the copper will be attacked in a few instances, provided there is an adequate quantity of atmospheric air present. The metal then will be completely dissolved, doubtless owing to the tendency of salts of copper to combine with ammoniacal salts.

By evaporating *in vacuo*, at a carefully managed, or even at a cold temperature, blue solution obtained with copper, air, and ammonia (without adding an ammoniacal salt, which would complicate the already difficult process of separating the products), a non-homogeneous product is obtained, parti-coloured with violet, green, and blue. Cold water separates from it nitrite of ammonia almost free from copper; but this salt cannot be obtained in the isolated state, its solution yielding water and nitrogen in proportion as it concentrates.

The blue solution, when boiled, yields black oxide of copper and nitrite of ammonia; that obtained by the aid of sal-ammoniac, furnishes a green crystalline residue of oxichloride of copper.

Ordinary solvents will not effect the separation from the residue (left by evaporation conducted at a carefully managed or even cold temperature) of the products which it contains. Having long sought for processes suitable to effect this separation, I have obtained very précise results, by employing as a solvent alcohol, previously saturated with ammoniacal gas. In the same way I have obtained, in a crystallized state, the salt, which is the principal product of the reaction. The properties of this new body illustrate perfectly the production and the nature of the complex residue furnished by the blue solution when concentrated or when submitted to rapid or spontaneous evaporation.

To obtain it in large quantities, evaporate to dryness, in a porcelain capsule placed in a water-bath, the blue liquid produced by the simultaneous action of air and ammonia on copper; pulverize the residue, and submit it to the action of boiling ammoniacal alcohol. The

filtered liquid, by cooling, deposits this salt in the form of needle-like prisms of a beautiful violet-blue colour. The mother-water from which it is separated, will serve anew to treat in the same way the residue left by the ammoniacal alcohol, or a fresh quantity of the rough product yielded by the evaporation of the copper solution. The matter which resists the action of this solvent, is the excess of oxide of copper contained in the solution.

The composition of the blue crystallized salt, dried at the ordinary temperature, is represented by the following formula:—

$$NO_3.CuO,NH_4O.HO.$$

By raising the temperature to 100°, this salt becomes green, gradually losing all the water and ammonia which it contained. To effect this result, the salt must be maintained for several days at 100°. The product which remains is anhydrous nitrite of copper, of the composition—

$$NO_3.CuO.$$

Certain difficulties in the analysis of nitrite of copper and ammonia, long left me in doubt as to its true composition. As this salt when heated decomposes with deflagration, it is impossible to estimate by calcination the oxide of copper which it contains. When the latter body is separated by caustic potash, there is always an excess, however carefully the precipitated oxide of copper may be washed; even when the washings are free from alkali, the oxide itself, after being made red-hot, contains a considerable quantity of it; there is, in fact, a decided alkaline reaction. The only process which I have found successful, is to calcine the salt with quenched and pulverized quartz. It leaves thus 35·2 per cent. of oxide of copper, which is exactly the quantity required by the preceding formula.

A small quantity of nitrite of copper and ammonia, wrapped in paper and placed on a steel anvil, detonates when struck with a hammer.

The salt dissolves in contact with a little water, producing intense cold; by leaving this solution to spontaneous evaporation, a portion of the ammonia frees itself and is disengaged. Nitrite of ammonia and a green crystallized salt are thus obtained, the composition being represented by the following formula:—

$$NO_3.3CuO.NH_4O.$$

There are considerable difficulties in the way of the regular production of this salt, furnished by the solution obtained directly from air, ammonia, and copper; for it is itself decomposable in water used in large quantities.

Water acts in a remarkable manner on these different products. By pouring a large quantity of it either into the solution obtained directly from copper, ammonia, and air, or on the two products derived from this solution, a beautiful turquoise-blue precipitate is obtained. This body is hydrate of copper, Cu O. HO. By calcination it gives 80 to 81·5 of black oxide of copper. The formula CuO.HO, requires 81·6 per cent.

This oxide appears identical with that formed by treating a soluble salt of copper by an excess of soda or potash. But all chemists know that hydrate of copper prepared in this way is not stable; it loses its water, and turns black almost immediately, even while being washed in cold water. The blue oxide which I have obtained resists the action of boiling water, and can be heated to 100° without undergoing alteration. It retains, it is true, traces of ammonia, which repeated washings fail to remove. But the quantity of this body is not greater than that of the foreign substances always found, when searched for, in all the oxides and salts obtained by precipitation; only ammonia is more easily detected, on account of the sensibility of the reagents serving to indicate its presence.

Blue hydrate of copper, which may be ranked as a new and useful acquisition to science and industry, absorbs atmospheric carbonic acid slowly and without changing colour. It is a very finely-divided crystalline precipitate, and its beautiful colour might doubtless be available for painting, and for printing coloured stuffs and paper. If this hydrate could only be produced under the above circumstances, its industrial uses would of course be very limited. But while studying its properties, I have found several methods of preparing it with all the salts of copper soluble in water, particularly with sulphate of copper. In fact, it can be obtained by treating with an alkali a salt of copper dissolved in a quantity of water to which a slight excess of ammonia has been added previously; also by pouring potash or soda into a salt of copper mixed with an ammoniacal salt; or by adding a quantity of water to a slightly ammoniacal solution of nitrate of copper. Thus the expense of preparing this colouring matter is no obstacle. It must not be confounded with the product known as "English blue ash," the preparation of which has always been kept secret. The English blue ash is composed of carbonate of copper, the tint of which, though rather darker, is generally less pure than that of the hydrate of copper.

Concentrated liquid ammonia dissolves from 7 to 8 per cent. of this hydrate. This solution, the blue colour of which is common to all salts of copper in contact with an excess of ammonia, is decidedly the best solvent for cellulose and the other substances more or less soluble in M. Schweitzer's reagent.

One of its advantages is that the dissolved substance can be precipitated without alteration by the addition of an acid; while, by operating under the same circumstances, with the blue liquid resulting from the action of air and ammonia on copper, the nitrous acid which becomes free, acts more or less energetically on the organic substances contained in the solution. It is, moreover, from the presence of this oxide, which is found in the liquid in simple solution in ammonia, that the solution obtained by the direct action of air and ammonia on copper, derives the property of dissolving cellulose; for by placing this substance in contact with nitrite of copper and pure ammonia, dissolved beforehand in a little water, it neither gelatinizes nor disappears, as happens when we employ either an ammoniacal solution of oxide of copper or the liquid furnished by copper under the simultaneous influence of air and ammonia.—*Comptes Rendus.*

Contributions to the History of Oxygen,
by M. C. F. Schönbein.

Action of Oxygen on Hematoxyline.—The three modifications of oxygen exert an action upon hematoxyline very similar to that which they have upon pyrogallic, gallic, and gallo-tanic acids. Free or combined negative oxygen rapidly colours this substance brown-red, then brown, and finally decolorises it, rendering it strongly acid. Oxy-water can remain a considerable time in contact with it without being destroyed, and without colouring it violet. Neutral oxygen does not act upon dry hematoxyline; when this substance is in solution it oxidises it, and the production of oxy-water

can be rendered evident when there is rapidly agitated in a flask holding a like 100 grammes of a solution of hematoxyline containing a thousandth part of this body and some drops of caustic soda added until the liquid begins to be coloured cherry-red, and then acidulated with dilute sulphuric acid.

Action of Oxygen on Aniline.—The same may be said of aniline as of hematoxyline. Free negative oxygen and the ozonides rapidly transform it into a brown resinous matter, almost insoluble in water, soluble in aniline, alcohol, and ether, and colouring these liquids reddish brown. Oxy-water has no action upon aniline. Ordinary oxygen rapidly resinifies it in the light, and alters it very slightly in the dark.

On Nitro-peroxide of Hydrogen, and on the Degrees of Oxidation of Nitrogen.—When a current of bin-oxide of hydrogen is passed into an aqueous solution of peroxide of hydrogen, a strongly acid liquid is obtained which energetically blues tincture of guaiacum, which is not coloured blue by chromic acid, and which decomposes iodide of potassium with liberation of iodine and tumultuous disengagement of binoxide of nitrogen, and formation of nitrate of potash. This liquid likewise transforms yellow—into red—cyanide of potassium, and oxidises pyrogallic acid with production of humus compounds, and always with disengagement of binoxide of nitrogen.

Nitric acid is not capable of producing these reactions, especially in the state of dilution in which it could exist in the liquid acted upon. Neither can it be attributed to one of the other oxides of nitrogen. M. Schönbein thinks that these facts may be explained by admitting that a direct combination is formed between the peroxide of hydrogen and the binoxide of nitrogen, in which the second atom of oxygen of the peroxide of hydrogen is modified by the combination, and is no longer in the positive but in the negative state; the formula of the new body would thus be $NO_2 + HO.\Theta$.

The liquid likewise contains nitric acid, which is formed by the combination of the oxygen in the $HO.\Theta$ with the nitro-peroxide of hydrogen. When a sufficient quantity of oxy-water is added to the liquid it ceases to produce the above reactions; however, some time elapses before all the nitro-peroxide of hydrogen has disappeared, and by the known reagents the simultaneous presence of oxy-water and the new nitro combination can be demonstrated.

The solution of nitro-peroxide of hydrogen may be preserved for some time with all its properties out of contact with air and at a low temperature. It gradually loses binoxide of hydrogen, its oxydising properties diminishing at the same time.

A perfectly analogous liquid to the preceding, and one which has the same reactions, may be obtained by mixing hypo-nitric acid with water in any proportion. When care is taken to pour the hypo-nitric acid, drop by drop, into the well-cooled water there is scarcely any disengagement of gas. In this case M. Schönbein supposes that two molecules of hypo-nitric acid and two molecules of water are transformed into one molecule of nitric acid, $NO_5.HO$, and one molecule of the compound $NO_2.HO_2$.

When a certain quantity of solution of nitro-peroxide of hydrogen is shaken with several times its volume of ether the aqueous solution loses its oxydising properties. The ether, after having had removed from it, by means of a little potash, a small quantity of nitric acid, is not blued by chromic acid, and does not act upon iodide of potassium, ferrocyanide of potassium, nor pyrogallic acid, at least, unless a small quantity of dilute hydrochloric, nitric, or sulphuric acid is added. Its action on these different bodies is then exactly similar to that produced by the original aqueous solution. When the ether is shaken with alkaline solutions nitrates are produced. Out of contact with air the ethereal solution does not alter; in contact with pure oxygen or with air it becomes gradually acid, with formation of nitric acid. It can be distilled without losing its properties.—*Journal für Praktische Chemie*, lxxxi. 257.

Chemical Notices, by M. J. J. Pohl.

Analysis of a Kind of Flint Glass which Tarnishes.—The following are the results obtained by M. Pohl, compared with those obtained by M. Dumas, in the analysis of Guinand's flint glass:—

	Pohl.	Dumas.
Silica	75·24	42·50
Oxide of lead	10·48	43·50
Oxide of iron	traces	
Oxide of aluminium		1·80
Lime	1·48	0·50
Potass	12·51	11·70
Arsenical acid		traces
	99·71	100·00

It is shown that the flint glass analysed by M. Pohl contained a large excess of silica.

Facts Relative to the Solubility of Chemical Combinations.—100 grammes of liquid ammonia, density from 0·986 at 15°, will dissolve, at 80° temperature, 0·5063 gramme of bromide of silver, previously dried at 100°; recently precipitated bromide of silver is, doubtless, much more soluble.

The same quantity of ammonia will dissolve, at 80°, 1·4916 gramme of chloride of silver, previously dried at 100°.

The author has before shown that glucose, while dissolving in water, lowered the temperature; this property seems to be common to all sugars.

560 grammes of cane-sugar dissolved in 1·12 kilogramme of water, lower the temperature from 16·62° to 15·5°. The temperature is lowered from 16·5 to 16·62° by dissolving 175·2 grammes of sugar of milk in 1·051 kilogramme of water. 87·6 gramme of manite, dissolved in 348 grammes of water, lower the temperature from 16·5° to 13·5°.

Sugar of gelatine seems to produce the same phenomenon.

The following salts behave in a similar manner:—

Bromide of Sodium.—20 grammes of this salt, dissolved in 20 grammes of water, lower the temperature from 21·25° to 8·38°.

Acetate of Soda.—140·16 grammes, with an equal quantity of water, lower the temperature from 15° to 0°.

Chloride of Barium.—140 grammes, dissolved in 350 grammes of water of 13·6°, depress the temperature to 5·85°.

Succinate of Soda.—This salt, while dissolving, induces a considerable diminution of temperature.

On the Composition of Carbonate of Potassa Crystals.—The author first remarked that the analyses, formerly made by M. Berard, of hydrated carbonate of potass, agree in no respect with the formula $KO.CO_2$, 2 HO; they nearly correspond with the formula $KO.CO_2$, 4 HO. M. Giese only has obtained a salt with the formula $KO.CO_2$, 2 HO.

M. Pohl has analysed large pointed hexagonal prisms, formed in a saturated watery solution of carbonate of

potass. Dried over sulphuric acid, these crystals present the composition 2 ($KO.CO_2$), 3 HO; after having been heated to 100° they show a composition expressed by the formula $KO.CO_2$, HO. When heated for some hours at 130° to 135°, the water is completely expelled, and their crystalline form destroyed.—*Sitzungsberichte de K. Akademie der Wissenschaften zu Wien.*

On Tungsten and Some of its Alloys, by M. F. A. Bernoulli.

In this work the author proposes to investigate principally the alloys of tungsten with other metals. Having, in the first place attempted to obtain metallic tungsten in the fused state, he did not succeed, although having at his disposal very perfect means of heating.

Tungsten is always obtained in the form of a black powder, having a density of 17·1 to 17·3 for the metal reduced with carbon, and 17·9 to 18·2 for the metal reduced by hydrogen.

Upon fusing turnings of gray cast iron with tungstic acid or white cast iron with the addition of carbon, the author obtained alloys possessing remarkable properties and especially of great hardness. He admits that the carbon present in cast iron in the state of combination does not act upon tungstic acid, and that only the carbon in the state of mixture reduces the tungsten to the metallic state. Indeed, the alloys obtained still contain carbon, and that in a proportion up to 5·18 per cent. Tungstic acid can be replaced in the preparation of these alloys by wolfram or by Scheelite.

With respect to copper, lead, antimony, bismuth, zinc, nickel, and cobalt, the fusion of these metals with tungsten does not yield alloys. Upon reducing a mixture of the oxides, very slightly fusible alloys are obtained. The fusion of tungsten with silver gives either non-homogeneous ingots or infusible masses; it is the same with gold.

On the Degrees of Oxidation of Tungstic Acid.—It is known that tungstic acid assumes, under some circumstances, a green colour. It has been supposed that this coloration may be due to a partial reduction of the acid. This is not the case; indeed, yellow tungstic acid, heated to red-whiteness in a current of oxygen, becomes green, and assumes, at the same time, a crystalline texture.

The same transformation takes place when the acid is heated in a crucible in the midst of an oxydising atmosphere; at the same time, a partial sublimation of the acid in the crucible is observed. Upon reducing by hydrogen the yellow and the green acid, it is found that their composition is exactly the same, and that, therefore, they constitute two isomeric modifications of the same oxygenated combination. To these two modifications, of which the author proposes to designate the first by the name of *pyro-tungstic acid* (*b*), reserving for the first the name of *tungstic acid* (*c*), must be joined a third tungstic acid, the *meta-tungstic acid* (*a*) of M. Scheibler, which is perfectly soluble in water.[1]

The experiments of M. Bernouilli lead him to assign to tungsten the equivalent 93·4, his analyses yielding numbers varying between 93·22 and 93·97.

After having quoted a certain number of old analyses of wolfram and Scheelite, and comparing them with his own, the author terminates by proposing the following process for the estimation of tungstic acid:—The insoluble tungstates, reduced to fine powder, are fused in a platinum crucible with carbonate of soda. The fused mass is taken up with water; the aqueous solution, filtered to separate the insoluble metallic oxides, is neutralised in the cold with acetic acid. The silica, if there be any, is completely precipitated by a slight excess of acetic acid, and may be separated by filtration.

The solution is treated with acetate of lead, when there is precipitated tungstate of lead, $PbO.WO_3$, easy to wash. This salt is digested with the filter during half-an-hour in sulphide of ammonium, which dissolves the tungstic acid and precipitates the lead in the state of sulphide. The solution is filtered and evaporated to dryness in a platinum capsule, with the addition of nitric acid, and the residue is calcined and weighed.

If the acid contains niobic acid, it is necessary before completing the evaporation to dryness with nitric acid to take it up with dilute ammonia, which leaves the niobic acid undissolved.—*Poggendorff's Annalen.*

[1] *Monatsberichte der Berliner Akademie*, 1860.

On a New Harmonica Chemica, by Dr. T. L. Phipson, *F.C.S., &c.*

It is known that protosulphite of iron, Fe O. SO_3 + 7HO, contains seven equivalents of water. Six of these can be driven off at 100° C., but the other one equivalent only at a much higher temperature. Now, when Fe O. SO_3 + 7HO is taken and heated gently, to drive off six equivalents of water, and the white mass then pulverized, and introduced into a short glass tube about four inches long and three-quarters of an inch wide, perfectly dry, and supported by the cork, through which passes a little piece of bent tube, a curious phenomenon is observed when the tube is heated by means of a spirit-lamp. The salt occupying the narrow end of the tube, the lamp must be moved backwards and forwards along the whole length of the tube, until a buzzing sound begins to be heard. It is then deposited, and left under the dry sulphate. As soon as the last equivalent of water begins to leave the salt, a low buzzing sound begins to be audible, and soon increases rapidly in intensity as long as the salt continues to lose any water. This sound appears to be due to a rapid vibration of the tube caused by the evolution of successive molecules of water from the salt; it does not cease until the whole of the water has been expelled; and if then the tube and its contents be allowed to absorb moisture from the air, the experiment may be repeated the next day. I have thus produced this loud humming noise three successive times in the course of four days. When the white sulphate, containing only one equivalent of water, is heated in a capsule, the loss of the last molecule of water takes place with a series of cracking noises; at the same time the substance is more or less projected out of the capsule. Now, in the tube supported by the cork, the substance being finely pulverised, no decrepitation takes place, but a series of vibrations, which produce the humming noise in question.

If, in making this experiment, any water is allowed to condense in the small bent tube, the noise ceases.

Experiments on the Deportment of Carbonate of Lime at a High Temperature, both with Fluxes and alone, by G. Rose.

In a recent paper on the heteromorphous states of carbonate of lime, presented to the Berlin Academy of Sciences, Rose gives the following experimental results:

I. On heating a mixture in atomic proportions of the carbonate of soda and potash in a platinum crucible until in perfect fusion, then adding small quantities of chloride of calcium, the latter will be completely dissolved without effervescence. If the fused mass is allowed to cool, and a portion of this is placed in water at the ordinary temperature, it will gradually pass into solution, leaving a pulverulent residue of carbonate of lime. An examination of this residue shows it to consist of an aggregate of very small globules; after twenty-four hours—sometimes in less time—these increase in size, and are converted either into beautifully crystallized single rhombohedrons or rhombohedral groups of calcite.

II. If another portion of the mass is thrown into boiling water, boiled for some time, and the residue examined under the microscope, it will be found to consist of small prisms of aragonite, with occasional rhombohedral crystals of calcite, but none of the above-mentioned globules; if the residue be allowed to remain under water, the prisms after a time are converted into minute rhombohedrons of calcite. These phenomena are then identical with those obtained when a mixture of the solutions of carbonate of soda and chloride of calcium is made, as long since described by the author in his first article on this subject. (See *Pogg. Ann.* (1837), xlii. 354.)

III. Instead of adding chloride of calcium to the fused carbonates, powdered calcite, or rhombohedral fragments of calcite, chalk, or aragonite, may be substituted; these dissolve completely, without effervescence in the flux, and give the same results as above-mentioned when the fused mass is treated with hot or cold water.

IV. Oxalate of lime at a low red heat, after losing the water it contains, is converted into carbonate of lime with evolution of carbonic oxide; under the microscope the product appears to consist of minute amorphous globules[1] similar to those obtained above, and these remain unchanged when placed in water; even when boiled, the globules still retain their amorphous form—they are not converted into calcite.

From the experiments thus far described, it will be seen that rhombohedral carbonate of lime is never a direct product. But according to the well-known experiments of Sir James Hall, made in 1804, this has been directly produced when chalk and compact limestone were exposed to a high temperature under great pressure. The author, assisted by Mr. Warren Siemens, has repeated Hall's experiment. A gun-barrel was charged with dry elutriated chalk, this last was rammed into a compact mass, and then the gun-barrel was hermetically sealed at both ends, and exposed to the heat of one of Mr. Siemens' gas furnaces. During the experiment the barrel sprung, and in the crack there appeared a faint blue flame, evidently of carbonic oxide; the gun-barrel was then removed from the furnace. Upon opening the barrel, the chalk was found converted into a compact, light, bluish-white coherent mass, slightly lustrous on the fracture, and with cracks running through the whole. The surface was covered with a snow-white, earthy, well-defined crust, and the cracks were lined with white earthy particles; these, as well as the crust, were composed of caustic lime. The compact mass, however, on examination proved to be unchanged in chemical properties; and its physical properties, though seemingly changed, when examined under the microscope showed the same small globules, and identically the same properties as the unignited amorphous chalk. Although somewhat more coherent, the chalk was not materially altered, and in no-wise converted into crystalline calcite.

The experiment repeated with fragments of rhombohedral calc-spar, was also interrupted by the rupture of the gun-barrel. The smaller fragments were converted into caustic lime without change of form, the larger pieces were only superficially altered; notwithstanding the great heat, the inner mass was unchanged, and the limit of the surface-crust sharply defined. The same result was obtained by the author under different conditions: Mitscherlich presented him with a mass of limestone from Rüdersdorf, which, on account of its size, had passed through the lime-kiln without being completely burned. It contained a kernel of unburned lime, and, notwithstanding the great heat through which this had passed, it proved on examination to have the same characters as the compact limestone which had not been exposed to the heat of the kiln.

It appears therefore, from these experiments, that chalk or compact limestone cannot be converted into crystalline limestone (or calc-spar) by exposure to a high temperature in closed vessels; and as a general fact, that rhombohedral carbonate of lime is not formed in the dry way. The author further observes, that on comparing accurately the description of Hall's[2] experiments and those afterwards made by Bucholz[3], that probably they obtained results similar to those just described, and that the slightly coherent, but otherwise unaltered, mass was erroneously considered to be crystalline marble.

Notwithstanding the frequency with which this experiment of Hall's has been quoted, and the use that has been made of it, not only in explaining geological phenomena, but in serving as the foundation of whole theories, it was never repeated[4] or confirmed; and the experiments of the author show how hasty the conclusion was. It is not to be disputed that at the junction with granite and basalt, compact limestone and chalk are often converted into marble, as in Paradiesbacken, near Drammen, in Norway, and near Belfast, in Ireland; but these changes cannot be considered as due to heat alone, they were manifestly assisted by other agencies,—a conclusion also arrived at by Bischof, although in a different manner.—*Pogg. Ann.*, cxi. 156.

[1] Oxalate of lime is amorphous, and when converted by heat into carbonate, this character still remains.

[2] *Gehlen; Neues allgem. Journ. d. Chem.*, v. 287.

[3] *Gehlen: Journ. f. Chem. u. Phys.*, i. 271.

[4] Bucholz made his observations incidentally in the production of caustic lime from chalk, which in the experiment had not been entirely burned.

PHARMACY, TOXICOLOGY, &c.

On the Behaviour of Essential Oils to Iodine and Bromine, by John M. Maisch.

(*Continued from page* 64.)

Oleum Juniperi.—Yellow, very old, resinified, thick, but said to be unadulterated.

Iodine.—Slow reaction, little heat, no vapours; the residue consists of a brownish black resin and a brownish yellow liquid; after some time they are miscible to a dark brown oily liquid; the odour of which is juniperous, balsamic, reminding of turpentine.

Ether sol. iodine.—Some irregular spreading out, homogeneous liquid of deep iodine colour, spreading after a while a thinner and lighter liquid; the whole

gradually congeals into a reddish-brown resinous mass of the consistency of butter.

Bromine.—Fulmination with very few gray vapours; the residue is a brownish red resin and a yellow liquid having a greenish tint, the whole being miscible to a wine yellow oil; the odour is little altered, scarcely terebinthinate.

Oleum Petræ.—Colourless, very thin and mobile.

Iodine.—Not soluble, but gradually altering into a tough, dark yellow-brown, resin-like mass, which firmly adheres to the glass, the oil assuming a pale carmine colour, which, in contact with the air, gradually turns brown. Z.

Very little soluble, without any visible reaction to a pale, gradually deeper carmine-coloured liquid; otherwise, the oil is unaltered.

Ether sol. iodine produces some spreading of a small quantity of a carmine coloured oil, with a larger portion of a nearly black liquid; after this reaction has subsided fine spots of a brown coloured mass remain upon the sides of the vessel, a black oily sediment separates, while the supernatant transparent liquid is of a rose colour. After six hours, part of the oil has worked itself over the edges of the vessel, leaving a little of a deep carmine coloured liquid behind, and a black mass, which in very thin layers appears of a brown carmine red.

Bromine.—No reaction by three drops; the bromine collects on the bottom, dissolves slightly in the oil, colouring it yellowish red, and constantly evaporating from the surface with a slight effervescent motion; six hours afterwards, the whole mixture has evaporated, leaving no residue, and the odour of the oil unaltered.

Ether sol. bromine sinks to the bottom, leaving the supernatant oil scarcely tinged with yellow.

Oleum Sabinæ.—Thin, colourless.

Iodine.—The oil distilled from the leaves and the tops of the branches, showed with iodine the highest temperature, fulminated quickly and violently, and left a reddish brown residue, of the consistence of honey, easily miscible with the undecomposed oil. Another heavier oil was after the reaction with iodine, thinner and olive green. Z.

Brisk detonation, with violet and gray vapours; the residue consisted of a little greenish brown sediment and a blackish green clear liquid, which were readily miscible.

Ether sol. iodine yields a light reddish yellow liquid and a heavier iodine coloured oily fluid; the two are miscible, though not very readily.

Ether sol. bromine.—The mixture turns instantly colourless with a hissing noise; a lower stratum is separated, which has a whitish colour, changing to dark brown, while the upper liquid, at first transparent and colourless, is rendered slightly milky.

Oleum Succini.—Thin, and nearly colourless.

Iodine.—Without visible reaction, the dissolving iodine combines to a dark, thick liquid, which, after some time, mixes but imperfectly with the thinner yellowish brown oil. Z.

It is partly dissolved with little motion to a yellowish brown liquid, scarcely miscible with the thicker lower stratum.

Ether sol. iodine.—Without any reaction two strata are formed, the upper one of which is thin and yellow, the lower one thicker and of iodine colour. After six hours, the greater part of it has been working itself out of the vessel, leaving a thin yellow liquid behind.

Bromine.—Violent fulmination, most of the oil is thrown out of the dish.

Ether sol. bromine generates some white fumes; the oil remains nearly unaltered in colour, while the lower liquid soon takes a purplish brown colour.

Oleum Terebinthinæ.—Thin, colourless, free of resin.

Iodine fulminates with energy, evolving violet vapours: the residue consists of undecomposed, brown-red oil, and a blackish brown iodine compound, which, after some time, is miscible to a soft dark brown mass, which in contact with the air turns greenish brown, and has a disagreeable empyreumatic terebinthinate odour. Z.

Violent fulmination, with purple-coloured vapours; the oil has a brownish carmine colour, turning to red brown; around the dark sediment some greenish coloured liquid is observed, the whole after a while readily miscible; odour terebinthinate, empyreumatic.

Ether sol. iodine settles to the bottom without any visible reaction, the oil gradually assuming a brown carmine colour. After six hours, it has been spreading out of the watch crystal, leaving a light brown resinous mass behind, of a peculiar resinous, empyreumatic odour.

Bromine causes a most violent fulmination, with an abundance of white vapours; much of the oil is thrown away, the remaining oil is colourless and unaffected.

Ether sol. bromine evolves white vapours; during the reaction the whole turns white; the sediment then assumes a light yellowish colour, leaving the oil colourless; the two strata are not miscible.

II. The Oxygenated Oils.—*Oleum Absinthii.*—Brown, thick, old.

Iodine.—The action of iodine varies considerably with the age of the oil or of the herb from which it has been produced. Fresh and well-kept oil is rendered of a grass green colour, with little heat, but otherwise without reaction; older oils may show a radiating motion, with iodine vapours and a tough extractive residue; or detonation, with evolution of violent vapours, and leaving a brown syrupy residue. Z.

It dissolves with radiating motion to a deep brown syrupy liquid.

Ether sol. iodine mixes with the oil to a dark olive brown liquid.

Ether sol. bromine yields a blackish brown mixture without any reaction.

Oleum Anisi.—Transparent, colourless.

Iodine.—The oil, moderately heated, gives off reddish and white vapours; by stirring, the whole hardens quickly to a resin-like mass, which on cooling is fragile. Z.

It dissolves slowly in the cold oil with some radiating motion and scarcely any vapours; the particles of iodine adhere together and the residue is an iodine coloured liquid. But if the reaction has been facilitated by quick and continued agitation or slight heating, the whole soon congeals to a brittle brown mass.

Ether sol. iodine causes little spreading, is miscible to an iodine coloured liquid, and in a few minutes congeals to a butyraceous mass, which in six hours is still sticky and has turned to a deep coffee brown; in twelve hours the mass is hard and brittle.

Ether sol. bromine produces a quick, radiating motion; the colour is white, afterwards colourless; more bromine alters it to a light purplish brown.

Oleum Anisi Stellati.—Thin, pale yellow.

Iodine generates without effervescence, some bluish red vapours and little heat; on stirring the mixture it soon hardens to a resin-like brittle mass. Z.

There is scarcely any difference in its behaviour to iodine from that of oleum anisi.

Ether sol. iodine.—Perfect solution of iodine colour, without spreading; it thickens in a few minutes to a butyraceous mass, which after several hours is hard and brittle.

Bromine.—Whizzing noise, white fumes, effervescence; the residue consists of a reddish resinous body, and a liquid of a greenish-yellow colour, which is somewhat thicker than the original oil.

Ether sol. bromine.—Quick radiating motion; the oil turns white immediately, and remains of that colour; more bromine colours it yellowish red, it evaporates in two layers, the upper one of which is lighter coloured, somewhat turbid, and turns to a purplish colour.

Oleum Aurantii Corticis.—Pale yellow, thin, about a year old.

Iodine.—The fresh oil shows brisk fulmination, less than oil of neroli. A thick, dark iodine compound is separated from the brown red liquid residue, the whole miscible to a not homogeneous fluid. Beschorner observed no heat or fulmination, but a radiating motion and brown red solution, properties which probably belong to oil distilled from the old dry peel. Z.

Some radiating motion and no vapours were observed; the oil is of a reddish yellow, miscible with the iodine sediment to an iodine coloured solution.

Ether sol. iodine.—Spreading; forms two liquids, the upper one pale yellowish, the lower one thick, reddish brown, almost black; in six hours the whole was altered into a dark pitchy mass of an acidulous odour.

Bromine.—The reaction is so violent that most of the oil is thrown out of the vessel, and many white fumes are given off, with the evolution of much heat; the oil left behind assumes a brownish colour.

Ether sol. bromine generates some white vapours; the oil is at first left uncoloured, while a brisk reaction is going on; it then assumes a bright yellow colour, in a short time yellowish brown stripes appear, forming into a slowly-rotating crescent, which leaves a brown colour behind; the rotation ceases as soon as the colour has become a uniform deep brown; the supernatant oil is now of a pale brown colour and perfectly transparent.

The rotating crescent appears to be a characteristic reaction of oil of orange-peel, with ethereal solution of bromine, and appeared in all the repetitions of this experiment. I have tried to find out how far it is influenced by the presence of other similar oils, and will here state the results.

Oil of bergamot four parts, oil of orange one part.—The ethereal solution of bromine has but little radiating motion; a smal crescent appears and disappears alternately; after the reaction, the colour is of a deep reddish brown, the supernatant fluid is whitish, turbid, afterwards clear.

Oil of lemon four parts, oil of orange one part.—Around the ethereal solution of bromine in the centre of the oil, a brisk reaction takes place, and a bright yellow colour is produced, in which soon a small crescent is formed, running together to a brown spot; the whole sedimentary liquid assumes a yellowish brown colour; the supernatant is milky and brownish white.

Oleum Bergami.—Thin, greenish yellow.

Iodine.—Brisk fulmination with effervescence, violet and yellow-red vapours and much heat, more than oil of orange. The homogeneous residue is of a yellowish brown colour, an extractive consistence, and of an acidulous balsamic odour. Z.

Vivid reaction, with heat, detonation and the evolution of purple and yellowish fumes; the residue consists of a black extract and a yellow oil, which are readily miscible to a dark, nearly unctuous liquid; the odour is acetous, aromatic.

Ether sol. iodine.—Some effervescence; the homogeneal thin liquid is of a bright yellow iodine colour, gradually deepening; in six hours a dark brown pitchy mass remains behind.

Bromine.—Vigorous reaction, with detonation, heat, and white fumes; the residue is reddish yellow, the supernatant liquid yellow, and both are miscible to a greenish yellow oil; the odour is little altered and free of acetosity.

Ether sol. bromine.—The reaction takes place by an energetic working from the centre towards the circumference in one or two directions; the colour is greenish brown yellow, at the conclusion of the reaction reddish brown yellow; the supernatant liquid is slightly cloudy, whitish, subsequently limpid. (See also Oleum limonis.)

Oleum Cajeputi.—Thin, pale yellowish green.

Iodine dissolves slowly in the crude oil without visible reaction, the residue appears curdled. Another sample showed radiating motion, with few reddish fumes and little heat, and a more coherent residue. The rectified oil evolves yellowish red vapours, little heat, and leaves as residue a greenish brown curd, by agitation adhering to a crumbling mass, which appears to be characteristic for this oil. Z.

It produced a radiating movement, without vapours; the oil has an iodine colour, and the sediment by agitation runs together into a crummy resinous mass.

Ether sol. iodine.—Spreading, mixing to an iodine coloured liquid; after six hours the liquid had partly overrun the edges of the vessel, leaving a crummy mass, of a nearly crystalline appearance, together with very little fluid of a yellow iodine colour.

Ether sol. bromine sinks to the bottom; the supernatant liquid is scarcely coloured; the reaction is manifested by the appearance of minute greenish, and blackish green drops; they are with difficulty miscible by stirring, but separate again as a more homogeneous heavy liquid.

(To be continued.)

PHYSICAL SCIENCE.

On the Molecular Dissymmetry of Natural Organic Products, by L. Pasteur, *Member of the Chemical Society of Paris.*

(*Continued from page* 109.)

Hemihedrity[1] is assuredly, among the peculiarities of crystallisation, the one which is the easiest to seize in its external manifestation: consider, for example, a species of mineral crystallising in the cubic form. This form, as all know, may surround different kinds of determinate forms by the law of symmetry,—a law so natural that it is, that a form being given, all others compatible with it may be obtained by a contrivance which would consist in modifying, truncating, as Romé de Lisle said, at the same time and in the same manner, the identical parts. The identical edges are those which are at the intersection of faces respectively identical angles, those which are formed by dihedral angles respectively equal and similarly placed. For example, in the cube there is but one kind of solid angle and one kind of edges. Let one of the solid angles be truncated by a face equally inclined

[1] *Emisus*, half; *edron*, side.

to the three faces of the solid angle, and the seven other angles should be, at the same time, by a face of the same nature. This is observed in alum, in galena, and generally in all cubic species.

Let us consider a right prism with a rhomb base. The eight edges of the bases are identical If one is truncated, the seven others should be, and in the same manner. The four vertical edges are of another kind. Generally, they will not be truncated at the same time as those of the bases, and if they are, it will be differently. These examples alone will suffice to explain the law of symmetry and its application.

Nothing is more simple than to have a clear idea of hemihedrity. Experiment has long since shown Haüy was acquainted with the most celebrated instances, that in a crystal the half only of the identical parts are sometimes modified at the same time and in the same manner. It is said in such case that there is hemihedrity. Thus, the cube ought to be truncated at the same time on its eight solid angles. But, in certain cases, it is so only on four. Boracite affords an instance of this kind. Under these circumstances, the modification occurs in such wise that in prolonging the four truncations in a way to make the faces of the cube disappear, we obtain a regular tetrahedron. If the modification were applied to the four remaining angles it would produce another regular tetrahedron, identical with and superposable on the first, and differing from it only by its position on the cube. In the same way, let us take our right prism truncated on the eight edges of its bases. In certain species the truncation occurs upon half of the edges only, and it also happens here that the truncations, bearing upon edges opposite at each base and crossing at the two extremities, when prolonged lead to a tetrahedron. There are two tetrahedrons possible, as for the cube differently placed in relation to the prism, accordingly as it preserves such or such group of four truncations; but here the two tetrahedrons are not absolutely identical. These are symmetrical tetrahedrons. We cannot superpose them.

These notions suffice to show what is meant by hemihedrity and what is understood by hemihedric faces or forms.

Now, quartz, of which we just spoke, is one of the rare mineral substances in which Haüy found hemihedric faces. The habitual form of this mineral, a regular hexagonal prism, terminated by two pyramids of six faces, is well known. It is clear that the trihedral angles, situated at the base of the faces of the pyramid, are identical, and, consequently, if one of them bears a face, it ought to be reproduced on all the others. This is true of the face termed *rhombiferous* by mineralogists.

But Haüy first remarked in certain specimens, a face very different from this, which he designated by the letter x, which falls more on one side than on the other, without being double, as the law of symmetry would in this case require. Another very curious peculiarity of these crystals has not escaped crystallographers. It is, that the face x is inclined sometimes in one direction and sometimes in another. Haüy, who was fond of bestowing epithets appropriate to each variety of a species, applied the term *plagihedral* to the variety of quartz having the face x. Crystals in which the face x inclined to the right are designated under the name right plagihedrals, the crystal being adjusted in a suitable manner; and those crystals in which the face x inclined in an opposite direction are termed left plagihedrals.

Nothing is more variable than this character. Here it exists; there it is absent. Upon the same crystal there are angles which bear the face x; others, which should have it, have it not. Sometimes we find plagihedral faces to the right and to the left. Nevertheless, all persons versed in a knowledge of crystals, admit that there is in quartz a true hemihedrity in two opposite directions.

Here is placed a very ingenious approximation, due to Sir John Herschel, communicated to the Royal Society of London in 1820.

M. Biot, as I previously said, made the remarkable observation, that among the specimens of quartz, some deviated in the direction to the right and to the left. This stated, Sir John Herschel associated the crystallographic observations of Haüy with the physical remark due to M. Biot. Experiment confirmed the idea of a relation, in fact, between the right and left plagihedrals and the right and left optical deviations. Specimens of quartz which bear in one direction the face x, deviate the plane of polarised light in the same direction. Such is an exposition of the principal facts which have preceded the researches, an abridged history of which I have to relate.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

PHARMACEUTICAL SOCIETY.

Wednesday, February 5.

Mr. P. Squire, *President, in the Chair.*

(*Continued from page* 110.)

The third paper read by Mr. J. Attfield was entitled "*Poisons not always Poisons.*" In his microscopic examinations of the saline efflorescences mentioned in the preceding paper, Mr. Attfield discovered that some of the most active extracts in the Pharmacopœia—extract of colocynth and extract of nux vomica, for example—supported colonies of lively little animals, greatly resembling cheese-mites. They proved, in fact, to be a hitherto unknown species of *acari*. Other irritating substances have been known to support similar animals. Ginger has been found to be infested with them; but in this and similar cases it was supposed that they lived on the starchy matter and rejected the active principle. But it was impossible that they could eat extract of nux vomica without eating strychnia. It might be, however, that the strychnia was not assimilated. Mr. Attfield, therefore, collected some acarine excrement (the excrement floats on, while extract of nux vomica sinks in water), tested it for strychnia, and only discovered a faint trace of that body, which he believed to have been dissolved off the extract by the moist excrement. But, to prove that these animals could live on food that was to other animals a deadly poison, Mr. Attfield took several of them from the extract and put some into microscopic cells containing powdered strychnia, and others into empty cells. In two days those in the empty cells were starved to death, while those supplied with strychnia were as lively as ever. Some of the animals from extract of colocynth lived just as well on strychnia, and, indeed, seemed equally well, whether their food was colocynth, strychnia, morphia, or cheese. As "poison-mites" relished cheese, Mr. Attfield thought cheese-mites might relish poison. He therefore took some from cheese and put them on powdered strychnia, but the experiment was fatal to them; they all died. Others, however, thrived on cheese, adulterated with twenty per cent. of strychnia. Mr. Attfield infers that *acari* digest strychnia, which becomes oxydised in their blood, and its chief elements removed in the respiratory process. The

fact of an animal becoming habituated to a poison is not new. Men eat arsenic, opium, and tobacco, until their daily dose is sufficient to kill from two to ten of their species. Sheep have been known to eat poisonous plants until their mutton produced serious effects on those who ate it. Hedgehogs will eat anything, and toads are indifferent to prussic acid. Drawings of three acari discovered were exhibited, and Mr. Attfield said that Mr. Busk had decided that those found on the extracts of colocynth and taraxacum belonged to the same genus, but were of different species, and that those existing on nux vomica were generically different to the others.

Mr. Deane noticed that the acari figured greatly resembled some found a few years ago under very peculiar circumstances. At a village near Colchester, the name of which we did not catch, a church was rebuilt, the floor being lowered to within a few inches of some coffins that had lain underground for two or three centuries. Soon after the new church was opened the pews were found to be infested with acari, so numerous in some places as nearly to hide the fittings. It turned out to be a new species of acarus, and received the name of the *Acarus ecclesiasticus*. The parishioners were greatly alarmed at the visitation, and regarded it as a judgment of the Almighty for desecrating the graves of their forefathers. Mr. Deane added that the animal was very like that made by galvanism, some years ago, by the late Mr. Cross.[1]

The Chairman, in a neat speech, expressed the thanks of the meeting to Mr. Attfield for his liberal contribution of papers.

The time for adjournment having arrived,

Professor Bentley's paper "*On Podophyllin*" was taken as read.

CHEMICAL SOCIETY.

Thursday, February 20, 1862.

Dr. A. W. Hofmann, *F.R.S., President, in the Chair.*

Dr. Matthieson read a note on Professor Bolley's paper "*On Alloys of Lead and Tin.*" He first referred to a table of the specific gravities of various alloys of lead and tin, drawn up by Professor Bolley, which had been published in the *Journal of the Chemical Society*. He considered that a mistake had been made in the method adopted of calculating the theoretical specific gravities for the purpose of ascertaining which alloy underwent the greatest expansion during its formation; the theoretical specific gravity having been deduced from the comparative weights of the metals contained in each alloy, instead of from the volumes, which accounted for the discrepancies between the calculated and the real specific gravities. In a paper that had been published in the *Philosophical Magazine*, Dr. Matthieson had pointed out that the gravity ought to be calculated from the volumes of the constituent metals. As an example, he took the case of an alloy of equal parts by weighing of platinum and aluminium, which according to Professor Bolley's method of calculation, would have a specific gravity 11·97; the real gravity being 4·46, which would also be the result arrived at by calculating from the volumes employed; for, taking the specific gravity of aluminium at 2·5, and that of platinum at 21·45, one gramme of aluminium would displace 0·4 grammes of water; one gramme of platinum would displace 0·047 grammes of water. Therefore, by dividing the weight of metal in the alloy by the weight of water displaced, the specific gravity would be obtained; but $2 \div 0{\cdot}447 = 4{\cdot}46$ = the specific gravity, found by experiment. It further appeared that the alloy in which the greatest expansion took place, was that containing the two equivalents of lead and one of tin, and not that containing single equivalents of the metals, as stated by Professor Bolley.

[1] Or, rather (according to Dr. Warwick), obtained by Mr. Cross from old glove-boxes.—Ed. Chem. News.

The next Paper was by Mr. G. Gore, "*On the Quantitative Determination of Alkalies in Fire Clays and other insoluble Silicates.*" The ordinary process of decomposing fire-clays, &c., for the determination of alkalies by means of hydrofluoric acid being very tedious, and the method of heating with nitrate of baryta, not altogether satisfactory on account of the difficulty of ensuring a perfect decomposition, the mixture being only agglutinated by heat and not perfectly fused, a rather different process was proposed. An intimate mixture of the finely powdered fire-clay, nitrate of baryta, and fluoride of barium, was to be projected into a heated crucible; when all the mixture had been thrown in, the crucible was to be covered and gradually heated in a furnace until the contents were completely melted; the mixture was then to be poured into a cast iron vessel and immediately covered over. The resulting fused mass finely powdered, digested with sulphuric acid, evaporated to dryness, and afterwards treated in the usual manner. The results of the analyses made in this manner agreed with those obtained by the hydrofluoric acid process.

The next Paper was by Professor Kolbe, "*On the Constitution and Artificial Formation of Taurin.*" From a consideration of the relation existing between propionic acid, alanine, and lactic acid on the one hand, and ethyl sulphuric acid, taurin, and isethionic acid on the other, the formulæ of these substances being as follows—

Propionic acid . $HO, C_4H_5[C_2O_2]O.$

Alanine . . $HO, \left[\begin{matrix}C_4H_4\\H_2N\end{matrix}\right][C_2O_2]O.$

Lactic acid . . $HO, \left[\begin{matrix}C_4H_4\\HO_2\end{matrix}\right][C_2O_2]O.$

Ethyl sulphuric acid $HO, C_4H_5[S_2O_4]O.$

Taurin . . . $HO, \left[\begin{matrix}C_4H_4\\H_2N\end{matrix}\right][S_2O_4]O.$

Isethionic acid . $HO, \left[\begin{matrix}C_4H_4\\HO_2\end{matrix}\right][S_2O_4]O.$

it was inferred that taurin was similarly constituted to alanine. It had been pointed out some years ago by Strecker, that taurin might be obtained from isethionate of ammonia, and when treated with nitrous acid it again furnished isethionic acid. The action of caustic potash on taurin proved that it was not an amidogen compound, for no ammonia was formed on boiling with this reagent. It might be objected to this view, that taurin does not form saline compounds with alkalies or acids, but against this it was to be remembered that amidogen introduced into a compound weakened its acid properties, and that although definite compounds could not be prepared, still taurin showed a tendency to combine with bases, as would appear from the following properties of taurin prepared from ox bile. Taurin may be dissolved in acids and crystallized from thee solutions unaltered, it does not combine with salts nor does the hydrochloric acid solution furnish a platinum salt. It shows some tendency, however, to unite with bases, for a saturated aqueous solution is not precipitated by alcoholic ammonia, although alcohol alone precipitates it. Caustic potash has a similar power of preventing the precipitation by alcohol, but carbonic acid passed into the solution causes a precipitate; a compound with oxide of lead may also be formed, which is decomposed by carbonic acid. From these properties it would appear that, although incapable of forming definite saline compounds, taurin possesses feeble acid properties. It seemed probable that if isethionic acid had a constitution similar to lactic acid, it would, when acted on by pentachloride of phosphorus, yield chloride of chlorethyl-sulphuric acid; on trial this proved to be the case, the reaction being analogous to that with lactic acid, the body produced having the composition $C_4\left[\begin{matrix}H_4\\Cl\end{matrix}\right](S_2O_4)Cl$. This substance on being heated with water, was decomposed, hydrochloric and chlorethyl-sulphuric acids being formed; some diffi-

culty, however, was experienced in the purification of the chloride on account of the simultaneous formation of a body having nearly the same boiling point, which appeared to be the chloride of isethionic acid; a solution of chlorethyl-sulphuric acid might be boiled without decomposition, but on heating its salts with alkali decomposition took place; the chlorine in this substance might be replaced by hydrogen by means of an amalgam of sodium. Taurin might be obtained either from the chloride or from the silver salt of this acid, by the action of ammonia, but in the latter case it was contaminated with a substance which gave off ammonia when boiled with potash. The properties of the taurin obtained in this manner agreed with those of the taurin from ox bile. Professor Kolbe was of opinion that many similarly constituted organic compounds might be prepared artificially.

Dr. Odling did not agree with this view of the constitution of taurin, for the relation between isethionic acid, taurin, and chlorethyl-sulphuric acid was the same as that existing between hydrochloric acid, water, and ammonia; and there was no ground for concluding that, because a similar series was found to exist both in the isethionic acid and the propionic acid group, these bodies were similarly constituted, for in almost every group the same series of chlorides, hydrates, and amide existed; it seemed, however, that a difference between isethionic and sulphovinic acid had been established, for the isethionic acid formed a chloride, which passed through two successive steps of decomposition, tending to show that it was a bibasic acid.

A Paper, by Mr. W. Valentine, "*On a New Source of Acetone*," was then read by the Secretary. An examination of the inflammable liquid formed during the preparation of aniline, proved that it consisted chiefly of acetone, together with benzole and its homologues.

Mr. Perkin remarked that he had found very small quantities of acetone in the aniline residues.

Mr. Church said that two years ago he had seen some acetone in Mr. Williams's laboratory, which had been obtained from the same source.

The next Paper was by Dr. Phipson "*On Some Transformations of Citric, Butyric, and Valeric Acids.*" From a consideration of the change that malic acid undergoes when fermented, yielding first succinic and afterwards butyric acid, he had concluded that citric acid might undergo a similar transformation. To ascertain this, a mixture of citrate of lime and casein was allowed to ferment; but under these circumstances only butyric acid was formed. It then occurred to him that a different ferment might answer better, so a mixture of citrate of soda, carbonate of soda, and putrid beef was prepared; in which after some time a volatile and odorous substance was formed together with butyric acid, which appeared to be succinic acid; under certain circumstances, the butyric acid was formed without an evolution of hydrogen. Boiled beef and citrate of lime also gave traces of succinic acid. He next endeavoured to obtain succinic acid by oxydation, permanganate of potassa being employed as an oxydizing agent; with citric acid only oxalic acid was obtained, but butyric acid yielded succinic acid, acetic acid being formed at the same time, and consequently butyro-acetic acid. M. Dessaignes had already shown that succinic acid might be obtained from butyric acid by the action of nitric acid; valerianic acid also furnished acetic and succinic acids when acted on by permanganate, and suberic acid was perhaps produced at the same time. The method of conducting the experiment was as follows:—Permanganate of potash and nitric acid were added to the acid to be oxydized, and the whole heated until a reaction set in; after this had subsided, the solution was boiled, a long tube being fixed to the neck of the flask containing the mixture, to condense the vapour arising; if at the end of the operation any permanganate was left, this was decomposed by means of a little zinc before testing for succinic acid; the oxydation took place more readily if the ethyl compounds of the acids were employed.

Professor F. Field then read a Paper, "*On the Double Sulphides of Iron and Copper.*" He had previously made a communication to the Society on this subject, in which he had alluded to a method of treating copper ores which was adopted in countries where fire-clay was expensive, the ordinary method of roasting the copper pyrites being very hurtful to the furnace, on account of the oxide of iron forming a slag with the clay of the furnace. The method consisted simply in adding carbonate of copper to the regulus, by which the oxydation of the ore was hastened; this plan, however, was very rarely adopted. From the analyses that he had made of the double sulphide of iron and copper, he had come to the conclusion that this ore always contained the copper in the state of disulphide, and that on roasting, all the iron was oxydised before the disulphide of copper was touched. It had been observed that as the percentage of copper increased, the ore became less easily acted on by acid, and therefore that the iron had a different degree of sulphuration in the latter case, but he considered that this fact alone did not establish this difference, for that the smaller per-centage of the iron was sufficient to account for the difficulty with which it was acted on,—a similar effect being produced when a small quantity of silver is alloyed with a large quantity of gold, the gold protecting the silver from the action of nitric acid, which, under ordinary circumstances, attacks it readily. From his experiments, it appeared that the successive stages of decomposition were as follows, the original regulus having the following composition:—$3(Cu_2S)$, Fe_2S_3, FeS, $2Fe_2S$; after roasting for some time the only change was an increase in the amount of sulphide of copper, the composition being represented by the formula $6(Cu_2S)$ Fe_2S_3, FeS, $2Fe_2S$; on longer roasting a still further increase in the amount of sulphide of copper took place, the compound containing twelve equivalents of sulphide of copper, the remaining iron being combined with sulphur as before. With the blue sulphide similar results were obtained, and a table had been draw up exhibiting the various stages of decomposition.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, February 4, 1862.

J. P. Joule, *L.L.D., F.R.S., President, in the Chair.*

Mr. Binney said that soon after the death of Professor Eaton Hodgkinson, F.R.S., one of the former Presidents of the Society, and a man of science, of whom not only this Society, but the city of Manchester, has good reason to be proud, a few of his friends and admirers gave a commission to Mr. Slater, the sculptor of London, to execute a bust of the deceased, to be presented to this Society. As his valuable memoirs which gave to the world the formulæ for solid and hollow pillars of cast iron, now the basis of calculation on all structures of that metal, were printed in our *Transactions*, it was thought that the Hall of the Literary and Philosophical Society of Manchester was the fittest place for the bust of their discoverer. The following gentlemen, viz., our worthy President, J. P. Joule, LL.D., F.R.S., E. Schunk, F.R.S., James Heywood, F.R.S., John Hawkshaw, C.E., F.R.S., Joseph Whitworth, F.R.S., R. P. Greg, F.G.S., Thomas Turner, F.R.C.S., G. R. Stephenson, C.E, Robert Rawson, and E. W. Binney, F.R.S., present the bust to the Society.

On the motion of Mr. Spence, seconded by Dr. R. Angus Smith, it was resolved unanimously:—"That the thanks of the Society be given to the donors of the bust of the late Professor Eaton Hodgkinson, F.R.S."

Mr. Spence brought under the notice of the Society a ball of the dried leaves and stems of a plant imported from the West Coast of Africa, in the Kingdom of Dahomey. This plant, which grows spontaneously in great abundance, is used by the natives in dyeing cloth, to which it is said to give a good, but not very permanent, blue colour.

The parties who have imported these leaves are Messrs. Burnet and Thwaites, of Manchester, and they state that two years ago the sample had been examined without any result. About two months ago, Mr. Spence gave Dr. E. Schunck, F.R.S., a portion of them, he kindly undertaking to see whether they contained indigo. Circumstances prevented Dr. Schunck, until very lately, from entering upon the investigation, and the importers being anxious as to the matter, Mr. Spence, assisted by Mr. Bottomley, undertook the investigation, and at once found that the plant contained indigo perfectly formed, and which was easily extracted by the usual modes of deoxidisation and solution, the indigo being then precipitated pure and of a beautiful deep coppery shade. The only question now was as to its containing indigo in quantity sufficient to render the importation profitable. The solution of this question being partly one of chemical manufacture, has been undertaken by Mr. R. Rumney, who is to operate on a large quantity so as to get results of commercial value. Mr. Spence was chiefly induced to bring this subject before the Society from the fact that a new source of indigo at the present time would be a matter of great importance to trade, the growth of the article in India being from peculiar causes rather on the decline.

Dr. Schunck corroborated Mr. Spence's statements so far as that he had found indigo fully formed existing in the specimens submitted to him: he had not had time as yet to ascertain what quantity of that body they contained.

Mr. Mosley stated that it had been known for some time that the indigo plant grows wild in many parts of the West Coast of Africa; he believed it would be of great importance at present if a new source of indigo could be found, as the Indian manufacture seemed to be declining. He believed the manufacture had been attempted in Africa, but had not succeeded.

A paper was read "*On the Causes of Sickness and Mortality in the Manufacturing Towns of the North-West of England*," by Dr. C. J. Shearman, of Sheffield, communicated by Dr. R. Angus Smith, F.R.S. Previously to any examination of the disturbing causes of vitality the proportion of age to the whole population should be ascertained for town and country districts; for under all circumstances favourable or otherwise, age of an individual determines to a great extent the kind of sickness and the consequent mortality. In the towns of this part of England there are up to 15 years of age *less*, from 15 to 55 *more*, from 55 upwards *less* than in the adjacent country. Hence one of two evils results—either that towns kill young people and old fast—or middle age is imported. As to the feeding of town population, an average of 6 per cent. is imported from purely country districts of those under 20 years of age, and of 26 per cent. over 20 years. This constitution of replenishment of towns, associated with what is subsequently shown, *large mortality at early ages*, leads to the view that considerable drafts from the country from fifteen years upwards are made by towns, and which latter are responsible for their health. With the exception of infantile diseases, those of 15 to 55 years of age are the most fatal, and towns present a large field for the operation of disease at that age. The diseases of infancy and childhood are in a great degree infectious. Hence *density* of population would be considered a great element of mischief at those periods; but smallpox, scarlet fever, and some others, more infectious than others, present a less increase of mortality than measles over that of the country. The same law will apply to fever, influenza, and others at a later age—density and infection do not obey the same law of proportionate increase: neither will Dr. Farr's formula, for mortality increasing according to a certain root of the density, apply to provincial towns. Imprudent and early marriage, and inebriety have a certain value in the production of town mortality, but only to a slight extent, especially as to the former; the latter evil is not productive of so great an increase in the direct disease consequent upon it as materially to elevate town mortality over that of the country. Atmospheric causes, those from malarious mischief, in this Paper are not entered upon to any extent: they have a variable value, which is balanced to a great extent by the higher rate of income and consequent improvement of sustenance. The centralising system of this century is in itself not, therefore, it would appear, a great bane to the country; and yet towns kill considerably greater numbers of people than the country. There is an element of destruction which sanitary movements have overshadowed—viz., "The special influence of occupation." In this abstract, to simplify as much as possible, the town population is divided into groups: one-fifth consists of certain occupations not peculiar to towns, but common to the present state of society, viz., *out-door heavy occupations*. This class is amenable to the worst influence of town life, and without the advantage of high wages and consequent choice of abode and addenda to the necessities of life—its mortality in towns is yet only 17·5 in the 1000, the lowest mortality of the kingdom being 15 in the 1000. The other four-fifths consists mainly of the skilled operative and commercial and professional classes. The mortality of this section is caused by the skilled workers, and mostly so when constrained position is requisite. One main object of the paper is to draw attention to this special element of town mortality, the alteration in the normal proportion of respiration to circulation of the blood appearing to be the turning point from health to disease. This portion of the subject cannot be entered into without considerable detail, given in the Paper itself. One point appears to be prominent, viz., that in the endeavour to ventilate, cleanse, drain, and distribute our town population, the necessary employment to earn a *livelihood* in towns has carried with it the seeds of disease and death, and escaped to a great extent the notice of those able and philanthropic people who have devoted their energies to the best mode of increasing the health and prosperity of their fellow-labourers.

MICROSCOPICAL SECTION.

January 21, 1862.

Professor Williamson, *President of the Section, in the Chair.*

Mr. William K. Deane was elected a Member of the Section.

A letter was read from Mr. H. A. Hurst, late of Calcutta, making a donation to the Section of his entire collection of mounted microscopical objects, consisting of upwards of 400 specimens; they comprise 90 slides of diatomaceæ; 100 of algæ, mostly marine; about 30 different kinds of starches, and the remainder an assortment of Desmidiæ, preparations of insects, bone sections, and a variety of other objects. Mr. Hurst also presented a collection of specimens of Asiatic woods, in small blocks, obtained from the Horti Agricultural Society of India at Calcutta. They consist of 98 specimens from Arracan; 76 from Upper Assam; 6 from Central India; 8 from the coast of Tenasserin; and 13 from Chittagong; 201 specimens of woods in all, most of them with the native names.

Captain Penrice, of the ship *Pegasus*, from Shanghae, forwarded to the Section a number of soundings taken during his last voyage, amongst which may be noted one each from the mouth of the Yang-tse-Kiang river in China, coast of Borneo, Java; and a portion of mud, rich in foraminifera, from the anchor fluke, at Gaspar Island.

The Secretary laid on the table sixty specimens of soundings, which had been freed from the tallow "arming" in two evenings after business hours, at Mr. Dale's laboratory, by Mr. Dale, Mr. Dancer, and himself, assisted by Mr. Richard Dale. The system adopted is that described in Mr. Mosley's Paper, read to this Section on January 21, 1861, published in the *Quarterly Journal of Microscopical Science*, vol. i., new series, p. 143,[1] and is found to answer better than any other method yet made known.

Professor Williamson presented fourteen specimens of dredgings, supposed to be from the mouths of the Ganges.

A communication from Mr. A. G. Latham was read, upon the subject named for the evening's discussion, "*On the cause of the metallic lustre on the wings of the Lepidoptera, both diurnal and nocturnal.*" Mr. Latham believes that the metallic lustre may be simply referred to the presence of a pigment in the substance of the wing, in some cases light-absorbing, and in others light-reflecting; all the scales seemed equally adapted for reflecting the prismatic colours, consisting of three distinct membranous films, covered with minute irregularities. Mr. Latham sent to be exhibited a number of slides for illustration.

A communication was read from Mr. Dancer, in which he referred to a paper, read by Sir D. Brewster at the last meeting of the British Association, containing the following remarks by Professor Dove:—"In every case where a surface appeared lustrous there was always a transparent, or transparent reflecting stratum of much intensity, through which we see another body; it is, therefore, externally reflected light in combination with internally reflected or dispersed light, whose combined action produced the idea of lustre. . . . . This effect we see produced when many watch glasses are placed in a heap, or when a plate of transparent mica or talc, when heated red hot, is separated into multitudes of thin layers, each of which, of inconceivable thinness, is found to be highly transparent, while the entire plate assumes the lustre of a plate of silver."

Mr. Dancer sent for exhibition several pieces of talc, which in places, by the action of the blowpipe, had been heated to redness; the films were thereby separated, and the raised or blistered portion gave a metallic lustre like silver.

Mr. W. C. Unwin believed that the metallic lustre was due not to pigment but to the reflection of light from internal surfaces of the scales through the transparent outer layer. The light so reflected appeared to be modified in two ways—by the ribs or striæ; it was dispersed by them so that the scales were lustrous at various angles and it was also in some cases coloured by interference caused by them. Iridescence appeared to be also produced in some scales by the thinness of the laminæ through which the light was refracted causing interference. Mr. Unwin exhibited a number of specimens to illustrate his arguments.

Mr. Dale referred to beautifully-coloured films which arise upon various chemical solutions, the metallic brilliancy of which may arise from similar causes.

Mr. Sidebotham observed that the metallic appearance was not due to any colouring matter in the scales, as chemical agents, which destroy the coloured scales, have no effect whatever on the metallic ones; he also mentioned a curious polarizing effect produced by crossing the metallic scales of Plusia bractea. Mr. Sidebotham exhibited the metallic scales from Plusia orichalcea, Plusia bractea, Plusia festucæ, Plusia concha, &c., illustrative of his remarks.

It was ultimately resolved that the discussion should be adjourned, so as to enable the proposer of the subject, and other gentlemen not present, to express their views.

Mr. Sidebotham also exhibited a new finder for high powers. It consists of squares formed by crossing lines, one hundredth of an inch apart, enclosing progressive numbers, executed by photography. He promised to prepare a number for the use of the members, all to be exactly alike.

Mr. Joy exhibited a nose-piece, made for him nearly two years ago, consisting of a diaphragm plate, under which are screwed four objectives of different powers; the centreing is so true, that he can use the three-inch as a finder for the ¼ or even ⅛-inch power. Several members of the Section have nose-pieces made upon the same principle.

Mr. Linton and Mr. Watson exhibited the tuft scales on the upper wing of the Perophca Cristana and others.

[1] See Chemical News, vol. iii., p. 95.

NOTICES OF BOOKS.

Lehrbuch der Organischen Chemie oder der Chemie der Kohlenstoffverbindungen. Von Dr. Aug. Kekulé. Dritte Lieferung. Erlangen: F. Enke. 1861.

Dr. Kekulé's Text-book of Organic Chemistry was, we believe, to have been completed in four parts; but we are not sorry to find that the original plan has been enlarged, and that the third part lately published completes the first volume of the work, instead of commencing the second. In consequence, each volume will contain three instead of two parts.

We have already noticed and illustrated at some length the chief features of Dr. Kekulé's plan in reviewing the first and second instalments of his work. The third instalment is of equal excellence with the preceding parts, and merits the same praise. Its opening pages are occupied with an account of the compounds formed by the union of the alcohol radicals with certain members of the nitrogen or triatomic group, namely, antimony and bismuth; then the bodies containing alcohol radicals in union with monatomic, biatomic, and tetratomic metals are described. We next find the necessary information concerning the monhydric oxygenated radicals of the fatty acids and their derivatives; and, lastly, the first volume concludes with an intelligible and concise history of the biatomic radicals of the formulæ $C_n H_{2n}$ and $C_n H_{2n-2} O$, and of their derivatives.

It is impossible by brief extracts to give an adequate idea of the singular merits of Dr. Kekulé's work: we have made considerable quotations from its earlier portions in former notices, and had this not been so, we should have transferred to this Journal a few passages from its more recent pages. As it is, we will only refer our readers to the history of the aldehydes and acetones of the fatty acids as narrated in pp. 606—618.

NOTICES OF PATENTS.

1114. *Gutta-Percha.* P. A. Godefroy, New North Road, Islington. Dated May 3, 1861. (Not proceeded with.)

This proposition relates to the manufacture of a compound of gutta-percha and resin, which is said to be more durable and better fitted for insulating telegraph wires than gutta-percha alone, on account of it being a more perfect non-conductor of electricity. For the preparation of this material, the Inventor adds 5 to 10 per cent. of resin, softened by heat, to the mass of gutta-percha. After it has undergone the masticating process, the mixture is then further kneaded until intimate combination is effected.

1115. *Collecting Ammonia from the Waste Gases arising from the Combustion of Coal.* J. A. Manning, Inner Temple, London. Dated May 3, 1861.

This invention relates to a mode of collecting the ammonia from what have hitherto been deemed waste gases arising from the combustion of coal in the manufacture of coke,

and from the products given off on burning fuel in furnaces, or in other situations wherein coal is largely consumed. It consists in introducing jets of steam into the chimney-stack or main flues leading thereto, which, mixing with the gases as they escape from the furnace, becomes impregnated with the ammonia and effectually carries down the latter when subsequently condensed by passing through suitable refrigerators. A crude ammoniacal liquor is thus obtained, from which the sulphate or other required salt of this base may be prepared.

1117. *Treatment of Copper Ore.* W. E. Newton, Chancery Lane, London. A Communication. Dated May 3, 1861. (Not proceeded with.)

For the treatment of ordinary copper pyrites, whether previously roasted or not, the pulverized ore is to be mixed with sulphur and chloride of lime, employed in proportions which will have to be determined by experiments in each case, and sometimes with the addition of the fluxes generally used in copper smelting. These mixed ingredients are subjected to a high temperature in a furnace with limited draught of air, when a fused regulus of disulphide of copper will be formed, the gangue and metallic impurities of the ore being transferred to the slag. The grey sulphide resulting from this process may be treated by any of the ordinary methods for the extraction of its metal.

Considering that under ordinary circumstances the whole of the sulphur contained in copper pyrites and many other ores of copper is lost to the smelter, the advantage of heaping yet more brimstone on the fire would be deemed a step in the wrong direction.

1131. *Enamel.* J. V. Vignon, Brussels. Dated May 6, 1861. (Not proceeded with.)

The inventor describes a combination of materials which on being fused from an enamel white in itself, but capable on being tinted or coloured by association with any suitable pigment. The ingredients employed are the following:—White of egg was dissolved in spirit, plaster of Paris, and chloride of calcium (prepared by acting upon burnt shells with hydrochloric acid.) These are intimately incorporated and applied in the liquid state to the surface of the object about to be ornamented with enamel, using a brush as a convenient means of laying on the composition. Upon this white ground the ordinary coloured enamels may be afterwards applied for the production of artistic effects.

In the absence of facts it would be difficult to assert that the sulphate of lime enters into chemical union with chloride of calcium by fusion, but in the event of such a compound being formed its properties are likely to be consideraby influenced by the nature of the basis upon which it is applied. Under no modification would the ingredients here specified hold out the promise of very satisfactory results.

Grants of Provisional Protection for Six Months.

158. Alfred Joseph Martin, High Street, Bow, Middlesex, "Improvements in the treatment of fusel oil, and for various applications of the same to useful purposes."

163. Louis Martin, Rue Gaillon, Paris, "Improvements in the treatment of mineral oils, and in the apparatus connected therewith."

209. William Orr, Greenock, Renfrewshire, N.B., "Improvements in machinery or apparatus for the manufacture of sugar."

220. Arthur Herbert Church, Great Portland Street, St. Marylebone, London, "Improvements in the means of preserving stone, brick, slate, wood, cement, stucco, plaster, whitewash, and colour wash from the injurious action of atmospheric and other influences, also in the application of colours to the surfaces of stone, brick, slate, wood, cement, stucco, mortar, clay, plaster of Paris, plaster, whitewash, and colour-wash, and in the retention of such colours thereon."

Inventions Protected for Six Months by the Deposit of Complete Specifications.

298. William Edward Newton, Chancery Lane, London, "Improvements in the manufacture of iron and steel."—Communication from John Gardner and Henry Richard Gardner, U.S.—Deposited and recorded February 4, 1862.

Notices to Proceed.

2456. William Maltby, De Crespigny Park, Camberwell, Surrey, "Improvements in the manufacture of starch and starch gum."

2477. Charles Husson, Nantes, France, "An improved process for silvering looking glass in piles of several sheets superposed without interruption, which process consists in the use of a chemical powder."

2489. Ebenezer Partridge, Patent Axle Works, Smethwick, Staffordshire, "Improvements in hardening iron and steel, and a composition or substance to be employed therein."

2855. William Henry Balmain and John Kean, St. Helen's, Lancashire, "Improvements in the manufacture of flowers of sulphur, and roll, and other forms of sulphur."—Petition recorded November 13, 1861.

CORRESPONDENCE.

On the Manufacture of Iodine.

To the Editor of the Chemical News.

Sir,—I notice in the Chemical News, No. 116, page 111, a method of separating iodine by Dr. Luchs. Perhaps your readers, generally, are not aware that in 1853 I made the discovery of a method of precipitating iodine in a crystalline form by the use of bichromate of potash and sulphuric acid, and in 1855 applied for a patent for the process, having first satisfied myself of its novelty by communication with Professors Brande, Graham, Hofmann, Miller, Kane, Wilson, Redwood, Muspratt, and other eminent chemists throughout the kingdom, who appeared to be unaware of such a method. I subsequently, through the medium of an advertisement in the *Times*, addressed iodine manufacturers, two of whom sent me a supply of kelp for the purpose of ascertaining by a series of large experiments the profits of working the process, on the completion of which I did not proceed further in the patent, having learnt from Dr. Penny that he had anticipated me in the discovery a few months before. I sent in a detailed account of my process to the Glasgow meeting of the British Association in 1854, suggesting the recovery of the sulphuric acid and the conversion of the chromic salt into pigments for painting and staining pottery ware, which was published in the *Chemist* and at the Cheltenham Meeting of the British Association in 1856, I exhibited to the members of the Chemical Section a bottle containing about two pounds of iodine, manufactured by me, and have the same in my possession at this moment, so that the process is *not new*, indeed, since my original application of it some other foreign chemist has proposed the same, and managed to get it introduced into Dr. Muspratt's work. If it be of any interest to you or your readers, I could furnish you with a leaf from *my note book* setting forth all particulars of the substances I extracted from a ton of kelp with the cost, &c. I am, &c.

John Horsley, F.C.S.

County-Analyst's Laboratory, Cheltenham.

Chemical Notices from Foreign Sources.

I. ORGANIC CHEMISTRY.

Prussic Acid.—M. Millon gives a method (*Comptes Rendus*, t. liii. p. 842) by which, he says, several quarts of

anhydrous prussic acid can be obtained with as little trouble as absolute alcohol. He first submits the dilute acid to fractional distillation, collecting what comes over between 50° and 100° C. After two or three distillations he passes the vapour through two Woolf's bottles containing dry chloride of calcium, and condenses in a receiver placed in a freezing mixture—on this occasion stopping the distillation between 70° and 80°. The anhydrous acid M. Millon found to form a crystalline compound hydrochloric acid gas, and also with bichloride of tin, the latter compound being soluble in an excess of the prussic acid. The anhydrous acid forms other compounds, which are only stable as long as water is excluded. Moisture destroys them, and formiate of ammonia is produced. M. Millon observed that ammonia had a strong influence on the production of paracyanide compounds. A bubble or two of ammoniacal gas produced in two or three days the complete solidification of 200 grammes of the anhydrous acid. Dilution with five or six volumes of water only delayed the same result a few days. Ammonia is the sole cause of the production of these compounds. Acids or acidifiable matter preserves prussic acid either by neutralizing ammonia as soon as it is formed, or by preventing its formation.

Benzulmic Acid.—Benzulmic acid is the name which MM. Schutzenberger and Sengenwald (*Comptes Rendus*, t. liii. p. 974) give to the brown compound which is formed when oxy-benzoic acid is formed by passing a current of nitrous acid through a solution of benzamic acid. It contains no nitrogen and is a bibasic acid. Its formation from oxy-benzoic acid is thus explained :—

$$2(C_{14}H_6O_6) + 2NO_3 = 2HO + C_{28}H_{10}O_{12} + 2NO_2$$

It forms soluble brown salts with alkalies, which give brown precipitates with metallic salts. It is insoluble in water, ether, alcohol, and generally all neutral solvents.

II. MINERAL CHEMISTRY.

Atomic Weights of Chromium, Arsenic and Antimony.—Kesster has continued his experiments to determine the atomic weights of the above bodies. (*Poggend. Annalen*, Bd. cxiii. s. 134.) The equivalent of chromium he gives as 26·15: that of arsenic at 75·15. As regards antimony he has not got to a certain conclusion. The results with this metal varied between 122·16 and 122·37.

III. ANALYTICAL CHEMISTRY.

Metallic Copper as a Test for Sulphurous Acid.—Reinsch states (*Neues Jahrt. für prakt. Ph.*, Bd. xvi. s. 277) that if a bubble or two of sulphurous acid gas be passed into half-an-ounce of strong hydrochloric acid, and then two drops of this acid mixed with 20 cubic centimètres of water and 10 cubic centimètres of strong and pure hydrochloric acid, a small piece of bright copper wire placed in the mixture and boiled, the wire is coloured distinctly brown, and in a short time has the same appearance as in the author's arsenic test. If a larger quantity of sulphurous acid is present, the wire becomes a deep brown black. Air containing SO_2 passed through a bulb containing hydrochloric acid and a piece of copper wire acts sensibly on the wire. This test, Reinsch says, will detect one-millionth part of sulphurous acid.

MISCELLANEOUS.

Chemical Society.—The next meeting of this Society will take place on Thursday, the 6th inst.

Fraunhofer's Lines.—Professor Zantedeschi, of Padua, in opposing an assertion of Dumas, to the effect that Fraunhofer's spectrum has not, up to the present time, displayed any appreciable change, and that, consequently, the solar light itself has not undergone any alteration, says —"I conclude that this Parisian chemist ignores the existence of my *mémoire*, published at Menaco, in 1837, *De mutationibus quæ contingunt in spectro solare fixo*." He continues that, having recently made use of a camera with a constant aperture, a fixed position and distance, for a prism of flint glass of sixty degrees, and an immovable plane of projection in order to make sure that the solar spectrum is absolutely fixed, he is thoroughly convinced that in Fraunhofer's spectrum there are both fixed and variable lines. He remarks :—"The movability of the lines proved to me it was an indication of the influence of the medium through which the solar light passes, and of the variations to which bodies in a state of combustion are subject, and this led me naturally to suspect the modifications which arise not only in the solar photosphere, but also in the entire universe, and I at length concluded therefrom that the spectrum is a very perfect photoscope—that it is a faithful mirror which reflects the most delicate images of incandescent bodies, and the changes to which our planetary system is incessantly submitted."

Scientific Ethics.—Priority of Discovery.—Most scientific disputes arise from questions of *priority*. Naturalists are peculiarly liable to anticipation of their labours by others working in the same line of investigation. Hence, all agree that *publication* alone can entitle an investigator to priority. However hard the rule may be in a given case, it is the only safe one, and its force is universally acknowledged. But what is publication? The answer is equally definite, although not, perhaps, so generally accepted. It is the actual distribution of the results claimed in a printed form to the principal workers in the same department—whether by means of a scientific Journal, a scientific Society which publishes a Journal or Transactions, or in a separate memoir. The latter case alone requires any care on the part of the author to see that copies are placed in proper hands, since the public nature of the other means of publication leaves no excuse for ignorance. Merely *printing* a memoir, without a reasonably general distribution of it, is not publication, neither is an oral or written communication to some non-publishing Society publication, since the chief end of publication is not thus gained, the matter does not thus come to the knowledge of others who are engaged in kindred pursuits. Publication, then, is not printing alone, but it is *distributing* a printed memoir or communication. It is easy in any case of importance to make the *date* of publication definite by printing it upon the cover, taking care that the *date* and the actual issue correspond as nearly as possible. — *Messrs. Silliman and Dana, in the American Journal of Science.*

ANSWERS TO CORRESPONDENTS.

Vol. IV. of the CHEMICAL NEWS, containing a copious Index, is now ready, price 12s., by post, 12s. 8d., handsomely bound in cloth, gold-lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. 6d. if sent to our Office, or, if accompanied by a cloth case, for 6d. A few copies of Vols. I. II. and III. can still be had. Vol. V. commenced on January 4, 1862, and will be complete in 26 numbers.

D. Knight.—The work has not been translated into English.

Gases Evolved in Coal Mines.—A Correspondent asks if there is any work treating of this subject.

Manufacture of Ink.—We have received a letter of which the following is an extract :—"Being engaged in the production of a 'Manual on the Art of Practical Ink Making,' I have taken the liberty of asking if you or any of your readers can in any way assist me with any practical suggestions or formulæ in this department of Chemical Science, George Dutton, 462, Duke Street, Glasgow."

G. F. Ansell.—The subject shall be attended to. A letter addressed to the Publisher at our Office would have met with prompt attention.

J. Horsley.—You mistake. We do not desire to hinder the publication of useful observations, but our space is too valuable to be occupied in fruitless and uninteresting discussion on personal matters.

Books Received.—"On the Death of Several Horses from Feeding on Oats affected with Fungi," by Assistant-Professor Varnell, reprinted from the *Veterinarian*. "On the Injurious Effects resulting from the Employment of Arsenical Pigments," by E. W. Davy.

THE CHEMICAL NEWS.

VOL. V. No. 118.—March 8, 1862.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Reactions of Ethylamine and Diethylamine, by M. CAREY LEA, *Philadelphia.*

THE materials for the following examinations were prepared by the action of nitrate of ethyl upon ammonia in sealed tubes, in the manner which I have described in a previous number of this Journal. The bases were separated from each other by means of picric acid.

Ethylamine.—In order to ascertain the purity of the ethylamine used, and further to test the exactness of the separation by means of picric acid, another platinum determination was made with great care. It gave the following results :—

1·3911 grms substance gave, platinum, ·5457

This corresponds to, per cent. .	39·23
Theory requires . . .	39·29

A result which, taken in connection with analyses already published, seems conclusive.

Reactions of Ethylamine.

The reactions of ethylamine with metallic solutions have been more studied than those of the other ethyl bases; the following, however, do not seem to have been previously described :—

Gold, terchloride.—Reddish precipitate, easily soluble in excess of precipitant.

Ruthenium sesquichloride.—No precipitate, either immediate or by standing 48 hours. The action of ethylamine differs from that of ammonia not only in producing no precipitate, but also in this, that the liquid, after treatment by ammonia, acquires a lilac or lilac-brown colour; whereas after treatment with ethylamine it assumes a greenish-brown or olive shade.

Palladium, protochloride.—An immediate highly crystalline precipitate which re-dissolves in part, in excess of the precipitant, forming a colourless solution.

Uranium, nitrate.—Yellowish precipitate, insoluble in excess of precipitant.

Cerium, protochloride.—Perfectly pure protochloride of cerium, prepared according to Holtzman's modification of Hermann's method, gave a dirty precipitate, insoluble in excess of the precipitant.

Cerium, nitrate of protoperoxide.—Light brown precipitate, insoluble in excess of the precipitant.

Glucinum, sulphate of glucina.—White, insoluble in excess.

Zirconium, chloride of zirconia.—White, insoluble in excess.

Molybdenum, protochloride.—Reddish brown, insoluble in excess.

Molybdenum, bichloride.—Reddish brown, insoluble in excess.

While the analogies which unite ethylamine to ammonia are extremely well marked, the differences in their reactions are also very well defined. Like ammonia, ethylamine re-dissolves its precipitates from salts of copper, of zinc, and of silver, but it also re-dissolves precipitates from solutions of gold, ruthenium, and aluminium, which ammonia does not. It is, on the other hand, incapable of re-dissolving the precipitates from solutions of cobalt, nickel, and cadmium. Towards protosalts and salts of protoperoxyde of cerium, glucinum, zirconium, protochloride and bichloride of molybdenum, peroxide of uranium, bismuth, and antimony, its behaviour is similar to that of ammonia.

It was a matter of interest to observe whether the substance produced by the reaction of ethylamine on solution of terchloride of gold would exhibit any analogy with that resulting from treatment by ammonia, viz., fulminating gold. The precipitate caused by ethylamine readily dissolved, as above stated, in excess of the precipitant; this, at a gentle heat, dried up to a yellow mass, which, when heated, melted, turned red, emitted dense white fumes, and left a brown spot. No sudden decomposition took place.

The following is a well-marked distinctive reaction between ammonia and ethylamine. If bichloride of tin be treated with ammonia, a precipitate is obtained which scarcely shows any disposition to re-dissolve in even a large excess of the precipitant; but with ethylamine the precipitate re-dissolves easily. In the case of ammonia, the presence of sal-ammoniac renders the precipitate from a stannic solution, according to Rose, altogether insoluble, whereas in the case of ethylamine, a considerable proportion of chlorhydrate of ethylamine may be added without diminishing the solubility of the precipitate.

Ethylamine, like ammonia, has the property of reddening an alcoholic solution of dinitronaphthaline.

Diethylamine.—The action of nitrate of ethyl upon ammonia is particularly well suited for obtaining diethylamine, on account of the relatively large proportion obtained at once. Further experience, since I published that process, has shown me that the product is even larger than I supposed. The quantity of diethylamine produced is fully as great as that of ethylamine.

Diethylamine obtained by that process was purified by solution of its picrate in ether. A specimen of platinum salt analysed gave Pt 35·45 per cent.; theory requires 35·45.

Reactions of Diethylamine.

The reactions of diethylamine with metallic solutions have not hitherto been examined. The following were observed :—

Cerium, protochloride.—Dirty white precipitate, insoluble in excess of the precipitant.

Cerium, nitrate of protoperoxide.—Light brown precipitate, insoluble in excess of the precipitant.

Zirconium, chloride of zirconia.—White precipitate, insoluble in excess.

Gold, terchloride.—Brownish-red precipitate, easily soluble in excess of precipitant.

Ruthenium, sesquichloride.—No precipitate. The

colour of the solution presents the same characteristics as in the case of ethylamine.

Palladium, protochloride.—No precipitate from a somewhat dilute solution, but the deep red liquid is instantly decolorised.

Platinum, protochloride.—No precipitate from a moderately concentrated solution. Hydrochlorate of diethylamine dissolves in protochloride of platinum to a clear solution, so that if a diethylamine base analogous to Magnus' green base, exists, it must be very soluble.

Platinum, bichloride.—No precipitate unless both solutions are very concentrated.

Molybdenum, protochloride.—Red brown, insoluble in excess of precipitant.

Molybdenum, bichloride.—Red-brown, insoluble in excess of precipitant.

Copper, sulphate.—Blue precipitate, very sparingly soluble in excess of the precipitant.

Silver, nitrate.—Brown, easily soluble in excess of the precipitant.

Zinc, sulphate.—White, insoluble in excess of the precipitant.

Cadmium, sulphate.—Same reaction.

Nickel, sulphate.—Pale green, insoluble in excess of the precipitant.

Cobalt, protochloride.—Blue, insoluble in excess of the precipitant.

Aluminium, alum.—White, soluble in excess.

Chromium, chrome alum.—Bluish gray, insoluble in excess.

Lead, acetate.—A small quantity produces no precipitate; a large quantity a white precipitate, insoluble in excess.

Lead, nitrate.—An immediate precipitate, insoluble in excess.

Mercury, protochloride.—White, insoluble in excess.

Tin, protochloride.—Same reaction.

Tin, bichloride.—White, soluble in excess.

Glucina, sulphate.—White, insoluble in excess.

Manganese, protosulphate.—Pale brown, insoluble in excess.

Magnesia, sulphate.—White, insoluble in excess.

Iron, sesquioxide, ammonia alum.—Brick red, insoluble in excess.

Antimony, chloride.—Brick red, insoluble in excess.

Antimony, tartar emetic.—At the first moment no precipitate, then a cloudiness, and finally a heavy precipitate.

Bismuth, nitrate.—White, insoluble in excess of precipitant.

Uranium, nitrate.—Yellow, insoluble in excess.

Some of these reactions are highly interesting. It has been already shown, under the head of Ethylamine, that, in addition to the differences already known to exist between its reactions and those of ammonia, its behaviour towards solutions of gold and ruthenium is highly characteristic. We now see that diethylamine not only resembles ethylamine in these properties, but shares with it its remarkable capability of re-dissolving precipitates of alumina. Ethylamine and diethylamine, moreover, resemble each other and differ from ammonia in their reactions with cadmium, nickel, cobalt, and bichloride of tin. They both act like ammonia towards solutions of glucina, zirconia, protoxide and protoperoxide of cerium, peroxide of uranium, protoxide and deutoxide of molybdenum, and many other metals. The only oxides which all three are capable of re-dissolving are those of silver and copper. Silver dissolves abundantly in all three; copper much more sparingly in ethylamine than in ammonia; while in diethylamine this property almost disappears, a faint blue colour indicates the solution of a mere trace. Unless the aqueous solution of diethylamine is strong, not even a trace of copper is taken up by it.

The action of diethylamine on terchloride of gold was further examined to ascertain if the resulting compound had any properties corresponding with those of fulminating gold. The clear yellow solution obtained from solution of terchloride of gold by treatment with diethylamine, dried up to a somewhat crystalline, deliquescent mass, which, when heated, decomposed without the slightest explosion.

It is evident from the above that the relations which exist between ethylamine and diethylamine are much closer than those between ethylamine and ammonia. In fact, in all the above reactions, they differ in their behaviour to palladium and zinc salts only. Protochloride of palladium is precipitated by ethylamine and not by diethylamine; zinc precipitates are re-dissolved by ammonia and ethylamine, but not by diethylamine. In view of this remarkable analogy, all clearly distinctive reactions acquire an interest, and the following which I have observed is very well marked.

If protochloride of mercury be precipitated with a very large excess of ammonia, the precipitate readily dissolves on the addition of a little acetic acid, the liquid remaining very strongly alkaline. Ethylamine behaves in the same way, but the precipitate caused by diethylamine does not re-dissolve under the same circumstances. Acetic acid may be added, in fact, until the liquid acquires a decidedly acid reaction without causing a solution.

Diethylamine shares the property of ethylamine and ammonia of reddening alcoholic solution of dinitronaphthaline.

The analysis of the platinum salts of the ethyl bases requires great circumspection in the application of heat, as they decompose at a far lower temperature than the chloroplatinate of ammonium. The diethylamine salt blackens at a temperature at which the upper part of the porcelain crucible remains cool enough for it to be lifted by the fingers.

Action of Iodine on Ethylamine and Diethylamine.

When aqueous diethylamine is poured over iodine in powder, it becomes milky and there collects at the bottom of the vessel a black substance in thick oily drops. These, when heated in a platinum spoon, give off first violet vapours of iodine, then thick white clouds, and finally leave a carbonaceous residue. If this black substance be boiled a few minutes with a large excess of caustic soda, it emits an odour not unlike that produced by the combustion of phosphuretted hydrogen, diminishes in volume, and becomes thicker, so much so as to solidify on cooling. It then, when heated, gives off no violet vapours, but only thick white clouds, swells up, and leaves an enormously bulky residue of carbon. Ethylamine exhibits a nearly similar reaction. In neither case has the resulting substance the slightest explosive properties, as might be expected from the reaction of ammonia under similar circumstances.

The compound formed in the case of ethylamine has been examined by Wurtz and found to have the formula $C_4H_5I_2N$. By this result, the researches of Gladstone into the constitution of the so-called iodide of nitrogen, reliable in themselves, are supported. The latter chemist found for the explosive substance formed by the action of iodine on ammonia the constitution HI_2N. The action of iodine is, consequently, in both cases, exactly

analogous, two atoms of hydrogen are replaced by two of iodine.

Gilm, therefore, seems to be in error in asserting that no substitution-product from ethylamine analogous to so-called iodide of nitrogen (biniodamine) can be obtained.[1]

This iodine substitution-product from ethylamine and diethylamine cannot, unfortunately, be obtained in a state of purity sufficient for reliable analysis, otherwise the examination of the diethylamine product would be interesting; for in ethylamine there are two atoms of the hydrogen of the ammonia type remaining,

$$\left.\begin{matrix} C_4H_5 \\ H \\ H \end{matrix}\right\} N$$

and there is no difficulty in supposing them to undergo a substitution by iodine; but in diethylamine there remains but one,—

$$\left.\begin{matrix} C_4H_5 \\ C_4H_5 \\ H \end{matrix}\right\} N$$

Nevertheless, diethylamine, under the action of iodine, affords a substance exactly resembling that produced from ethylamine. To effect the analysis of a substance having an equivalent in the neighbourhood of 300 so as to speak with confidence of the presence or absence of a single equivalent of hydrogen, it would need to be obtainable in a state of perfect purity. This, however, has not, so far, been possible.

Isomorphism of Ammonias.

A few experiments made in this direction gave the following results:—

Ethylamine alum.—Octahedra of sulphate of ethylamine and alumina were obtained.

Tartrate of diethylamine and soda.—In order to determine whether diethylamine was capable of replacing ammonia in the magnificent crystalline forms of the double tartrates, this salt was formed, but whether crystallised from water or from weak alcohol, only needles could be obtained.

Sulphate of diethylamine and zinc.—In order to determine if diethylamine was capable of replacing ammonia in Mitscherlich's group of double sulphates, $RO,SO_3 + MO\,SO_3 + 6HO$, with the characteristic crystal form, the double sulphate of zinc and diethylamine was formed. A deliquescent solution was obtained, which, *in vacuo* over sulphuric acid, afforded a crystalline mass, from which no conclusion as to isomorphism could be drawn. —*American Journal of Science.*

On Perchromic Acid, and the Action of Peroxide of Hydrogen on the Higher Oxides, by M. H. ASCHOFF.

IT is known that when dilute solutions of bichromate of potash and peroxide of hydrogen, containing a certain amount of acid, are mixed together, a blue liquid is obtained which becomes decolorised more or less rapidly with disengagement of oxygen (Barreswil). When there is sufficient peroxide of hydrogen there finally remains only a chromic salt, the oxy-water and the chromic acid having both lost part of their oxygen.

M. Aschoff having endeavoured to ascertain the quantity of oxygen disengaged in this reaction, found that for one atom of bichromate of potash employed there were always collected more than six atoms of oxygen; but that this relation was not constant. M. Wöhler[1] found that one molecule of peroxide of manganese decomposed exactly one molecule of oxy-water. According to this fact, and also to the opinions of M. Schönbein,[2] it ought to be supposed that one atom of bichromate of potash should decompose three molecules of oxy-water, setting at liberty six atoms of oxygen. It will be seen that this is not the case.

M. Aschoff first satisfied himself that the decomposition of oxy-water by permanganic acid took place between one molecule of the acid and five molecules of peroxide of hydrogen. He commenced by estimating the peroxide of hydrogen in a dilute solution by means of a standard solution of ammoniacal sulphate of protoxide of iron, and a similarly standardised solution of permanganate of potash: the estimation was made very readily. On directly treating oxy-water with permanganate of potash, he found that the same volume of solution of permanganate is necessary to decompose the oxy-water, and to peroxidise the amount of ammoniacal sulphate of iron, which on its part can remove from the same volume of peroxide of hydrogen its second atom of oxygen. One molecule of permanganic acid therefore readily decomposes five molecules of oxy-water, and may thus be used advantageously for the estimation of peroxide of hydrogen. When the solution of oxy-water is very acid, the reduction of the permanganic acid takes place immediately, and the manganese passes to the state of a salt of the protoxide. When, on the contrary, there is only a small quantity of the free acid in the liquid, as, for instance, when the oxy-water is poured drop by drop into the neutral permanganate of potash, there separates at first a manganese compound which is probably the hydrated peroxide; but upon the addition of a larger quantity of oxy-water, this hydrate re-dissolves, and the final result is the same as before; that is to say, that ultimately for one atom of oxygen which the permanganic acid gives up, the oxy-water likewise disengages one atom of oxygen, or one molecule of permanganic acid decomposes five molecules of oxy-water, becoming reduced to the state of manganous acid.

Having at his disposal a means of standardizing solutions of oxy-water, the author caused hydrated peroxide of manganese and peroxide of lead to react upon an excess of oxy-water. On estimating the undecomposed peroxide of hydrogen, he found that these bodies act upon oxy-water atom for atom.

On working in a similar manner with bichromate of potash, he, as in his first experiments, obtained variable results, according to which from 3·2 to 4·2 molecules of oxy-water are decomposed by one molecule of bichromate of potash; this may be explained by admitting with M. Barreswil, that perchromic acid is formed at first, but that at the same time a portion of the chromic acid is immediately reduced by the oxy-water before being peroxidised by it.

M. Aschoff has found a still more conclusive proof of the existence of perchromic acid. M. Barreswil had observed that on carefully mixing very dilute solutions of bichromate of potash and peroxide of hydrogen, the disengagement of oxygen does not take place immediately; that the ether perfectly removes from the water the combination which communicates to it the blue colour; that the ethereal solution, much more stable than the aqueous solution, leaves, when evaporated at a low temperature, chromic acid without a trace of oxide; and

[1] *Jahresbericht der Chemie*, 1858, p. 340.

[1] *Annalen de Chemie und Pharmacie*, xci. 128.

[2] *Repertoire de Chimie pure*, i. 205.

that this same solution, shaken with an excess of alkali, becomes converted into chromate with disengagement of oxygen.

On shaking a known volume of the blue ethereal solution with an acid standard solution of a ferrous salt, there is an immediate decoloration, and the liquid then only contains oxide of chromium, peroxide of iron, and an excess of protoxide of iron, which can be estimated in the usual way. If the blue liquid is previously treated with potash, and then mixed with ferrous sulphate, it is found that the amount of protoxide of iron changed into peroxide is not the same as in the preceding experiment, and that the quantities of oxygen yielded are in the proportion of 4 to 3, as would be the case on the supposition that the blue liquid contains perchromic acid, and that the effect of the potash is to change this acid into chromic acid.—*Journal für Praktische Chemie*, lxxxi. 401.

On the Colouring Matters Derived from Aniline, by Dr. A. W. Hofmann, *F.R.S.*

In a note presented to the Academy on September 20, 1858, on the action of tetra-chloride of carbon on aniline, I described carbo-triphenyl-triamine,

$$C_{19}H_{17}N_3 = \left.\begin{matrix} C'''' \\ (C_6H_5)_3 \\ H_2 \end{matrix}\right\} N_3$$

a crystalline base formed by the condensation of three molecules of aniline, reunited by carbon substituted for hydrogen.

The production of this body is accompanied by that of a magnificent crimson colouring matter.

It may, perhaps, be useful to reproduce the passage which treats of the colouring matter:—

"On submitting a mixture of bichloride of carbon and three parts of aniline, both in the anhydrous state, for nearly thirty hours, to a temperature of 170° to 180°, the liquid becomes transformed into a black mass, either soft and viscid, or hard and brittle, according to the length of time and the temperature.

"This black mass adhering persistently to the tubes in which the reaction takes place, is a mixture of several bodies. On exhausting with water a portion is dissolved, another remaining insoluble in the form of a more or less solid resin.

"The aqueous solution yields with potash an oily precipitate, containing a considerable proportion of unchanged aniline. On boiling this precipitate in a retort with dilute potash, the aniline distils over, whilst there remains a viscid oil gradually solidifying with a crystalline structure. Washing with cold alcohol and one or two crystallisations from boiling alcohol, render the body perfectly white and pure, a very soluble substance of a magnificent crimson remaining in solution.

"That portion of the black mass which remained insoluble in water, dissolves very easily in hydrochloric acid; it is again precipitated from this solution by alkalies in the form of a amorphous powder of a dingy red colour, soluble in alcohol, which it colours a rich crimson. The greater portion of this substance is the same colouring matter which accompanies the crystalline body."

The action of tetra-chloride of carbon on aniline only furnishes a comparatively small quantity of the red matter; moreover, the temperature to which the mixture is exposed, and the relative proportions of the two substances which react on one another, are not without influence on the result of the experiment. Carbo-triphenyl-triamine and the base, which takes a crimson tint on dissolving in alcohol, are not the only products of the reaction. Other bases are formed, for the most part amorphous, and accessible only in the form of platinum salts, which on account of the similarity of their chemical characters hinder the purification of the new compound. In spite of many attempts, I could not succeed in obtaining the colouring matter in a fit state for analysis, and I temporarily abandoned the investigation.

However, commerce has not failed to discover new and more advantageous methods for the production of aniline red. Certain metallic chlorides (tetra-chloride of tin) and nitrates (mercurous nitrate) as well as a large number of oxidising agents, are capable of converting aniline into this crimson body. M. Verguin was the first to prepare the colour on the large scale by the action of tetra-chloride of tin on aniline. Since that time the production of aniline red has become an important branch of manufacture, which, in the hands of MM. Renard Brothers, in France, and Messrs. Simpson, Maule, and Nicholson, in England, has rapidly attained colossal proportions. The interest attached to this subject is evident on glancing at contemporary periodicals. The journals of applied chemistry especially furnish numerous descriptions and processes for the formation of the colouring matter which it has been proposed to call Fuchsine, Magenta, or other fantastic terms. The action even of tetra-chloride of carbon on aniline, which, at first sight, does not appear to possess any importance, has been utilised on the large scale, and interesting observations on the commercial production of the colour by means of chloride of carbon have been published by M. Charles Dolfus Galline[1], by MM. Monnet and Dury[2], and finally by M. Louth[3], and have proved that aniline red thus prepared on a large scale can be applied to dyeing, and furnishes exactly the same results as the colouring matter produced by other methods. This is not the place to give the details of the development of this new branch of industry, which has, moreover, been admirably traced by M. E. Kopp in a series of interesting articles. I have, however, thought it proper to quote the above authorities, so as to show that the basic colouring matter which I observed in 1858, when studying the action of tetra-chloride of carbon on aniline, is identical with the aniline red now manufactured by several processes on so large a scale.

A substance possessing such remarkable properties as aniline red, and which can also be procured as a commercial product, merits the attention of men of science.

The subject has been successively examined by M. Guignet[4], M. Béchamp[5], M. Wilm[6], MM. Persoz, De Luynes, and Salvétat[7], M. Schneider[8], and more recently by M. Emile Kopp[9] and by M. Bolley[10]. The results obtained by these experiments are far from being concordant. I attribute this divergence of the results on the part of such skilful observers, to the obstacles which they met with in procuring the colouring matter in the

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1 *Repertoire de Chimie Appliquée*, 1861, p. 11.
2 *Ibid*, p. 12.
3 *Ibid.*
4 *Bull. Soc. Chim.*, December 23, 1859.
5 *Annales de Chim. et de Phys.*, 3°, lix., 396.
6 *Bull. Soc. Chim.*, July 27, 1861.
7 *Comptes-Rendus*, li., 538.
8 *Ibid.*, li., 1087.
9 *Annales de Chim. et de Phys.*, 3°, lxii., 222.
10 Dingler's *Pol. Journal*, clx., 57.

state of purity, and to the facility with which the smallest quantity of foreign matter is capable of masking the properties of this remarkable compound.

The red colouring matter of aniline and its saline compounds appear to have been obtained for the first time in a state of purity by my friend and former pupil, Mr. Edward Chambers Nicholson, a manufacturer as much distinguished by his scientific erudition as by the skill and persistent energy which has several times enabled him to render the results of purely scientific researches available for commercial purposes.

With great liberality, Mr. Nicholson has placed at my disposal, not only a very complete series of the magnificent compounds which he prepares, but also the numerous and precise observations which he has accumulated on this subject during his lengthened researches. It is, therefore, owing to the kindness of Mr. Nicholson, that I have myself been enabled to study these remarkable compounds.

Mr. Nicholson calls the pure base of the colouring matter by the name of *roseine*, which appears very appropriate, since this substance, which yields solution of so beautiful a rose colour, is, when in the solid state, perfectly white. However, as this body appears to be the prototype of an entire series of similar compounds, which can be obtained by the application of similar methods to the homologues and probably to the analogues of aniline, it will be useful to recall the origin of this substance by its name. I therefore propose the name of *rosaniline* for the new base.

(To be continued.)

On the Action of Chlorine on Metallic Oxides, by M. R. WEBER.

HAVING attempted to prepare oxychloride of arsenic by the action of dry chlorine on arsenious acid, M. Weber obtained, instead of the body sought for, chloride of arsenic; a portion of the arsenious acid being at the same time changed into arsenic acid, which was itself converted into chloride of arsenic at a higher temperature. Antimonious acid behaved in an analogous manner. Stannic acid and peroxide of iron were decomposed by chlorine at a high temperature.

Having continued the study of the action of chlorine on the other metallic oxides, the author found that alumina, and even silica, were partially transformed, at a red-white heat, into chlorides of aluminium and silicium, without the action of the chlorine being assisted by the presence of carbon. This action is much weaker on silica than on alumina.

It is already known, by the experiments of Davy and those of Gay-Lussac and Thénard, that baryta and lime are changed into chlorides with ignition when the commencement of the reaction is assisted by heating the substance to near redness.

Strontia behaves in the same way.

Magnesia requires to be raised to a much higher temperature for its decomposition to take place.

Carbonate of protoxide of manganese becomes brown in a current of chlorine, and the residue contains chloride of manganese and a higher oxide of this metal. Upon heating the residue higher in a current of chlorine, the whole is changed into chloride of manganese.

Peroxide of iron gives at a red heat perchloride, which sublimes in small crystalline flakes. This reaction is slow, and requires a high temperature.

Carbonates of nickel and cobalt, and the oxides of zinc, cadmium, copper, and lead, easily yield the corresponding chlorides.

Protoxide of tin burns in chlorine with the production of bichloride and stannic acid. At a red heat the stannic acid itself is changed into bichloride.

Molybdic acid volatilises in the state of oxychloride. Tungstic acid acts in the same way; there is, however, noticed at the same time the formation of a few crystalline needles of chloride of tungsten.

Unignited sesquioxide of chromium, treated with chlorine at a temperature below that at which the incandescence of this oxide is produced, gives red vapours of oxychloride of chromium. At a red heat the same vapours are also obtained, together with violet chloride of chromium.

M. Rose has already shown the action of chlorine on tellurous acid and on oxide of bismuth.[1] It is also known, through the experiments of Gay-Lussac and Thénard, that chlorine decomposes aqueous vapour at a red heat.[2]

To sum up, the affinity of chlorine at high temperatures appears to surpass that of oxygen for the metallic radicals.—*Poggendorff's Annalen*, cxii. 619.

PHARMACY, TOXICOLOGY, &c.

On the Behaviour of Essential Oils to Iodine and Bromine, by JOHN M. MAISCH.

(*Continued from page* 119.)

Oleum Calami.—Thickish, pale yellow.

Iodine dissolves in the heaviest oil, without vapours, a tough extractive mass; oil of medium specific gravity evolves a few vapours; the lightest oil shows a higher temperature and more green and yellow vapours; the residue of the last two was softer. Z.

It dissolves slowly to a thick, nearly black liquid, some of the iodine adhering firmly to the glass.

Ether sol. iodine mixes quickly to an iodine-coloured liquid, with a darker sediment.

Ether sol. bromine works from the centre towards the circumference, one-half of the mixture being of a yolk yellow colour, the other half of a reddish brown; the first passes through various shades into a brownish green supernatant liquid; the last changes to a rose colour, and passing through reddish brown to a deep black brown, constitutes the sedimentary fluid.

Oleum Carui.—Thin, colourless.

Iodine dissolves without commotion or vapours in the heaviest oil (specific gravity ·96); by the oil of ·91 and ·92 prepared from recently dried seed, few gray and violet vapours are evolved; oil ten years old of ·939 specific gravity produces a lively disengagement of violet vapours and some heat; oil of ·947 specific gravity, distilled from old seed, is fulminating with violent evolution of vapours. The reddish brown residue of all the various oils shows little thickening and have but little of a caraway odour, but a peculiar acidulous balsamic smell. Iodine appears to be not a reliable test for oil of caraway. Z.

It dissolves with a radiating motion and little heat to a yellowish red transparent liquid, which is readily miscible with the darker and thicker sediment.

Ether sol. iodine.—Brisk effervescence by the first drop, the second and third drops mix quietly with the

[1] *Poggendorff's Annalen*, xxi. 443.

[2] *Recherches Physico-Chimiques*, ii. 144.

oil to a thin homogeneous fluid of iodine colour, with scarcely any precipitate.

Ether sol. bromine shows little reaction; a circular yellowish brown colour is produced; the supernatant liquid nearly colourless and perfectly transparent, afterwards light brown and cloudy.

Oleum Carui.—Yellow, old, thick, oily.

Iodine.—There was some reaction, but no perceptible heat and no vapours; the residue was a yellowish brown liquid and a dark brown extractive mass, which were readily miscible. Odour balsamic, acidulous.

Ether sol. iodine.—Some irregular motion towards the circumference; the homogeneous thick liquid has a concentrated iodine colour, a small quantity of a lighter liquid separates, which afterwards re-unites to a uniform thick balsamic mass.

Bromine produces a hissing noise on coming in contact with the oil, and evolves some gray vapours; the mixture consists now of a deep reddish brown sediment and a light yellow coloured oil, which are readily miscible to a yellow oil with but a faintly brown tint. Odour little modified.

Ether sol. bromine appears not to readily mix at first, but is for some time retained in a semicircle of yellowish brown, afterwards red brown; the supernatant oil is light brown and slightly turbid.

Oleum Caryophylli.—Fresh specimen, pale, yellowish brown.

Iodine dissolved slowly and quietly to a greenish yellow brown mass of honey consistence and unaltered odour; the India oil dissolves it still slower to a clear, viscid, yellowish red liquid. Z.

Slow solution, at first yellow, afterwards reddish, thick odour unaltered.

Ether sol. iodine mixes uniformly to a yellowish red, iodine coloured liquid.

Ether sol. bromine.—The yellow colour of the mixture quickly disappears, by changing to greenish, greyish green; an aqueous liquid (from the ether) collects on the surface and is nearly colourless; the oily heavy stratum alters its colour to a pale greyish black, then brownish black.

Oleum Cinnamomi Chinensis.—Fresh specimen, brownish yellow.

Iodine dissolves quickly in the oil of Ceylon cinnamon, with little radiating motion and much heat, to a tenacious, greenish yellow brown mass of cinnamon odour. In the oil of Chinese cinnamon, iodine dissolves slowly without any visible motion or reaction, and, with little heat, to a yellowish red brown mass of the consistence of soft extracts, and of cinnamon odour. Z.

It dissolves quietly to an iodine coloured liquid; it then turns yellowish brown, blackish brown, and thickens considerably.

Ether sol. iodine.—No spreading; the colour changes from iodine to a peculiar yellowish brown black; the liquid is of a uniform thick consistence and in thin layers transparent; in six hours it is of a darker colour and the consistence of a soft extract.

Ether sol. bromine mixes readily to a lemon yellow liquid, which gradually turns brown to umber brown, and is less thick than the product of the reaction of iodine.

Oleum Chenopodii Sem.—Thin, reddish yellow, Baltimore oil.

Iodine.—Rather slow reaction, without the evolution of vapours; the residue consists of a yellowish brown liquid and a dark brown resinous iodine compound, readily miscible to a brown or yellowish brown oil.

Ether sol. iodine causes a slight effervescence and forms two strata, the upper one of which is yellow, the lower one of a thick iodine colour; afterwards spreading takes place, and in a few hours nothing has been left in the vessel.

Bromine.—Hissing fulmination, with red and white vapours; the residue is a reddish brown resinous mass, and a light brown liquid, which after a while are miscible to a deep yellowish brown oil. Odour altered, somewhat resinous.

Ether sol. bromine.—The reaction proceeds slowly; the heavier liquid is reddish, tinged with brown; the supernatant fluid is light brown, almost clear.

Oleum Erigeronis.

The commercial article of this oil, which has a yellowish brown colour, and has been recommended by eclectic practitioners, was used for the following experiments. As Mr. F. L. John, of Philadelphia, obtained but an exceedingly small quantity from Erigeron Philadelphicum, the examined oil must be gained from Erigeron Canadense; but I have no means of ascertaining its purity.

Iodine dissolves with radiating motion, but without explosion; the solution is iodine-coloured, and has a thick blackish sediment.

Ether sol. iodine mixes to a solution and sediment of the same properties.

Ether sol. bromine is miscible, then separates to a brownish green yellow thin liquid, a brownish black sediment, and a few violet-coloured spots.

Oleum Fœniculi—Almost colourless, agreeable and pure fennel odour.

Iodine.—Oils, rich in stearopten, show radiating motion, evolve some reddish vapours, and thicken to a syrupy or soft extractive mass; oils with much stearopten, produce almost instantly a solid brittle mass, which prevents the further reaction of iodine. Iodine seems, therefore, applicable as a test for the proportion of stearopten in oil or fennel; but stearopten altered by age does not congeal to such a brittle mass, and the specific gravity must then decide whether the oil is in its fresh state, or old and resinified. Z.

Iodine dissolves with radiating motion, and some vapours, to a yellow, afterwards red liquid; the dark sediment is with some difficulty miscible, and forms an extractive mass.

Ether sol. iodine mixes uniformly to a bright red iodine-coloured solution, which, on standing six hours, is reddish brown, viscid.

Ether sol. bromine produces a mixture which is at first white and milky, and gradually shows some brown streaks; afterwards the supernatant fluid is milk white, tinged with greenish brown; the sediment has a purely brown colour.

Oleum Gaultheriæ.—Brownish yellow, thin, oily.

Iodine dissolves quickly and completely with brisk reaction and some white vapours; the liquid is not thickened, and has a yellow iodine colour.

Ether sol. iodine.—Some spreading; miscible to a yellowish red solution; after six hours the thin liquid is of a deep reddish yellow, without a trace of sediment.

Ether sol. bromine.—Many white fumes are given off, reminding somewhat of the odour of fresh parsley-root; the whole watch crystal is covered with a coating of a soft, resinous white substance, leaving some unaltered oil behind.

(To be continued.)

PHYSICAL SCIENCE.

On the Molecular Dissymmetry of Natural Organic Products, by L. Pasteur, *Member of the Chemical Society of Paris.*

(*Continued from page* 120.)

III. When I began to devote myself to particular pursuits, I sought to fortify myself in the study of crystals, anticipating that I would derive assistance from it in my chemical researches. The most simple means, it seemed to me, was to take a somewhat extensive work upon crystalline forms, to repeat all the measurements, and to compare my determinations with those of the author. In 1841, M. de la Provostage, whose exactness is well known, published a handsome work on the crystalline forms of tartaric acid, paratartaric acid, and their saline combinations. I took hold of this memoir. I crystallised tartaric acid and its salts, and I studied the form, of their crystals. But in my progress, I perceived that a very interesting fact had escaped the learned physicist. All the tartrates I studied afforded decided indications of hemihedric faces. This particularity of form in the tartrates was not very evident. This may be readily conceived, since it had not yet been observed. But when, in one species, it appeared in doubtful characters, I always succeeded in rendering it more manifest, by recommencing the crystallisation and slightly modifying the conditions. Sometimes the crystals bore all the faces required by the law of symmetry, but hemihedrity is the frequent irregularities of crystals, which are never easily developed. Hence their results, deformities, arrests of development in this or that direction, faces accidentally suppressed, &c. Except under circumstances almost exceptional, the ascertaining of hemihedrity, especially in crystals of the laboratory, requires a very attentive study. Besides that, although hemihedrity may be possible in a form, although it may be a function of the internal structure of the body, it may not be externally manifest any more than is found upon every crystal of the cubic species all the forms compatible with the cube.

But be that as it may, I repeat, I found hemihedric tartrates.

This observation would have been barren, probably without the following:—

Let a, b, c, be the parameters of the crystalline form of any tartrate; α, β, γ, be the angles of the crystallographic axes. These are ordinarily right angles or a little oblique. Besides the relation of the two parameters, such as a and b, is nearly the same in the different tartrates, whatever may be their composition, the quantity of their water of crystallisation, the nature of their bases; γ alone differs sensibly. There is a kind of semi-isomorphism among all the tartrates. One might say that the tartaric group prevails, and impresses a stamp of resemblance between these different forms, in spite of the difference of the other constituent elements.

It follows from this that there is something in common in the forms of all the tartrates, and it is possible to arrange them similarly, in taking for example, as character of similar position, the position of the axes α and β.

Now, if the disposition of the hemihedric faces be compared on all the prisms of the primitive forms of the tartrates, arranged in the same manner, this disposition is found to be the same.

Let us sum up, in two words, these results, which have been the point of departure in all my ulterior researches; the tartrates are hemihedric, and they are so in the same direction.

Guided, on the one hand, by the fact of the existence of molecular rotary polarization, discovered by M. Biot, in tartaric acid and in all its combinations; on the other by the ingenious approximation of Herschel; in the third place, by the learned views of M. Delafosse, with whom hemihedrity has always been a law of structure and not an accident of crystallisation, I presumed there might be a correlation between the hemihedrity of the tartrates and their property of deviating the plane of polarized light.

It is important to seize here the sequence of ideas.

Haüy and Weiss ascertained that in quartz there exist hemihedric faces, and that these faces fall to the right in certain specimens, and to the left in others. On his side, M. Biot found that crystals of quartz are also divided into two groups, under the relation of their optical properties, some deviating to the right, and the others deviating to the left, the plane of polarized light according to the same laws. Herschel comes in his turn, places between these two facts, until then isolated, a line of connexion, and says:—The plagihedrals of one direction deviate in the same direction; the plagihedrals of the other direction deviate in the opposite direction.

For my part, I find that all the tartrates are plagihedral, if I may so express myself, and that they are all in the same direction. I should then presume that here, as in quartz, there was a correlation between hemihedrity and circular polarization. At all events, the essential differences which I have just noticed between the circular polarization of quartz and that of tartaric acid ought not to be neglected.

We are now, thanks to the new facts which precede, and to the approximations which I have just enumerated, in possession of a preconceived idea (for it is, as yet, nothing more) on the possible correlation of hemihedrity and the rotary power of the tartrates.

Very desirous to find in experiment a corroboration of this still speculative proof, my first thought was to ascertain whether the very numerous crystallisable organic products, which possess the molecular rotary property, have hemihedric crystalline forms, which no one suspected, notwithstanding the approximation of Herschel. This investigation had the success which I anticipated for it.

I occupied myself also with the examination of the crystalline forms of paratartaric acid and its salts, substances isomeric with tartaric combinations, but all of which M. Biot found inoperative upon polarized light. None was found hemihedric.

The idea of the correlation of hemihedrity, and the molecular rotary power of natural organic products, gained ground.

I was soon led to develope it clearly by a very unexpected discovery.

(To be continued.)

Chemical Society.—Arrangements have been made for the delivery of the following Discourses during the present year: April 3, H. Débus, Ph.D., F.R.S., "On the Influence of the Quantitative Method in the Development of Scientific Chemistry." May 1, T. Anderson, M.D., F.R.S.E., "On the Chemistry of Opium." June 5, W. Marcet, M.D., F.R.S., "On the Chemical Phenomena of Digestion." November 6, A. W. Hofmann, LL.D., F.R.S., "On Vapour-Densities." A Fifth Lecture will probably be delivered by Prof. Wurtz, of Paris.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Lecture by Dr. W. ODLING, *F.R.S., "On Mr.* GRAHAM'S *Researches in Dialysis," delivered at the Royal Institution on Friday, February* 14, 1862.

BEFORE proceeding to unfold to you in regular order the subject which I have undertaken to bring under your notice this evening, I wish to direct your attention for a few moments to one or two experiments which, as some little time is required for their performance, I will now set in operation, so that they may be ready for me to refer to at a later part of my address. The instrument which I hold in my hand is called a Dialyser. It is constructed somewhat like a tambourine. It consists of a piece of membrane, or vellum, or parchment paper stretched tightly over a hoop of gutta-percha, so as to form a sort of tray capable of holding water, and capable of being floated upon water. In each of these jars is a dialyser of similar construction to the one in my hand, but somewhat smaller. We have in this bottle a quantity of the red colouring matter obtained from coal tar, known as Magenta; and I am about to pour a little of it into one of these dialysers [A small portion of the Magenta was floated in a dialyser.] Now, I have here another red colouring matter, though a much less brilliant one; nevertheless it is possessed of very considerable tinctorial power, as I can show you by pouring a small quantity of it into a glass of water. This is the colouring matter of blood. You perceive that a very little of it is sufficient to impart a considerable degree of colouration to a large body of liquid. Now, I will pour some of this other red colouring matter—the colouring matter of blood—into this second dialyser,—[referring to one floating in another jar.] I have here a brown colouring matter, caramel, obtained by the roasting, or rather, heating of sugar. Its colour, I suppose, will be tolerably apparent at a distance. I will mix some of this brown colouring matter with the Magenta, and pour the two into the dialyser floating on the water in this tall jar [referring to a third]. We will allow the whole to remain undisturbed some little time, and at a later period of the lecture return to our examination of the jars in order to see whether or not any effects have taken place.

And now, by way of introduction to the proper subject of my story this evening, I will recall to your recollection that intermixture, or diffusion, which takes place when two different liquids are in contact with one another. I have here a tube bent in the shape of the letter U, such a one as is ordinarily known in laboratories as a U-tube. Into one limb I have already introduced a weak solution of common salt, which, for the sake of greater distinctness I have coloured pale blue. Into the other limb I now pour a solution of another salt,—namely, Epsom salts, or sulphate of magnesia, of about equal strength, which, for the sake of distinctness, I have coloured red. We have now in one limb of the tube a weak solution of common salt, coloured blue, and in the other limb of the tube a weak solution of Epsom salts, coloured red. The question is, whether these two salts will remain separate, as they now are, or whether they will mix with one another. We find, as a matter of observation, that after a certain time a mixture does take place A slow diffusion of the one into the other occurs; the common salt passes into the sulphate of magnesia limb, and the sulphate of magnesia passes into the common salt limb, until a perfect uniformity of composition in the liquid is established. I may show you the experiment in a somewhat different manner. Here, instead of taking a double-limbed tube, I take a double-bulbed tube. The lower bulb is filled with a solution of common salt, coloured pale blue. I will now pour in some water; and here we shall have to consider the same question as before, Will the heavy common salt remain at the bottom of the tube, or will it gradually rise up into the light water? Here, as in the other instance, repeated observation tells us that the heavy common salt will not remain at the bottom of the tube, as, by its superior gravity, we might expect it would, but that it will, in opposition to the law of gravity, so far as that alone is concerned, rise up through the light water. Now, this experiment, although a very well-known one, is, nevertheless, of considerable interest. We have to inquire why the common salt, which is heavy, should not remain at the bottom, instead of rising up to the top through the light water. We admit generally that, in order to set a body in motion, the action of some external force is required. Now, I would ask, what is the external force which causes the heavy common salt to rise up through the light water? I have poured in this water somewhat roughly, and thereby have created a greater disturbance of the liquids than I intended, so that some of the blue solution of common salt has, by a mere accidental shaking, got into the upper bulb; but, quite irrespective of that shaking, a quantity of the common salt would gradually rise into the upper bulb and into the stem above it.

This rising up of the common salt into the water is termed its diffusion. The phenomena of liquid diffusion were first examined some fifteen or sixteen years ago by Mr. Graham, who has, with more or less of interruption, continued his researches upon the subject down to the present time; and it is to some of his results that I propose to direct your attention this evening.

His first experiments were conducted by means of a process which he termed "vial diffusion." The substance was allowed to diffuse from a small vial or cylinder. The experiment was conducted somewhat in this manner:—I have here two glass jars, one within the other. The internal jar is filled nearly to the top with a solution of chloride of copper—the salt whose diffusiveness is to be made the subject of experiment. I may here direct your attention to a similar experiment which is going on in the actual kind of apparatus used by Mr. Graham. In the internal cylinder or vial of this jar we are causing the diffusion to take place, not of the green salt, chloride of copper, but of the yellow salt, bichromate of potash. You see the interior vial containing the bichromate of potash solution, and the exterior jar the water. Well, the experiment was conducted in this manner. The interior cylinder was filled with the solution of the salt whose diffusiveness was to be ascertained; it was then introduced gradually into an external jar of water, and the whole was set aside for some time. During that time a portion of the salt contained in the interior vial diffused itself out into the exterior liquid. The experiment was terminated by withdrawing the interior vial gradually and then ascertaining, by evaporating down the external liquid, or by some other means, how much of the salt from the vial had got into the external water. This was the mode in which his original experiments were conducted. I may say, with regard to these two experiments which are in progress in the jars to which I have just referred, that they were arranged this morning at about eleven o'clock, and you perceive that very little, if any, perceptible diffusion has taken place. Nevertheless, some of the salt has in each instance passed into the external liquid, as we might very readily prove.

Now, among the very many interesting results to which this very simple process gave origin, there are only two or three which have a direct bearing upon the subject of my lecture this evening, and of them I will now speak.

The first general conclusion arrived at was this—that different salts differ very much from one another in the rapidity with which they diffuse. That is shown in this table labelled "Vial Diffusion." A series of these vials, perfectly similar to one another, having the same capacity, and having, more particularly, the same area of

opening, were filled with the same quantities of solutions of the same strength of different substances; one with a 20 per cent. solution of common salt, one with a 20 per cent. solution of sulphate of magnesia, one with a 20 per cent. solution of nitrate of soda, and another with a 20 per cent. solution of gum. They were introduced respectively into jars of water of this size and character—possibly into this identical jar—and allowed to diffuse for eight days at the temperature of 60°. This table [pointing to the subjoined] gives some of the results obtained:—

Vial Diffusion.

Substance.	Diffusate.
Chloride of Sodium	58·68
Sulphate of Magnesia	27·42
Nitrate of Soda	51·56
Sugar	26·74
Gum	13·24
Albumen	3·08

It was found that from the 20 per cent. solution of chloride of sodium or common salt, 58·68, or rather more than 58½ grains, had passed out into the larger jar—had diffused in fact. Now, this quantity which had passed out into the external jar was spoken of by Mr. Graham as the *diffusate*. He found that from the 20 per cent. solution of sulphate of magnesia not quite 27½ grains had diffused; from the solution of sugar 26·74 grains had diffused; from the solution of gum 13 grains; and from that of the albumen only 3 grains.

Now, it is obvious from a mere inspection of this table that the process of diffusion might be made available to effect, at any rate, a *partial* separation of substances—a partial separation, for instance, of a highly diffusive substance, like chloride of sodium, from a feebly diffusive substance, like albumen. Thus, if we were to take a solution containing equal weights of albumen and common salt, and were to pour it into a jar of water, the ratio of the albumen to the common salt would be the same in the dilute liquid resulting from the mixture of the water into which we pour it and the solution, as it would be in the original solution itself. But if, instead of pouring the solution into a mass of water, we were to pour it into one of these vials, and then allow it to diffuse into a mass of water, a very different result would take place: for every 58 grains of common salt that passed out there would be only 3 grains of albumen pass out into the external vessel, so that whilst in the liquid into which we poured the solution we should have the two compounds in the ratio of 1 to 1, or 100 to 100, in the liquid into which we allowed it to diffuse the ratio of common salt to albumen would be as 58½ to 3, or as 100 to about 5.

But even this is not all. Not only are we capable by diffusion of effecting a partial separation of bodies which are merely mixed together, but we are also capable of effecting the decomposition of definite chemical compounds. I have here a piece of alum, a remarkably definite and crystalline body. It is a double sulphate of the two bases potash and alumina. Now, if we make a solution of this alum, and pour it into one of these vials, and introduce the vial into a diffusion-jar and allow diffusion to take place, the tendency of the potash to diffuse being much greater than that of the alumina, the potash actually breaks away from the alumina with which it was in combination, in order to diffuse itself into the external water; so that at the termination of the experiment we find in this exterior liquid a certain quantity of free sulphate of potash, whereas in the internal vial we find a certain quantity of free sulphate of alumina, which sulphate of potash and sulphate of alumina have resulted from the chemical decomposition,—that is, the breaking up of the chemical compound, alum, effected by the superior tendency which the potash had to diffuse over the tendency which the alumina had. And so in a great number of other instances, actual decomposition may be produced by the process of diffusion.

Mr. Graham's later series of experiments on diffusion were conducted in a somewhat different manner. A certain quantity of water was introduced into a jar, and the saline solution,—the solution of salt, whose diffusiveness was to be ascertained,—was then carefully conveyed to the bottom of the jar by means of a pipette. I have here the experiment on a somewhat large scale. We have constructed a pipette out of a separater, and we will now allow a quantity of common salt solution (which, for the sake of distinction, I have coloured red) to pass down through the water. We are now conveying it to the bottom of the jar, where, you perceive, it forms a distinct layer. I have made this experiment on a somewhat larger scale than Mr. Graham employed, in order that it may be seen over the theatre. Here, however, we have one of Mr. Graham's jars, in which an experiment was begun early this morning. It will be rather difficult for me to render it visible all over the theatre, but some of you will see that there is here at the bottom of the jar a distinct layer of yellow liquid, namely, a solution of bichromate of potash, which was introduced this morning in much the same manner as I am now introducing the red liquid. The salt so introduced is allowed to diffuse into the superincumbent water. The experiment is conducted for a certain number of hours or days, and at the end of that time the exterior liquid is very carefully drawn off from the exact top of the liquid by means of a syphon, in definite layers. For instance, in this mode of conducting the experiment, there were 700 cubic centimètres of the liquid, and 100 cubic centimètres of the solution of the salt to be diffused. After the diffusion had gone on for some days, the top 700 centimètres of liquid were drawn off in portions not of 100 but of 50 centimètres, so as to make 14 portions or layers of 50 cubic centimètres each; a double layer of 100 centimètres being left at the bottom. The amount of salt which had diffused into each layer was then ascertained by evaporating each one separately, or the amount of salt contained in each layer was determined in some other manner. Some of the results obtained in this way are given in the table. I should say that, for the sake of distinguishing between the two processes, Mr. Graham designated this one (that of the former experiment) as vial diffusion; while this (the experiment last described) he called jar diffusion. These, then are some of the results of jar diffusion:—

Jar Diffusion for Fourteen Days.

Stratum.	Common Salt.	Epsom Salts.	Albumen.
1	·104	·007	
2	·129	·011	
3	·162	·018	
4	·198	·027	
5	·267	·049	
6	·340	·085	
7	·429	·133	
8	·535	·218	·010
9	·654	·331	·015
10	·766	·499	·047
11	·881	·730	·113
12	·991	1·022	·343
13	1·090	1·383	·855
14	1·187	1·803	1·892
15 and 16	2·266	3·684	6·725

Let us consider the results obtained with a solution of common salt. Originally there were 10 grammes of common salt introduced into the 100 centimètres of liquid which occupied the lowest layers. In the top layer we now find only $\frac{1}{10}$th of a gramme of salt; in the 5th layer we find nearly $\frac{2}{10}$ths; in the 7th layer $\frac{3}{10}$ths; in the 8th layer $\frac{4}{10}$ths; in the 12th layer $\frac{9}{10}$ths; in the 13th layer $\frac{10}{10}$ths; and in the 14th layer $\frac{11}{10}$ths of a gramme of salt. The two original layers, the 15th and 16th, were not separated, but examined together, and in them were found $\frac{23}{10}$ths of a gramme of the salt. Now, if we compare

this diffusion of common salt with the diffusion of albumen which took place under precisely the same conditions, we find that, whereas there was a considerable quantity of common salt in the topmost stratum of the liquid into which that substance had diffused, the albumen in the same time did not rise higher than the eighth stratum; and whereas in this case we introduced ten grammes of common salt into the lowest hundred cubic centimètres of liquid, and found that out of those 10 grammes $7\frac{3}{4}$ had passed out; in the case of the albumen we likewise introduced 10 grammes into the lowest 100 cubic centimètres, and found that less than 3 grammes had diffused into the superior strata. Perhaps these results will be rendered more evident by means of this diagram, which represents sections of the two jars. In the case of the common salt the whole of the two lower layers were at first occupied by the solution of common salt: at the end of the experiment we find that the common salt has actually risen up to the very top of the liquid, and could have risen considerably higher, as is shown by the fact that the quantity of common salt contained in the top layer of liquid into which it diffused is 20 times as great as the quantity of sulphate of magnesia contained in the top layer of liquid into which it diffused under precisely the same conditions. The common salt then not only rose to the top, but it could have risen higher, whereas the albumen rose only to the top of the eighth stratum. And whereas in the one case we have little more than two grammes of the common salt left in the lowest two layers, in the other case we have more than 7 grammes of the albumen, showing the great difference in the diffusive nature of salt and albumen. Of all the bodies in this table salt is the most highly diffusive; then sugar and sulphate of magnesia, which are much alike; gum is still less diffusive, and tannin much the same as gum; albumen, again, is still less; and caramel still less than albumen. Now, these results of jar diffusion bear out generally those of vial diffusion. We are capable of obtaining the same separation of salts from one another; and although my time is getting on very rapidly, I must for one moment direct your attention to the actual separation that was in this way effected. This larger table of the jar diffusion of mixed salts refers to an experiment in which a 5 per cent. solution of common salt was mixed with a 5 per cent. solution of the analogous chloride of potassium; and the two salts were allowed to diffuse together for a period of 7 days at the temperature of 55 degrees. Now, it was known that chloride of potassium diffused more rapidly than chloride of sodium. The results obtained by vial diffusion had shown that they diffused in about the proportion of 10 to rather more than 8. Now, the uppermost 6 strata of the liquid, into which the diffusion took place, were found to contain, altogether, at the end of 7 days, 561 milligrammes of the mixed salts, and out of that 561 milligrammes, 404 were chloride of potassium; so that these 6 upper layers contained, altogether, 72 per cent. of chloride of potassium. At the beginning of the experiment, the liquid contained of chloride of potassium and chloride of sodium in the proportion of 50 to 50; but in these top 6 layers we obtained a mixture of the two salts in the proportion of 72 per cent. of the one, and 28 per cent. of the other.

Now, this [pointing to another table] is a similar experiment, conducted with the same base, but with different acids. Sulphate of soda and chloride of sodium were mixed in solution and allowed to diffuse for 7 days at a temperature of 55, and with these results. I should say that the difference in diffusibility between the sulphate of soda and the chloride of sodium is very much greater than that between chloride of potassium and chloride of sodium, being in the proportion of 10 to 7 instead of 10 to 8. The first 6 strata contained altogether 263 milligrammes of mixed salts, and of those 263 milligrammes, 239 were common salt, and the remainder only was sulphate of soda or Glauber's salt; so that nearly 91 per cent. of the mixed salt contained in the 6 topmost layers was chloride of sodium, and only the remaining 9 per cent. was sulphate of soda.

(To be continued.)

Friday, January 17.

Sir Henry Holland, *Bart., M.D., D.C.L., F.R.S., Vice-President, in the Chair.*

A Paper, by Professor Tyndall, F.R.S., &c., was read "*On the Absorption and Radiation of Heat by Gaseous Matter.*" Resuming, with a new apparatus, his experiments on the influence of Chemical Combination on the Absorption and Radiation of Heat by Gases, the speaker, in the investigation of which the evening's discourse would be a *résumé*, first examines the deportment of chlorine as compared with hydrochloric acid, and of bromine as compared with hydrobromic acid, and finds that the act of combination which in each of these two cases notably diminishes the density of the gas, and renders the coloured gas perfectly transparent to light, renders it more opaque for obscure heat. He also draws attention to the fact that sulphur, which is partially opaque to light, is transparent to 54 per cent. of the rays issuing from a source of 100 C., while its compound, heavy spar, which is sensibly transparent to light, is quite opaque to the rays from a source of 100 C. He demonstrates, in confirmation of Melloni, the transparency of lampblack in thin layers; but shows how irreconcilable its deportment to radiant heat is with the idea generally prevalent at the present day, that lampblack absorbs heat of all kinds with the same intensity. All his experiments with gases have been repeated with a different source of heat, and he finds the result still more pronounced than formerly, that the compound gases far transcend the elementary ones in absorptive power. Taking air as unity, ammonia, at thirty inches tension, is 1195, this latter figure representing all the heat that issued from the source. A layer of ammonia, three feet long, is perfectly black to heat emanating from an obscure source. The coloured gases, chlorine and bromine, though much superior in absorptive power to the transparent elementary gases, are exceeded in this respect by every compound gas that has been hitherto examined. When, instead of tensions of thirty inches, we compare tensions of one inch, the differences between the gases come out still more strikingly. At this tension, for example, the absorption of sulphurous acid is 8000 times that of air. The speaker also referred to a new and extensive series of experiments on the Absorption of Radiant Heat by Vapours. The least energetic, as before, he finds to be bisulphide of carbon; the most energetic, boracic ether. He shows that the absorption of the latter vapour (which is quite transparent) at 0·1 of an inch of tension is 600 times the absorption of the densely coloured vapour of bromine, while in all probability it is 186,000 times that of air. The speaker was led by a series of perplexing experiments, which are fully described in a memoir recently presented to the Royal Society, to the solution of the following remarkable and at first sight utterly paradoxical problem:—"To determine the absorption and radiation of a gas or vapour without any source of heat external to the gaseous body itself." When air enters a vacuum it is heated by the stoppage of its motion; when a vessel containing air is exhausted by an air-pump, chilling is produced by the application of a portion of the heat of the air to generate *vis viva*. Let us call the heating in the first case dynamic heating, and the chilling in the second case dynamic chilling. Let us further call the radiation of a gas which has been heated dynamically, dynamic radiation, and the absorption of a gas which has been chilled dynamically, dynamic absorption. Placing a thermo-electric pile at the end of his experimental tube, the latter being exhausted, the gas to be examined is permitted to enter the tube; the gas is heated, and if it possess any sensible radiative power, the

pile will receive its radiation, and the galvanometer connected with the pile will declare it. Proceeding in this way with gases, Professor Tyndall found that the radiation thus manifested, and which was sometimes so intense as to urge the needle of the galvanometer through an arc of more than sixty degrees, followed the exact order of the absorptions which he had already determined. After the heat of the radiating column of gas had wasted itself, the air-pump was worked at a certain rate, the rarefied gas within the tube became chilled, and the face of the pile turned towards the chilled gas became correspondingly lowered in temperature. The dynamic absorptions of various gases were thus determined, and they were found to go strictly hand in hand with the dynamic radiation. In the case of vapours the following method was pursued: —A quantity of the vapour sufficient to depress the mercury column 0·5 of an inch was admitted into the tube, and this was heated dynamically by allowing dry air to enter till the tube was filled. The radiation of the vapours thus determined followed exactly the same order as the absorption which had already been measured. The dynamic absorption of the vapour was obtained by pumping out in the manner just described, and it was found to follow the same order as the dynamic radiation. In these experiments the air bore the same relationship to the vapour that a polished silver surface does to a coat of varnish laid over it. Neither the silver nor the air, both of which are elements or mixtures of elements, possesses the power of agitating in any marked degree the luminiferous ether. But the motion of the silver being communicated to the varnish, and the motion of the air being communicated to the vapour, molecules are agitated which have the power of disturbing, in a very considerable degree, the ether in which they swing. The speaker finds by strict experiments that the dynamic radiation of an amount of boracic ether vapour, possessing a tension of only $\frac{1}{10125\,0000}$th of an atmosphere is easily measurable. He also shows and explains the fact that with a tube 33 inches long, the dynamic radiation of acetic ether considerably exceeds that of olefiant gas; while in a tube 3 inches long, the dynamic radiation of olefiant gas considerably exceeds that of the ether. Aqueous vapour has been subjected to a special examination, and Professor Tyndall finds it a common fact for the aqueous vapour contained in the atmosphere to exercise 60 times the absorption of the air itself. The further he has pursued his attempts to obtain perfectly pure and dry air, the more has the air approached the character of a vacuum. He further points to the possibility of determining the temperature of space by direct experiment. Scents of various kinds have been examined. Dry air was passed over bibulous paper moistened by the essential oils, and carried into the experimental tube. Small as the amount of matter here entering the tube is known to be, it was found that the absorption of radiant heat by those odours varies from 30 times to 372 times that of the air which formed the vehicle. The speaker remarked that the absorption of terrestrial rays by the odour of a flower-bed may exceed in amount that of the entire oxygen and nitrogen of the atmosphere above the bed. Ozone has also been subjected to examination. The substance was obtained by the electrolysis of water, and from decomposing cells containing electrodes of various sizes. Calling the action of the ordinary oxygen, which entered the experimental tube with the ozone unity, the absorption of the ozone itself was in six different experiments,—21, 36, 47, 65, 85, 136. The augmenting action of the ozone accompanied the diminution of the size of the electrodes used in the decomposing cells. Professor Tyndall points out the perfect correspondence of these last results with those of M. Meidinger by a totally different method of experiment.

At the General Monthly Meeting held on Monday, March 3, 1862, William Pole, Esq., M.A., F.R.S., Treasurer and Vice-President, in the Chair, the following gentlemen were elected Members of the Royal Institution: —John Birkett, Esq., F.R.C.S., F.L.S.; Jonathan Sparrow Crowley, Esq., F.G.S.; Major-General Charles James Green; Alexander Henderson Macdougall, Esq.; The Rev. George Musgrave Musgrave, M.A. Oxon.; A. C. Brisbane Neill, M.D.; Francis Pirie, Esq.; Robert Pryor, Esq.; Sir Joshua Rowe, C.B.; Samuel Scott, Esq.; Edward Henry Sieveking, M.D.; Oswald Augustus Smith, Esq.; Alexander John Sutherland, M.D., F.R.S.; James Thomas White, Esq.; and Herbert George Yatman, Esq.

The following gentlemen were admitted Members of the Royal Institution:—Robert Russell Carew, Esq.; Wm. Whitaker Collins, Esq.; and John Parnell, Esq.

The thanks of the Meeting were returned to Professor T. H. Huxley, for his Discourse "On Fossil Remains of Man," on February 7; to Dr. W. Odling, for his Discourse "On Mr. Graham's Researches on Dialysis," on February 14; to James Fergusson, Esq., for his Discourse "On the Site of the Holy Sepulchre at Jerusalem," on February 21; and to A. E. Durham, Esq., for his Discourse "On Sleeping and Dreaming," on February 28.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, February 18, 1862.

J. P. Joule, *LL.D., F.R.S., President, in the Chair.*

Henry Ashworth, Esq., The Oaks, Bolton, and Thomas Clarke, M.D., Wilmslow, were elected Ordinary Members of the Society.

Mr. Dyer made some remarks relative to the first invention of the electric telegraph, and read the following extract from Arthur Young's "Travels in France" (Second Edition), London, 1794, which proved that electricity had been employed at that early date for the purpose of transmitting intelligence.

"In the evening to Mons. Lomond, a very ingenious and inventive mechanic, who has made an improvement in the jenny for spinning cotton. Common machines are said to make too hard a thread for certain fabrics, but this forms it loose and spongy. In electricity he has made a remarkable discovery. You write two or three words on a paper; he takes it with him into a room, and turns a machine enclosed in a cylindrical case, at the top of which is an electrometer, a small fine pith ball: a wire connects with a similar cylinder and electrometer in a distant apartment, and his wife by remarking the corresponding motions of the ball, writes down the words they indicate, from which it appears that he has formed an alphabet of motions. As the length of the wire makes no difference in the effect, a correspondence might be carried on at any distance; within and without a besieged town for instance, or for a purpose much more worthy, and a thousand times more harmless, between two lovers prohibited or prevented from any better connection. Whatever the use may be, the invention is beautiful. Mons. Lomond has many other curious machines, all the entire work of his own hands. Mechanical invention seems to be in him a natural propensity."

A Paper was read "*On the Present State of Meteorology*," by Mr. Thomas Hopkins, M.B.M.S.

In this paper the author represented that certain recent meteorological writers had abandoned the Hadleian theory, —of winds being caused by the ascent of sun-heated air in the tropical regions, and its passage through the upper atmospheric space, to descend in the polar regions, and return to the tropics. It was shown that great efforts had been made in different countries to discover the causes of those atmospheric disturbances which often take place,

without much uniformity in the conclusions arrived at. From extensive researches made by American observers, Commander Maury had attempted to prove that near to each tropic there was the crest of a large atmospheric wave, from which air flowed down towards the equator on one side, and towards the pole on the other; and that light air ascended from the surface in both the polar regions. Numerous English registrations have been placed in the hands of Admiral Fitzroy, who has not like Commander Maury, promulgated a new hypothesis, but has exhibited what he considers the general action of cyclonic storms in middle latitudes; this is, however, opposed to the Hadleian theory. Sir J. F. Herschel, in his elaborate work "*On Meteorology*," omits to notice the disturbing influence of the liberated heat of condensing vapour on the gases; but he also abandons the old theory of winds, and attributes them to the action of aqueous vapour in a new form. It is contended by the writer, that the great cause of atmospheric disturbance is to be found in the local heating of gases by the liberated heat of condensing vapour. It is then pointed out that the term "atmospheric wave" is founded on a false analogy, and leads the mind in a wrong direction. To speak of storms coming *from* a certain quarter also misleads, as the cause of storms is to be found in the part towards which the wind blows. In conclusion it was suggested that aëronauts, when ascending into the higher regions to ascertain the state of the atmosphere in those regions, should, in addition to the ordinary instruments, use a wet bulb thermometer in conjunction with a dry one, in order that the hygrometrical state of the upper regions may be ascertained.[1]

CHEMICAL DISCUSSION ASSOCIATION.

THE following is the Report of the Fourth Anniversary Meeting held on Monday, January 13, 1862, Dr. T. REDWOOD, President, in the chair:—

The progress which has been made by the Chemical Discussion Association during the past year has been upon the whole satisfactory. The number of Members upon the list at the present time amounts to thirty-three. Of these six have been elected during the past year. In the case of five, however, their election took place during, or since October, and, therefore, in accordance with Rule IX. their subscriptions date for 1862.

At the previous Anniversary Meeting it was resolved to effect an alteration in Rule XI. by which greater facilities would be afforded to the Committee for removing members in arrear.

That alteration has since received the necessary sanction of the Council of the Pharmaceutical Society, and the Committee have in accordance with the amended Rule removed the names of seven members who had expressed themselves unable to take any further active part in the Association.

Seven ordinary meetings have been held during the past year, and although the attendance has been rather below the average, the number of communications has been well maintained. Three of the communications have been already published in the *Pharmaceutical Journal*, and three or four others will probably also shortly appear.

The following is the list of subjects which have been brought forward during the past year:—

"On the Purification of Gum Resins," Mr. Allchin. "On some Fluid Preparations of Ipecacuanha," Mr. A. F. Haselden. "On the Iodide of Potassium and Compound Iodine Ointments," Mr. Hall. "On the Preparation of Smelling Salts," Mr. Allchin. "On the Ammonio Chrome Compounds," Mr. J. H. Baldock. "On some Reactions of Liquor Nasturtii," Mr. J. Schweitzer. "The Volatile Portion of Sesquicarbonate of Ammonia," Mr. C. H. Wood. "On the Action of Oxide of Silver on Creasote," Mr. Haselden. "On the Adulteration of Tin-Foil," Mr. J. H. Baldock. "On a Cheap Form of Apparatus for Spectrum Analysis," Mr. Alexander Waugh. "On the Nutritive Value of Dika Bread," Mr. Attfield. "Some Remarks on the Efflorescences which form on Medicinal Extracts," Mr. Attfield. "On the Detection of Alum in Bread," Mr. C. H. Wood; and "On the Preparation and Properties of Peroxide of Hydrogen," Mr. J. Robbins.

[1] Mr. Welsh employed the wet bulb thermometer in his balloon ascents. —*Phil. Trans.*, 1853, Pt. 3.—ED.]

NOTICES OF PATENTS.

1144. *Lubricating Compound.* W. E. NEWTON, Chancery Lane, London. A Communication. Dated May 6, 1861.

THE patentee claims the use for lubricating purposes of various saponified combinations of fatty matters with the alkalies, potash and soda; and particularly specifies the animal fats, bees-wax, and the vegetable oils as being suitable for the preparation of such materials.

The potash and soda compounds here referred to are not in any respect different from the two ordinary qualities of soap, which have been so long and generally employed for cooling the surfaces of cutting implements, and for lubricating the bearings of machinery.

1149. *Manufacture of Artificial Fuel.* J. B. JARLOT, Port Mabon, France. Dated May 7, 1861.

THIS specification describes the mechanical details of improvements in the construction of cylindrical rollers for the purpose of better adapting them to the moulding of small coal in the manufacture of artificial fuel.

1166. *Gutta Percha and Compounds thereof.* J. R. HUNT, Chichester Place, Wandsworth Road. Dated May 8, 1861.

THESE compounds are prepared by incorporating vulcanized India-rubber in a finely divided state with masticated gutta-percha in a doughy condition, employing the former in the proportion of about one-half by weight. In some cases it is considered desirable to mix in with the before-mentioned compound a small quantity of arrow-root, four ounces of the latter is prescribed as being sufficient for three pounds weight of the compound of gutta percha and India-rubber. These materials require to be very thoroughly incorporated before being rolled into sheet.

The uncertainty regarding the durability of some kinds of gutta percha may often be traced to the employment of foreign substances, and especially to the admixture of those which do not really evince any property of uniting with the natural gum. Starch, in the form of arrowroot, must surely be considered one of that class which would be prejudicial to the quality of the gutta percha.

1177. *Decoloration and Disinfection of Liquids.* J. H. JOHNSON, Lincoln's Inn Fields, London. Dated May 9, 1861. (Not proceeded with.)

THIS proposal refers to the employment alternately of chloride of lime and hydrochloric acid for the purpose of destroying the colour and objectionable odour of some kinds of oil. In applying this process the oil is introduced with a sufficient quantity of dry chloride of lime into a closed vessel fitted with a rotating vertical shaft provided with numerous inclined blades by which the oil is kept constantly agitated for the period of about half an hour; at the expiration of that time, the hydrochloric acid is admitted, and when the action of the chlorine is complete the oil is passed through fine strainers, and freely exposed to the oxidising influence of the air.

1192. *Treatment of India Rubber.* P. A. Godefroy, New North Road, Islington. Dated May 10, 1861. (Not proceeded with.)

In treating the India-rubber according to this invention the goods are to be immersed in a mixture of bisulphide of carbon and chloride of sulphur, removed from this solution, and dried by exposure to air. The rubber is next submitted to the action of Gallipoli oil to which sulphuric acid in the proportion of one-eighth has been previously added and intimately mixed by stirring. After remaining in this bath for about fifteen minutes it is taken out, rinsed in hot water, and placed in the boiler or steam chamber, where, according to the thickness of the rubber it is exposed to a temperature between the limits of 212° and 300° Fahrenheit, and left in the apparatus from one to four hours. The goods having undergone this process, are perfectly vulcanized and require only to be dried and finished as usual.

The bisulphide of carbon and the chloride of sulphur are most convenient agents for effecting the vulcanization of India-rubber; the use of olive oil would, on the other hand, be likely to deteriorate the goods by penetrating the substance of the caoutchouc and giving rise to the formation of a soft, unctuous compound with surfaces of an adhesive character.

Grants of Provisional Protection for Six Months.

3080. Marc Antoine François Mennons, Rue de l'Echiquier, Paris, "A new or improved combination of microscopic photographs and lenses with certain precious stones or imitations thereof."—A communication from Louis Henri Bouillette and Jean Amable Hyvelin, Rue Michel-le-Comte, Paris.—Petitions recorded December 9, 1861.

113. William Cleland, St. George's Hill, Everton, Liverpool, "Improvements in treating and utilising certain materials used and products obtained in the manufacture of gas, and in apparatus connected with the said treatment."—Petition recorded January 15, 1862.

123. Thomas Myers, Bloomsbury Square, London, and Edward Myers, Millbank Street, Westminster, "An improved composition for preventing rust on bright steel, iron, brass, or metal surfaces."

243. George Phillips, sen., and George Phillips, jun., Holborn Hill, London, "Improvements in the distillation and rectification of alcohols or spirits."

275. Friedrich Wilhelm Daehne, Swansea, Glamorganshire, "Improvements in furnaces used in the manufacture of zinc."

303. John Browning, Minories, London, "Improvements in aneroid barometers."

305. Edward Harrison, Oldham, Lancashire, "A certain compound, or certain compounds, to be used as a substitute for gunpowder."

312. James Pitkin, Clerkenwell, London, "An improvement in aneroid barometers."

320. John Tonkin, jun., Pool, Illogan, Cornwall, "Improvements in the manufacture of gunpowder."—Petitions recorded February 6, 1862.

Notices to Proceed.

2525. Thomas Tidmarsh, Dorking, Surrey, "An improved artificial manure."—Petitions recorded October 9, 1861.

2562. Frederick Burnett Houghton, Clarendon Terrace, Kensington, Middlesex, "Improvements in apparatus employed in reducing straw and other vegetable substances in the manufacture of pulp for making paper."—Petitions recorded October 14, 1861.

2574. Thomas Forster, Sparrow Hall, Streatham, Surrey, "Improvements in re-working waste vulcanised india-rubber."

2700. George Mowbray Gilbert, Albany Terrace, Britannia Square, Worcester, "Improvements in preparing blue colour, and in apparatus for applying such colour to water."—Petition recorded October 20, 1861.

243. George Phillips, sen., and George Phillips, jun., Holborn Hill, London, "Improvements in the distillation and rectification of alcohols or spirits."—Petition recorded January 28, 1862.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Pyrophoric Iron.—M. Carlet says (*Bull. de la Soc. Chim. de Paris*, No. 6, p. 112) is more easily prepared from calcined oxalate of the protoxide than from the sesquioxide ordinarily employed.

Action of Sodium Amalgam on Sulphide of Carbon.—M. Guignet (*Ibid.*, p. 111) has observed that when an amalgam of sodium with an excess of mercury is left in contact with sulphide of carbon for several days, the amalgam breaks up into small granules, and the whole of the sodium is acted on. On adding water to the mixture, a very deep blood-red solution is obtained. An acid precipitates yellow flocculi from this, which dissolve again in sulphide of sodium, and reproduce the blood-red compound. The red body is also soluble in alcohol. M. Guignet found small quantities of mercury in the compound, but as it had not been purified by crystallization, he is unable to say whether the mercury is an essential or accidental constitutent of the salt.

Atomic Weight of Lithium.—Dr. Karl Diehl has made in Bunsen's laboratory (*Annal. der Chem. und Pharm.* Bd. cxxi. s. 93) fresh investigations to ascertain the atomic weight of lithium. He obtained the following results:—7·023; 7·034; 7·012; 7·036; mean, 7·026.

Double Salt of Iodate of Soda and Chloride of Sodium.—Rammelsberg describes (*Bericht der Akad. d. Wissenchaft zu Berlin*, 1861, s. 893) crystals of a salt containing $2\ (NaO, IO_5) + 3\ (NaCl) + 18$ aq. He had previously described a salt containing one atom of iodate of soda, two of chloride of sodium, and twelve of water; but he thinks this formula may not have been correct.

The Sulphates of Cadmium, Didymium, and Cerium Isomorphous.—According to Rammelsberg (*Ibid.* s. 891), the above salts constitute a new isomorphous group exactly analogous in composition and form. They all crystallize in the oblique prismatic system.

II. ORGANIC CHEMISTRY.

Iodal.—M. Schoonbroodt (*Bulletin de la Soc. Chim. de Paris*, No. 6, p. 109,) formed a solution of hypochlorite of potash into an alcoholic solution of iodine until it was decolorized, and in that way obtained an abundant precipitate of white, silky flocculi, which he found to be soluble in water. The addition of a solution of potash to a solution of the flocculi split up the compound into iodoform and formic acid, so the author considers it *iodal* $C_4HI_3O_2$. It does not, however, look very much like the body described under that name by Gmelin.

Test for Caffein.—Schwarzenbach gives the following (*Chem. Centralblatt*, No. 62, 1861,) test by which the caffein extracted from a single coffee bean may be recognised. The caffein is evaporated to dryness with some chlorine water, by which means a purple red residue is obtained. This, when strongly heated, changes to a golden yellow colour, but after cooling, ammonia will instantly restore the purple red.

Crystalline Structure of Wax.—According to Decjordin and Böttger (*Neues Jahrb. für Prakt Pharm.* Bd. cxvi. s. 309,) wax can be seen to assume a crystalline form in the following way: A piece of bees'-wax is placed in an evaporating dish three-quarters filled with distilled water; the vessel is heated until the wax is perfectly fluid, and is then removed from the fire to cool slowly. Any bubbles of air in the wax must be got rid of by stirring with a hot iron spatula, so that it may have a per-

fectly clear surface. On watching the surface as it cools, solid points will be seen to form at the same moment at equal distances from each other, from which the crystallization starts, and soon spreads over the whole surface. The form of the crystals, say the authors, is the same as the honeycomb.

MISCELLANEOUS.

Royal Institution.—Tuesday, March 11, at Three o'clock, John Marshall, Esq., "On the Physiology of the Senses." Thursday, March 13, Three o'clock, Professor Tyndall, "On Heat." Friday, March 14, Eight o'clock, W. S. Savory, Esq., "On Motion in Plants and Animals." Saturday, March 15, Three o'clock, H. F. Chorley, Esq., "On National Music."

Use of Apples in Dyeing.—The following letter appears in the *Journal of the Society of Arts*:—"Sir,—There has been an opinion prevalent, especially in the West of England, that apples have been extensively used in the application of some of the new and brilliant dyes mentioned by Dr. Crace Calvert; and I have found many well-informed men in the City, as well as the majority of the members of the Society of Arts to whom I have mentioned the subject, fully believe it. You will, perhaps, not think it out of place to allow the *Journal* to be the medium of giving the assertion a distinct denial. Dr. Calvert has allowed me to use his name in stating that it is a complete hoax; but, to show the utility of giving publicity to this statement, he told me the rumour has obtained such general currency, that he has been called on in Manchester by persons who have been thus misled, and have involved themselves in some expense to supply the market with malic acid from some cheaper source, as, of course, may be easily done; but they have found, to their disappointment, that there is no market for the article.—W. Symons."

Enamelling Iron.—Enamelling iron is almost a new art. No metal is capable of receiving a coating of vitrified porcelain or enamel unless it is capable of withstanding a red heat in a furnace. Articles of cast iron, as a preparation for enamelling, are first heated to a low red heat in a furnace, with sand placed between them, and they are kept at this temperature for half-an-hour, after which they must be allowed to cool very slowly, so as to anneal them. They are then subjected to a scouring operation with sand in warm dilute sulphuric or muriatic acid, then washed and dried, when they are ready for the first coat of enamel. This is made with six parts, by weight, of flint glass broken in small pieces, three parts of borax, one of red lead, and one of the oxide of tin. These substances are first reduced to powder in a mortar, then subjected to a deep red heat for four hours in a crucible placed in a furnace, during which period they are frequently stirred, to mix them thoroughly; then toward the end of the heating operation the temperature is raised, so as to fuse them partially when they are removed in a pasty condition and plunged into cold water. The sudden cooling renders the mixture very brittle and easily reduced to powder, in which condition it is called frit. One part of this frit, by weight, is mixed with two parts of calcined bone dust, and ground together with water until it becomes so comminuted that no grit will be sensible to the touch when rubbed between the thumb and finger. It is then strained through a fine cloth, and should be about the consistency of cream. A suitable quantity of this semi-liquid is then poured with a spoon over the iron article, which should be warmed to be enamelled, or if there is a sufficient quantity the iron may be dipped into it and slightly stirred, to remove all air bubbles and permit the composition to adhere smoothly to the entire surface. The iron article thus treated is then allowed to stand until its coating is so dry that it will not drip off, when it is placed in a suitable oven, to be heated to 180 deg. Fah., where it is kept until all the moisture is driven off. This is the first coat; it must be carefully put on, and no bare spots must be left on it. When perfectly dry the articles so coated are placed on a tray separate from one another, and when the muffle in the furnace is raised to a red heat they are placed within it and subjected to a vitrifying temperature. The furnace used is similar to that used for baking porcelain. This furnace is open for inspection, and when the enamel coat is partially fused the articles are withdrawn and laid down upon a flat iron plate to cool, and thus they have obtained their first coat of dull, white enamel, called biscuit. When perfectly cool they are wetted with clean soft water, and a second coat applied like the first, but the composition is different, as it consists of thirty-two parts by weight of calcined bone, sixteen parts of China clay, and fourteen parts of feldspar. These are ground together, then made into a paste, with eight parts of carbonate of potash dissolved in water, and the whole fired together for three hours in a reverberating furnace; after which the compound thus obtained is reduced to frit, and mixed with sixteen parts flint glass, five and a-half of calcined bone, and three of calcined flint, and all ground to a creamy consistency, with water like the preparation for the first coat. The articles are treated and fired again, as has been described in the preparation coat, and after they come out of the furnace they resemble white earthenware. Having been twice coated, they now receive another coat and firing, to make them resemble porcelain. The composition for this purpose consists of four parts by weight of feldspar, four of clear sand, four of carbonate of potash, six of borax, and one each of oxide of tin, nitre, arsenic, and fine chalk. These are roasted and fritted as before described, and then sixteen parts of it are mixed with the second enamel composition described, excepting the sixteen parts of flint glass which is left out. The application and firing are performed as in the other two operations, but the heat of vitrification is elevated so as to fuse the third and second coats into one, which leaves a glazed surface, forming a beautiful white enamel. A fourth coat, similar to the third, may be put on if the enamel is not sufficiently thick. The articles may be ornamented like china ware, by painting coloured enamels on the last of the coats, and fusing them on in the furnace. A blue is formed by mixing the oxide of cobalt with the last named composition; the oxide of chromium forms a green, the peroxide of manganese makes a violet, a mixture of the protoxide of copper and red oxide of lead a red, the chloride of silver forms a yellow, and equal parts of the oxide of cobalt, manganese, and copper form a black enamel when fused. The oxide of copper for red enamel is prepared by boiling equal weights of sugar and acetate of copper in four parts of water. The precipitate which is formed after two hours' moderate boiling is a brilliant red. The addition of calcined borax renders all enamels more fusible.—*Engineer.*

ANSWERS TO CORRESPONDENTS.

Gun-Cotton.—J. S.—To make a highly-explosive and perfectly insoluble gun-cotton, mix together 10 fluid ounces of nitric acid, sp. gr. 1·6, and 13 fluid ounces of sulphuric acid, sp. gr. 1·84. Allow the mixture to cool, and immerse 400 or 500 grains of cotton in it for ten minutes. Wash as usual.

W. Kaye.—We do not know the exact address. Somewhere in the United States.

H. S. R.—Several different products may be left behind on burning gunpowder,—such as sulphide of potassium, or sulphate of potash. We cannot say what the globules are without an examination.

A Constant Reader.—Naphthaline and naphthalidine may be separated from benzol by distillation over a water bath. The benzol comes away, and leaves the other bodies, which do not distil except at a high temperature.

A Subscriber.—One shilling a month.

Books Received.—"On Pepsine." By M. Boudault. Translated by W. S. Squire, Ph.D., F.C.S. London: John Churchill.

THE CHEMICAL NEWS.

VOL. V. No. 119.—March 15, 1862.

THE SELECT COMMITTEE ON THE SEWAGE QUESTION.

THERE is another opportunity for an ingenious and enterprising Chemist. A Select Committee of the House of Commons is about to enquire into the possibility, or the best method, of utilising sewage. Whatever may be Mr. Brady's reason for moving for this Committee, it cannot be because the subject has hitherto received no attention. The Government referees on the Metropolitan Main Drainage Schemes, in 1857, went fully into the question. They employed Dr. Hofmann and the late Mr. Witt to make the necessary chemical investigations; and the elaborate report of these gentlemen constitutes a text-book of the chemistry of the subject. In the same year, Mr. Austin, one of the superintending Inspectors of the Board of Health, wrote a long report "On the Means of Deodorising and Utilising the Sewage of Towns." In the same year, too, Her Majesty, "reposing full trust and confidence" in several noblemen and gentlemen well known in various capacities, constituted them a Royal Commission, and gave them "full power and authority" to investigate the same subject. But in this case Her Majesty's trust and confidence seem to have been a little misplaced; for, after a year's delay, the Commissioners issued a "Preliminary Report," telling us nothing but what was well known before, and since then have been silent—the "Preliminary Report" apparently being a final one.

During the same time, the private investigators and speculators on the subject have been numerous. Many patents have been taken out for treating sewage in a variety of ways to make valuable manures; and the persevering Mr. Mechi and others have laboured hard to bring about the general adoption of a scheme for flooding the country with liquid sewage. But few, however, of these schemes are tried, or, if they are, they do not often pay. On a small scale, the irrigation plan has been found to answer. But the sewage of Edinburgh is too much for the meadows around that city, and whenever it rains—and it often rains in Edinburgh—the sewage is sent into the sea. What, then, can be done with the 100 millions of gallons which flow daily through the London sewers—enough almost to fertilise the Desert of Sahara?

It is the quantity with which our ingenious and enterprising chemist has to deal, that constitutes his great difficulty. True, if the valuable part were solid, there would be no great difficulty in filtering 100 millions of gallons, and saving the solid portion. But, unfortunately, six-sevenths of what is worth anything in sewage exists there in solution, and how shall 100 millions of gallons be evaporated every day? The lime process is said to be the best of the precipitation plans, but nobody will buy the manure produced in that way, even when made conveniently in the centre of a large agricultural district, as at Leicester; and how could it possibly be got rid of in London, produced at the rate of more than 2000 tons a-day?

We need not now speak of the charcoal and other plans for clarifying and filtering sewage, nor of Sir James Murray's process for (according to Hofmann and Witt) obtaining four pennyworth of ammonio-magnesian phosphate by materials costing 3s. 2d. It is enough that we call the attention of chemists to the present opportunity, and invite them to discuss the subject. Perhaps some one of our readers carries locked up in his bosom the secret of a cheap and effectual precipitant of ammonia and phosphoric acid in very dilute solution. If so, now is his time. Mr. Brady's Committee will make his fame and fortune, and we recommend him to send in his name without delay.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Colouring Matters Derived from Aniline, by Dr. A. W. HOFMANN, *F.R.S.*

(*Continued from page* 131.)

Rosaniline.—The most available substance for the extraction of this base is the acetate, which is generally employed in dyeing, and which Mr. Nicholson prepares perfectly pure. The boiling solution of this salt, decomposed by a large exeess of ammonia, yields a crystalline precipitate of a reddish colour, which consists of the base, in a state of great purity.

The colourless liquid separated by filtration from the precipitate, deposits on cooling perfectly white needles and crystalline tablets. This is absolutely pure rosaniline. Unfortunately, the solubility of the base in ammonia, or even in boiling water, is extremely slight, so that a very small quantity only of this compound is obtained in an absolutely pure condition.

Rosaniline is a little more soluble in alcohol; the liquid possesses a deep red colour; it is insoluble in ether. Exposed to the action of the atmosphere, the base rapidly becomes rose colour, and ends by taking a deep red tint. During this change of colour no sensible variation of weight is observable. At the temperature of 100° C., rosaniline loses a small quantity of adherent water; it can then be heated to 130° without change of weight; at a higher temperature rosaniline decomposes, disengaging an oily liquid consisting chiefly of aniline, and leaving a black carbonaceous residue.

A combustion of rosaniline led to the formula:—

$$C_{20}H_{21}N_3O = C_{20}H_{19}N_3,H_2O,$$

which was corroborated by an examination of several salts and characteristic derivatives.

Rosaniline is a powerful and well-defined base, forming several series of salts, almost all remarkable for their facility of crystallisation. The proportions in which this substance unites to acids assign to it the characters of a tri-acid triamine. Like many other triamines which I have examined, it appears capable of producing three classes of salts.

$$C_{20}H_{19}N_3,HCl,$$
$$C_{20}H_{19}N_3,2HCl,$$
$$C_{20}H_{19}N_3,3HCl.$$

At present, however, I have only succeeded in forming the representatives of the first and the third class. The tendencies of rosaniline are essentially mono-acid. The salts with one equivalent of acid are extremely stable compounds. I have crystallised them four or five times without altering them in the slightest degree. The salts with three equivalents of acid possess only a comparatively slight stability, being decomposed by the action of water, or by an exposure to a temperature of 100°.

On glancing at the above formulæ, it is evident that the white crystals of the base itself constitute a hydrate; the saline compounds of rosaniline, as may be seen on considering several of the processes giving birth to this base, do not contain oxygen. The salts of rosaniline may be obtained by two different processes, either by the direct action of the several acids, or by submitting the ammoniacal compounds of these acids to ebullition in the presence of an excess of the free base. These two methods furnish the salts equally pure and of the same composition.

The salts with one equivalent of acid for the most part present in reflected light the green metallic aspect of cantharides' wings. Seen by transmitted light, the crystals are red, becoming opaque when they are of any thickness. The solutions of these salts in water or alcohol possess the magnificent crimson colour for which this substance is famous. The salts with three equivalents of acid, are, on the contrary, of a yellowish brown, both in the solid state and in solution. They are much more soluble in water and alcohol than the mono-acid salts, which for the most part are comparatively slightly soluble. The two classes of salts of rosaniline crystallise easily, especially the mono-acid compounds. Mr. Nicholson has obtained several of them in perfectly defined crystals, which are at present in the hands of M. Quintino Sella, to be examined crystallographically.

Chlorides.—These compounds, and especially the mono-acid salts, have particularly served to determine the formula of rosaniline. Prepared either by the action of hydrochloric acid, or by means of chloride of ammonium, this salt deposits from its boiling solution in well-defined rhombic tables, often united together in the form of a star. The chloride is difficultly soluble in water, more soluble in alcohol, and insoluble in ether. This salts retains a small quantity of water at 100°, but becomes anhydrous at 130°. At this temperature it contains,

$$C_{20}H_{19}N_3,HCl.$$

Like most salts of rosaniline, it is very hygroscopic, a character which must not be lost sight of in analyses of these compounds. The mono-acid chloride dissolves more easily in moderately-diluted hydrochloric acid than in water. If the slightly warm solution is mixed with very strong hydrochloric acid, it solidifies on cooling to a mass of magnificent needle-shaped crystals of a reddish brown colour, which must be washed with concentrated hydrochloric acid and dried *in vacuo* over sulphuric acid and lime. Water decomposes them, reproducing the mono-acid compound. The salt obtained by the action of concentrated hydrochloric acid is the compound, with three equivalents of acid,

$$C_{20}H_{19}N_3,3HCl.$$

Heated to 100°, this salt gradually loses its acid; the brown crystals become of an indigo-blue colour, and if kept at this temperature until their weight becomes constant, the original green salt, with one equivalent of acid, is re-produced, as has been proved upon analysis. Possibly the blue coloration indicates the ephemeral formation of an intermediate di-acid compound. The two chlorides combine with bichloride of platinum. The compounds so produced being uncrystallisable, are not easily obtained in a state of purity. From the determination of the platinum, which has only given approximate results, I ascribe to these salts respectively the following compositions:—

$$C_{20}H_{19}N_3,HCl,PtCl_2,$$
$$C_{20}H_{19}N_3,3HCl,3PtCl_2.$$

Bromide of Rosaniline resembles in every respect the chloride. It is still less soluble than the latter. Dried at 130°, the salt contains

$$C_{20}H_{19}N_3,HBr.$$

Sulphate of Rosaniline is easily obtained by dissolving the free base in dilute boiling sulphuric acid. Upon cooling, the salt is deposited in green crystals, with a metallic reflection, which are rendered quite pure by one crystallisation. It is difficultly soluble in water, more so in alcohol, and insoluble in ether. At 130°, a temperature at which it loses a small quantity of water, the composition of this salt is,

$$\left.\begin{matrix}C_{20}H_{19}N_3,H\\C_{20}H_{19}N_3,H\end{matrix}\right\}SO_4.$$

The acid sulphate crystallises difficultly. I have not analysed it.

Oxalate of Rosaniline.—The preparation and properties of this salt are quite similar to those of the sulphate. The salt retains at 100° one equivalent of water, and at this temperature is represented by the formula,

$$\left.\begin{matrix}C_{20}H_{19}N_3,H\\C_{20}H_{19}N_3,H\end{matrix}\right\}C_2O_4,H_2O.$$

The water may perhaps be removed at a higher temperature, but the temperature at which it is expelled and that at which the oxalate commences to decompose are so near to one another that it is not very easy to obtain the salt in the anhydrous state. I have not succeeded in preparing an oxalate containing a larger proportion of acid.

Acetate of Rosaniline.—This salt is probably the most beautiful of the series. Mr. Nicholson has obtained it in crystals a quarter of an inch thick, which, submitted to analysis, have proved to be the mono-acid acetate in a state of purity,

$$C_{20}H_{19}N_3,HC_2H_3O_2.$$

The acetate is one of the salts most soluble in water and alcohol. It cannot be readily re-crystallised.

Formiate of Rosaniline is similar to the acetate.

Amongst the other salts which this base forms I may mention the *chromate*, which is obtained by the addition of bichromate of potash to the solution of the acetate, in the form of a brick-red precipitate, changing by the action of water to a crystalline green powder, almost insoluble. The *picrate* ought also to be mentioned. It crystallises in magnificent red crystals, likewise very difficultly soluble in water.

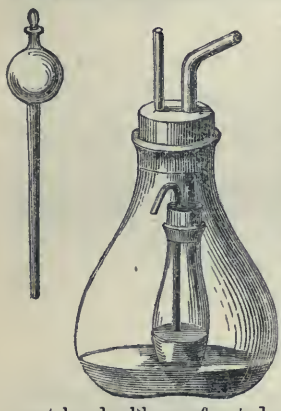
(To be continued.)

A Convenient Apparatus for the Estimation of Carbonic Acid, by EMERSON J. REYNOLDS.

THE little apparatus which it is my object in the present paper to describe, is one that I have continually used for

the last year and a-half in carbonic acid determinations with such good results as to induce me to bring it under the notice of chemists, in the hope that it may prove as useful to others as it has been to me. Since the introduction of Fresenius and Will's excellent and well-known arrangement for the indirect estimation of carbonic acid, numerous modifications have been proposed, but Fresenius himself, in speaking of these, says:—"I have not as yet been able to convince myself that any one of the modifications of our apparatus affords essential advantages." I do not pretend to say that, in general, my arrangement possesses "essential advantages" over that of the distinguished German chemists; but I have no hesitation in saying that, under certain circumstances, it will be found more convenient and equally accurate in its results. Moreover, I have not seen an arrangement similar to this one described in any of the books or periodicals with which I am acquainted, though the principle is so obvious that I can hardly conceive it possible that a similar proposition to mine has not been made before.

Description of Apparatus.—The annexed wood-cut will aid me in describing the arrangement which I use. A flask of the required size is taken and fitted with a perfectly sound cork, which is then bored so as to carry the two tubes shown in the cut. The next step is to prepare the little internal tube, which may be easily blown from a piece of moderately stout tubing, care being taken, however, to expand the body of the tube to such an extent as to admit of its only just entering the wide mouth of the flask, at the same time leaving the bottom in the shape of a rather acute cone. The tube, so prepared, should then be fitted with a cork, which must be doubly perforated, in order to receive the tubes shown in the wood-cut, the larger of the two tubes passing through the cork of the flask, thus establishing a communication between the external atmosphere and that within the small tube, at the same time serving as a means of suspension for the latter; the smaller of the two is formed from a piece of quill tubing which should be curved at one extremity, the other reaching to the bottom of the small tube flask, into the cone of which it should closely fit, without actually touching the glass on either side. All that now remains to be done is to fit the piece of quill tubing (closed at one end with a little wax or gutta-percha) into the cork of the larger flask, when the apparatus is complete.



Method of using the Apparatus.—In the analysis of the carbonates of bases which form soluble salts with sulphuric acid, the following method is adopted:—From ten to twenty grains of the carbonate to be analysed is introduced into the larger flask, together with a little water; the small tube-flask is next about two-thirds filled with concentrated sulphuric acid; the corks are then properly adjusted, and the apparatus wiped perfectly dry and then weighed. After the weight has been carefully ascertained, connect the end of the large tube with a chloride of calcium tube, and apply the lips to the open extremity of the latter, then force air gently into the apparatus, which, by pressing on the surface of the sulphuric acid, will force it out through the curved end of the quill tube. Some care is required in this part of the manipulation, as, if the drop of acid be allowed to fall immediately into the solution of the carbonate, too rapid evolution of gas would take place, but if the flask be so inclined that the drop may fall on the external curved surface of the little tube containing the sulphuric acid, the rate at which the gas is evolved is perfectly under control. The moment the gas is liberated it forces its way through the curved tube, and likewise through the sulphuric acid, in bubbling through which it is completely dried, this desirable object being facilitated by the way in which the bubble is given off from the end of the tube, as, having to strike against the conical bottom, it is thus made to present a larger surface to the sulphuric acid; the gas finally escapes through the wide tube into the atmosphere. The manipulation is thus continued, blowing over the acid guttatim until the carbonate is completely decomposed; when this is effected, a small additional quantity of acid is blown over, and the apparatus placed on the sand-bath, or in boiling water, until the temperature is sufficiently raised to expel all the gas held in solution by the water. The wax plug is now taken out of the piece of quill tubing carried by the cork of the flask, and suction applied at the wider tube, until the air ceases to taste acid. The apparatus is now allowed to cool, then wiped perfectly dry, and re-weighed; the loss in weight will indicate the amount of carbonic acid contained in a given weight of the sample under examination.

If it is desired to analyse the carbonate of a base which forms an insoluble salt with sulphuric acid, it is only necessary to replace the short tube plugged with wax by a bulb tube containing nitric acid; the latter is held *in situ* by atmospheric pressure, the upper part of the tube being stopped with wax. When the plug is removed, the acid flows out and decomposes the carbonate; the carbonic acid evolved has still to bubble through the sulphuric acid before escaping into the atmosphere. The remaining steps of the manipulation are precisely the same as in the former case.

The apparatus, if well made, and the proportions be properly adjusted, need not weigh more than from five to six hundred grains; though I have an arrangement at present which, when charged with the carbonate, acid, and water, weighs only about 470 grains.

For the accuracy of the results obtained by means of this apparatus I can vouch, as I have controlled them repeatedly by the analyses of perfectly pure carbonates of known composition, and my opinion has been confirmed by the testimony of both Professor Cameron and Mr. Tichbourne, Chemist to the Apothecaries' Hall of Ireland, who have carefully ascertained the capabilities of the apparatus before using it generally in their analyses.

Dublin.

Reaction of Ethyl Bases with Dr. Knop's New Hydrofluosilicic Acid, $2HFl + Si_2Fl_3$, by M. Carey Lea, *Philadelphia.*

In the *Chemisches Centralblatt* for August 21, 1861, Dr. Knop publishes an account of a very interesting combination which he has obtained by the action of peroxide of copper and metallic copper on silicofluoric alcohol, and subsequent removal of the copper by sulphydric acid. He thereby obtains an acid $2HFl + Si_2Fl_3$, which exhibits different properties from ordinary hydrofluosilicic acid $3HFl + 2SiFl_3$, and which he thinks may prove a valuable

re-agent. It precipitates, according to its discoverer, potash and soda completely from their solutions, while with sulphate, chlorhydrate, phosphate, and oxalate of ammonia, it gives no precipitate. It appeared to me to be of interest to examine the behaviour of this re-agent with some of the ethyl bases. I prepared some by Dr. Knop's process; with it obtained immediate precipitates with carbonated and caustic alkalies, hydrochlorate of ammonia afforded no precipitate—results in accordance with his.

Caustic ammonia afforded the following results:—When the acid was in excess no precipitate was formed, but where the ammonia was in excess a very abundant caseous precipitate was obtained, which showed little disposition to dissolve in an excess of precipitant, even by standing and with the application of heat. Chlorhydric acid dissolved it somewhat better, but still left a considerable portion undissolved.

Ethylamine, when the acid was in excess did not give a precipitate, but when the ethylamine was in excess, the mixture, by standing awhile, coagulated to a jelly so stiff that the test-glass could be inverted, and this with scarcely any loss of transparency. In an excess of the new acid, this jelly re-dissolved, with the exception of a few flakes.

Diethylamine exhibited almost the same reaction as ethylamine, except that the jelly was less transparent, and the solution in excess of the re-agent less complete. The acid gives no precipitate with either the chlorhydrate of ethylamine or that of diethylamine. In this respect, therefore, these ethyl bases resemble ammonia. —*American Journal of Science.*

On Crystallised Platinum, by Dr. T. L. Phipson, *F.C.S., &c.*

When a plate of platinum is left for about two months in a mixture of nitric and hydrochloric acids in a moderately warm place, the surface of the plate has, at the end of that time, become perfectly crystalline, whilst a certain quantity of the metal has been dissolved. On examining the surface with a proper magnifying power, it is seen covered with an infinite number of crystalline scales, some of which lie flat upon the surface, others being elevated above it; they all appear to be of the tetrahedron or octahedron form. The only mention I find of crystallised platinum is in Berzelius (*Traité de Chim.*), who says that some of the small grains of native platinum occasionally shows traces of crystallisation.

On the Absorption of Carbonic Oxide by Alkalies, by M. Berthelot.

Solid alkali, carbonic oxide, and the substance, the influence of which is to be studied, are placed together in a tube over mercury; the carbonic oxide is replaced as absorbed.

Formic acid always remains the principal and, most frequently, the only product of the reaction.

1. Potash and Water. — The absorption is the more rapid as the potash is present in larger quantity. In the presence of a sufficient quantity of potash all the carbonic oxide may be absorbed in six weeks.

2. Potash and Alcohol.—The absorption proceeds ten or fifteen times more rapidly than in the presence of water. At 100° an analogous difference is noticeable. This difference is connected with one of the same character between the solubilities of carbonic oxide in the two solvents, for this gas is seven or eight times more soluble in alcohol than in water.

If potash and absolute alcohol be used, or better, soda alcohol, the formation of the formic acid is accompanied by that of a very small quantity of propionic acid.

3. Potash and Wood-spirit.—The absorption is also hastened in this case.

4. Potash and Amylic Alcohol. — The same observation, only the absorption is slower than with the other two alcohols. The difference rises to about one-half. It is perhaps due to the viscosity of amylic alcohol.

5. Potash and Glycerine. — The absorption is much slower than with water, which is doubtless due to the viscosity.

6. Potash and Ordinary Ether.—The absorption is more rapid than with any other substance.

7. Potash and Compound Ethers.—Acetic ether is without any marked influence; with nitric ether the absorption is more marked than with water, which is no doubt due to the formation of ordinary ether.

8. Soda and Alcohol.—Soda behaves like potash.

9. Lime and Alcohol.—The absorption does not appear more rapid than in the presence of aqueous potash.

10. Baryta and Alcohol.—Alcohol hastens the absorption of carbonic oxide by baryta; this absorption is then very marked.

11. Baryta and Wood-spirit.—The influence of wood-spirit is the same as that of alcohol.

12. Baryta and Ether.—The absorption is at first hastened if the ether contains water; it afterwards stops, owing to the absence of the water necessary for the reaction.

An alcoholic solution of ammonia does not absorb carbonic oxide at the ordinary temperature.—*Annales de Chimie et de Physique*, lxi., 463.

On the Quantitative Determination of Lead in Chemical Analysis, by M. A. Levol.

The precise estimation of lead, though presenting no serious difficulties, nevertheless demands precautions sufficiently minute. The object of this paper is to state the results of my own observations on this subject.

The Quantitative Determination of Lead in the State of Sulphate.—The estimation of lead in the state of sulphate, by means of sulphuric acid and evaporating to dryness, insures accuracy, but the process requires constant attention. Towards the end of the analysis the evaporation exposes it to loss by projection; moreover, if the liquids contain iron, the sulphate of lead is often contaminated with the slightly soluble ferric sulphate. The solubility of sulphate of lead, even in water, is well known, as the following experiment shows:—Precipitate one equivalent of nitrate of lead by two equivalents of sulphuric acid diluted largely with water; then wash during several days, and long after the washings have ceased to redden litmus-paper, they will still become slightly turbid by nitrate of baryta and hydrosulphate of ammonia.

The *use of soluble sulphates*, suggested by various authors, is not to be recommended, as will be shown.

My opinion was, that the principal inconvenience arose from the incomplete insolubility of sulphate of lead, and that, consequently the employment of alkaline sulphates would produce but imperfect results. I was

then much surprised to find, under such circumstances that the fact could not escape notice, an overweight, in precipitating lead by sulphate of potash. If, in fact, liquids much charged with nitrate of lead and sulphate of potash or soda in excess are put in contact, precipitates are obtained, the weight of which considerably exceeds that of the sulphate of lead, corresponding to the weight of nitrate, and it is with difficulty that they are reduced to this weight by washing.

I will state some of the results:—5·1775 grammes (= 1 equivalent) of nitrate of lead, added to 5·455 grammes (= 2 equivalents) of sulphate of potash, yielded a precipitate which simply drained, then dried at 110° and weighed on the filter yielded 7·355 grammes, instead of 4·739 grammes. This precipitate was fusible, and to separate from it the sulphate it contained, no less than ninety washings in cold water were required.

An experiment made with nitrate of lead and sulphate of soda yielded 5·325 grammes of drained precipitate, instead of 4·739 grammes; and to extract from it the sulphate of soda thirty-six washings in cold water were necessary.

It seems evident that in both cases there must have been a formation of double sulphates, while that of potash appears to have been by far the most stable, the overweight of sulphate of potash being 2·616 grammes. (The formula $PbO.SO_3 + KO.SO_3$ would require 2·686 grms.) With the sulphate of soda, the overweight = 0·586 gramme, which represents about one-third of the equivalent. The formula $PbO.SO_3 + NaO.SO_3$ requires 1·764 grammes.

After the prolonged washings which I have mentioned, the sulphate of lead precipitated by sulphate of soda weighed 4·640 grammes, the loss by washing having been 0·099 gramme.

The previous precipitate = 4·585 grammes.
Loss by washings = 0·154 „

Independently of less stability, this difference seems to indicate greater solubility of the double sulphate of lead and soda, which is further rendered apparent by testing the washings by means of baryta and hydrosulphate of ammonia (apart from the solubility proper to the sulphate of lead, which I have taken into account in this instance), by comparing with it a third washing of sulphate of lead formed by sulphuric acid and nitrate of lead, equivalent to equivalent.

It appears, then, I repeat, that there are formed by the wet way, under certain conditions, double sulphates of lead and potash, or soda. I have found no similar salts mentioned in any French work; but in a paper, published in 1825, by Tromsdorff, it is pointed out that the potassic salt is obtainable by the precipitation of acetate of lead by sulphate of potash. The author adds, that by boiling this salt with a large quantity of water, the proportion of sulphate of potash which it contains gradually diminishes.

It is only under particular conditions of concentration of the liquids that these salts can be formed, and I am certain that the estimation of sulphates by means of standard nitrate of lead, which I have pointed out (*Bulletin de la Société d'Encouragement*, 1853), are not in any way affected by it. The mode of estimation would be out of place here.

On the whole, then, experience shows that alkaline sulphates should not be applied to the estimation of lead in the state of sulphate, by weighing the precipitate, partly because of the danger about to be described, and partly because of the fear of loss of sulphate of lead, by the numerous washings necessitated by the decomposition of the double salts by water.

Determination of Lead in the State of Carbonate.—In face of the difficulties to be encountered in estimating lead with great precision, it seems to me highly to be recommended that it should be determined in the state of carbonate, if for that purpose ordinary carbonate of ammonia, to which is added caustic ammonia, is used. The object of this addition is to avoid the employment of too large a volume of solution of carbonate of ammonia, a salt not very soluble in water. The ammonia forms, with nitrate of lead, for instance, a very incomplete precipitate, the composition of which is represented by $2(6PbO.NO_5) + 3HO$. It would not, then, be prudent, to divide the operation into two,—that is to say, by employing ammonia first to saturate the liquid,—and, consequently it should not be poured in until it has been charged with carbonate of ammonia, which it dissolves abundantly and easily. The precipitate separates perfectly from the liquid, is easily collected and dried on a filter. The deposition of the precipitate is completed in about twenty-four hours, especially under the influence of gentle heat. According to my experience two or three thousandths of lead can be estimated by this process.

The precipitate, which is anhydrous, $PbO.CO_2$, is deposited on a small double filter, each one of the same weight.

If, as frequently happens, in analysing metallic substances, the colour, which should be pure white, is yellowish, it is owing to the presence of iron, which is easily got rid of by washing the filter with water acidulated with sulphuric acid after weighing.

If there is reason to suspect the presence of bismuth, treat a small quantity of the weighed precipitate by a little nitric acid. A few drops of iodide of potassium in the liquid will detect the presence of bismuth by the forming of a brown precipitate, or yellow-brown if there is bismuth and lead The latter metal, when present alone, gives a pure yellow precipitate.

Determination of Lead by Oxalic Acid.—I have stated that, in estimating lead by carbonate of ammonia, in presence of an excess of ammonia, two or three thousandths of this metal can be determined. By operating, *under the same conditions*, with oxalic acid, I have found it impossible to determine it to less than 1 per cent. Writers have, in truth, observed, that the precipitation of lead by oxalic acid should be effected in neutral liquids; but I would remark that this necessity but ill agrees with the most ordinary instances of the analysis of metallic substances, where the presence of an excess of ammonia is indispensable for maintaining in solution certain substances from which the lead should be separated.—*Répertoire de Chimie Appliquée*, iv. 21.

Generalisation of the Acidimetric Method, by MM. E. Langer and R. Wawinkiewicz.

M. Bunsen has proposed an acidimetric method which admits of estimating by standard solutions acids combined with the bases which are completely precipitated by caustic or carbonated alkalies. This method consists in adding an excess of the alkaline standard liquid to the solution of the salt, separating the precipitate by filtration, and determining the excess of alkali in the liquid by means of an acid liquid of a known standard.

The authors have assayed the application of this process in a great number of salts; the results obtained are in general very close to those required by theory.

The standard solutions employed were caustic potash, carbonate of soda, and hydrochloric acid.—*Annalen der Chemie und Pharmacie*, cxvii., 230.

On Crystallised Manganate of Soda, by M. J. C. GENTELE.

A CRUCIBLE is filled with a mixture of equal parts of powdered peroxide of manganese and crude nitrate of soda, and exposed to the temperature of a pottery furnace during the time occupied by one baking; upon cooling, it contains a fused black mass. This mass, powdered and extracted with boiling water, leaves a residue having the appearance of hydrated peroxide of iron. The liquid, filtered through fragments of glass and allowed to cool, partially solidifies to a mass of almost colourless crystals, having the acicular form of sulphate of soda, and containing

$$NaO, MnO_3 + 10\,HO.$$

When dissolved in water, they partially decompose, forming a green solution. The crystals can only be dried on bricks or porous porcelain; they immediately decompose in the presence of organic matter.—*Journal für Praktische Chemie*, lxxxii., 58.

TECHNICAL CHEMISTRY.

Chemical and Physical Modifications of the Atmosphere Consequent on Habitation.

THE repeated observations of chemists have taught us to regard the identity of composition of the atmosphere as a fixed law—one to which no exception is to be found in Nature, unless it be in the neighbourhood of tropical rivers, where vast quantities of organic matter, the *débris* of a luxuriant vegitation, are rapidly passing into decomposition[1]. Everywhere, whether collected on the top of Mont Blanc, or on the banks of the Seine or Thames, or in the middle of the Atlantic, the two main constituents of the atmosphere are found in precisely the same proportion, and the more perfect the processes of analysis have become, the firmer has the constancy of this relation been established[2]. This fact has always, however, been rebelled against by the common experience of mankind; it has been almost an opprobrium to science that, in spite of the manifestly different feeling of the air on the Swiss mountain, and in the middle of London, the chemist can detect no difference in composition. During the last few years several chemists have directed their attention to this apparent inconsistency between the organoleptic and physical characters of the air with special reference to the condition of the atmosphere in towns. These researches have related mainly to the quantity of carbonic acid, and other products of combustion, and to the existence of organic matter in suspension. Among the most important are those of Dr. Dundas Thomson and Dr. Angus Smith.

The per-centage of carbonic acid usually existing in the air of London was found by Dr. Roscoe[3], to be 0·037 per volume, a result not differing materially from those obtained by Dumas and Boussingault in Paris. The analyses on which these are based were made by passing a known volume of air over weighed tubes containing alternately pumice-stone steeped in sulphuric acid and potash, a method which leaves nothing to be desired in respect of accuracy[4]. Dr. Smith's estimations of the carbonic acid of the air of Manchester, made by the same method, gives somewhat higher results. He found that on a windy day they averaged 0·045 to from 0·08 per cent., and that on a still day the per centage amounted to 0·12. When, however, we consider that although London is the greatest city in the world, Manchester is the largest manufacturing town, and that it is the centre of a manufacturing district comprehending many hundred square miles, over which an atmosphere darkened by smoke perpetually hangs, we are not surprised to find that the products of combustion exist in larger proportion than in London or Paris. Dr. Smith has calculated from the quantity of coal burnt in the neighbourhood of Manchester, that 15,000 tons of carbonic acid must be introduced into the atmosphere daily, without taking into account the quantity expired by man and animals.

A much more important product of combustion is derived from the oxidation of the sulphur contained in coal, and the introduction thereby into the atmosphere of sulphurous and sulphuric acids. In the researches undertaken by Dr. Thomson[5] during the last epidemic of cholera, which consisted in passing large quantities of the air of London through distilled water, it was found that such air invariably possessed an acid reaction, and that this reaction was due to sulphuric acid. Dr. Smith has further investigated this question, and has found that in Manchester the acid reaction of the atmosphere is much more constant and intense than in London. Blue litmus paper becomes red in half an hour, and sometimes in ten minutes when exposed to Manchester rain, and occasionally its acidity is such that a single drop is sufficient to effect the reaction. The actual quantity, however, is exceedingly small; of a solution containing a thousandth part of its weight of carbonate of soda, quantities varying from ten to fifty grains suffice to neutralize 1000 grains of such rain; and as much cistern-water is found to be neutralised by twenty-five grains; from which results Dr. Smith concludes that the largest quantity of sulphur acids existing in the atmosphere of the town does not exceed 0·004 per cent. by weight, a proportion amounting to not more than a twentieth part of that of the carbonic acid. As to the

[1] Daniell: *Philosophical Magazine*, Fifth Series, No. 121.

[2] Dumas and Boussingault: "Récherches sur la véritable Constitution de l'Air;" *Ann. de Chimie et de Physique*, troisième série, tome iii., p. 257. 1851.

[3] Roscoe: "On the Atmosphere of Dwelling-houses;" *Quarterly Journal of the Chemical Society*, October, 1857, p. 259.

[4] The researches of Hlasiwetz in 1856 (*Berichte der Wiener Akad. d. Wissens.*, vol. xx., p. 189) have shown that the sulphuric acid used for the absorption of the moisture, on the one hand, absorbs considerable of the carbonic acid from the air passed over it, which circumstance causes considerable variation in the results. On the other hand, the caustic alkalies which are employed for the absorption of carbonic acid, absorb considerable quantities of atmospheric oxygen. One should suppose that the improvements made of late years in the volumetric analysis of carbonates, which give results in every respect more accurate and expeditious than those obtained by weighing small quantities of gases in large apparatus; would deserve more extended application in the analysis of the air.—Dr. Hugo von Gilm, under the guidance of Hlasiwetz (*Berichte d. Wien. Akad.*, vol. xxiv., p. 279), and D. Pettenkofer have contrived methods which leave nothing to be desired in this respect. It was also noticed, that when ozonised air was conducted through weighed tubes containing chloride of calcium, ozone displayed an equivalent proportion of chlorine, which being four and a half times heavier than the ozone, must necessarily cause very great variations in the result. This error is likewise avoided by a previous volumetric determination or absorption of the active oxygen. A further source of error in the determination of the amount of water in the air by means of sulphuric acid is the ammonia present. From all of this it will be seen that the examination of air by passing it through a series of weighed tubes containing alternately various absorbents can only give results which, to say the least, are very doubtful.

[5] Appendix to "Report of the Committee for Scientific Inquiries in Relation to the Cholera Epidemic of 1854," p. 110.

share of sulphurous and sulphuric acids respectively in this total, it is of course impossible to arrive at a conclusion; but considering what we know of the rapidity with which the former is oxidised in the air, it is to be supposed that whenever the acidity of the atmosphere is marked, it will be mainly owing to the latter. The impregnation of the rain with the mineral acid must be regarded as rather beneficial to health than otherwise, as tending to retard the putrefaction of animal matter on which it falls.

Dr. Dundas Thomson appears to have been among the first to recognise the importance of organic matter as a constituent of the air of towns, and to express the conviction that the gaseous products evolved during putrefaction are not the main sources of danger. Proceeding on this idea, he subjected a large quantity of atmospheric air to chemical investigation, "with a view of condensing any vapour, or detaining solid particles, which might be disseminated." The result was entirely negative[6]. Further inquiries of the same kind were made, under the sanction of the Board of Health, in 1854, the air being passed, as has already been mentioned, through distilled water, the result invariably being that hyphaceous fungi made their appearance in the water, and in a short time, by their rapid growth, pervaded the whole of it, so as to be evident to the unassisted eye[7]. It was also found, that on passing the air through sulphuric acid in the same manner, the acid became soon dark coloured, in consequence of the charring of the organic matter introduced into it. Dr. A. Smith has worked out the idea much more completely. He has preferred a chemical to a microscopical test for the detection of the suspended organic matter. It consists, as most of our readers may be aware, in passing the air through a very dilute solution of permanganate of potash, the strength of which is determined by ascertaining how much is required to decompose a solution of a weighed quantity of oxalic acid, or of uncrystallisable sugar. This test obviously indicates, not the quantity of organic matter, but the quantity of oxidisable matter in the atmosphere, and hence it is only valuable in so far as we may assume that the atmosphere contains no reducing agent. Thus, in the presence of sulphurous acid, it would be of little value were it not that the agent exists, even in the town air, in exceedingly small quantity. Many of Dr. Smith's results are of such a nature as to be beyond the possible limits of this source of error. It was found that the same quantity of the solution of permanganate which was decolorised by one bottle of air obtained in a close court in Manchester, required twenty-two bottles to decolourise it on the hills in the neighbourhood. Assuming that sugar and the organic matter of the air are decomposed by the same amount of manganate, "a supposition which cannot be perfectly true, but which, from the minuteness of the amounts, leaves no room for a great error," Dr. Smith concludes that whereas, on the high ground north of Manchester, there existed but one grain of organic matter in 200,000 cubic inches, in close places in the town there was a grain in 8000 cubic inches. From his most recent observations he concludes that we have, "in different air breathed by people in the same country, a substance the amount of which in one case is twenty-two times greater than in the other, and in air breathed by people in the same town a difference which is as 9 to 22."

The whole importance of these investigations, regarded from the point of view of preventive medicine, lies in their relation to the putrefactive process. To discover a means of seizing upon and estimating putrid exhalations—under which term we include everything not gaseous that is disengaged into the atmosphere from the surface of living animals, no less than the exhalations from dead animal matter—would be certainly a most important step towards acquiring a more satisfactory knowledge of the influence of habitation on health. We have, therefore, to inquire what grounds there are for regarding Dr. Smith's test, or any other founded on a similar reaction, as affording a solution of this problem. It is not difficult to satisfy ourselves that animal matter in putrefaction does disengage from its surface portions of its substance, of sufficient tenuity to be suspended in the atmosphere. Without referring to offensive smells, which of course must be material, we have several satisfactory proofs. If a bell-glass be inverted over decomposing animal matter in a moist condition, the inner surface of the glass becomes in a few days bedewed with moisture, which on being examined under the microscope is found to contain the same filamentous fungi to which reference has already been made; and on evaporation it leaves a residue, which is blackened by incineration. Similarly we find that the moisture which is deposited in glutinous drops on the sides and arched roofs of sewers, is rich in organic matter, which must clearly have been derived from the air of the sewer. Dr. Smith has related the results of experiments showing that air kept for a length of time in contact with putrescent matter, becomes loaded with oxidizable material, and acquires the power of decomposing a correspondingly large quantity of permanganate of potash.

Another group of facts shows us that the existence of putrescent impurity in the air is a principal, though not a necessary, condition of the induction of putrefaction in bodies susceptible of the change. Thus, for example, I have found that milk which retained its freshness for hours, will at once turn on being exposed to a putrid emanation. Butchers are familiar with the fact, that meat cannot be successfully dressed in the neighbourhood of a stinking gully-grate, or of a stable reeking with ammonia; and for the same reason, every intelligent butcher keeps his slaughter-house in a state of scrupulous cleanliness. It is not, however, to be forgotten that other causes, possibly electrical, the nature of which is still involved in obscurity, have a still greater influence in inducing putrefaction. Thus, in this country, the butcher finds that on one day he is able to slaughter and dress even veal or lamb with safety; whereas on another, not differing in temperature, incipient putrefaction may render the carcass unsaleable, in spite of the most careful precautions; butchers are apt to believe that this occurs mostly on calm days when the air feels heavy. Still more remarkable are the facts recorded respecting the slaughtering of cattle in hot countries; the operation can only be safely performed when the air is clear, and the sky cloudless. Under such circumstances, we are told that the appearance on the distant horizon of a cloud "like a man's hand," the sure precursor of a storm, is a sign to the slaughterers on the Pampas of South America to desist from their work, for it is immediately followed by rapid putrefaction.

Air contaminated with putrescent matter is for the most part alkaline. Thus the air of sewers is invariably so, as has been proved by the experiments of Dr. Dundas Thomson, its alkalinity being owing partly to ammonia, partly to the sulphuret of ammonium, the form

[6] "Chemical Researches on the Nature and Causes of Cholera;" *Medico-Chirurgical Transactions*, vol. xiii. 1859.

[7] "Report on Cholera," p. 127.

assumed by the sulphur disengaged in the composition of fæcal matter. The air of stables and stable dwellings is strongly alkaline, as everyone in attendance on the sick poor in London well knows; and the air expired by men and animals, although at first probably acid, becomes alkaline by putrefaction. The relation between putrefaction and the existence of ammonia in the air is therefore so close, that the detection of this body may, under ordinary circumstances, be regarded as a proof of its existence.

Bleaching Power of the Air.—It is known to those who are commercially engaged in air-bleaching, that the bleaching power of the air varies very considerably, not only according to the season and time of day, but irrespectively of such periods. It has not yet been determined what is the relation between this reaction and the oxidizing power of the air, as exhibited in its power of decomposing iodide of potassium. But the remarkable experiments of M. Houzeau (*loc. cit.*), made simultaneously in town and country, have shown that country air bleaches much more rapidly than town air, and that in this respect the difference is no less marked than in that of the well-known ozone reaction, which has been so clearly shown to be destroyed by urban contaminations.

The pressure, temperature, and moisture of the air, are but little modified by habitation. The readings of the barometer and hygrometer in town and country do not differ. There is, however, one respect in which the temperature of great towns may be favourably compared with that of the country—viz., in that of equability. This subject has been very fully illustrated by Mr. Glaisher,[8] in his Report on the Meteorology of London during the year 1854; and more recently by Professor Hennessy, in a paper read last year to the Association for the Promotion of Social Science. Mr. Glaisher found that in the middle of London the night temperature is much higher, and the day temperature considerably lower, than in the country, and consequently that the range of daily temperature is nearly twice as great in the country as in London, especially in clear weather, when the cloudless sky of the country contrasts with the smoky obscurity of town. This immunity from great variations of temperature must tend to diminish, though probably it does not at all counterbalance, the generally injurious effects of town air on persons affected with chronic pulmonary disease.

In the preceding paragraphs we have reviewed all the differences which are discoverable, either by physical or chemical means, between the atmosphere of towns and that of the country; and we are in a better position to determine, in the light of physiology, which of these conditions is likely to exercise most influence on the health of man. As regards the existence of an excess of carbonic acid, it is clearly of no importance whatever; for in many large towns no such excess is met with. Sulphurous and sulphuric acids, if they have any influence, must act as "colytics"—*i.e.*, as agents tending to arrest putrefactive change. The absence of sunlight, on which the more equable temperature of towns depends, has unquestionably an unfavourable influence, but one which is very limited. We are driven then to the only difference which remains—viz., that which depends on the existence of oxidizable matter, as indicated by its power of reducing certain metallic oxides.—*British and Foreign Medical Review.*

[8] Report upon the Meteorology of London. Report on Cholera Epidemic, 1854, p. 21.

PHARMACY, TOXICOLOGY, &c.

On the Behaviour of Essential Oils to Iodine and Bromine, by JOHN M. MAISCH.

(*Continued from page* 132.)

Oleum Hedeomæ.—Brownish yellow, thin.

Iodine.—Brisk reaction with explosion, and violet red and grey vapours; the residue is a purely brown and viscous liquid.

Ether sol. iodine.—No spreading, some effervescence; the supernatant liquid is thin, transparent, of iodine colour, the sediment thick and dark; after six hours, the whole is of a uniform tar-like consistence.

Bromine.—Vigorous detonation, with white fumes; the residue consists of a heavy yellow liquid, and a lighter one which is scarcely coloured: they are miscible to a very pale, yellowish, thin oil.

Ether sol. bromine.—The heavier liquid assumes a purplish brown colour; the supernatant fluid has a faint brownish tint; the whole is not miscible. By exposure to the air, and consequent evaporation of the upper stratum, small purplish circles are formed on the surface, the sediment is rendered of a more yellowish brown.

Oleum Lavendulæ veræ.—Thin, almost colourless.

Iodine.—Quick and energetic explosion, much heat and many iodine vapours, the strongest reaction of any oil of the labiatæ. The yellowish brown residue is a soft extractive mass of a pungent acidulous balsamic odour. Z.

Brisk reaction, with detonation and reddish vapours; the yellowish brown liquid is miscible with the darker coloured sediment, to a viscous brown mass of an acidulous aromatic odour.

Ether sol. iodine.—Miscible to a purplish iodine-coloured liquid; the purple shade gradually disappears, is afterwards rendered brown.

Ether sol. bromine—The mixture is of a light greenish colour, darkening, afterwards of a deep sea-green; some aqueous drops separate at the bottom, which are of a light brown colour.

Oleum Limonis.

I regret very much that I omitted to test a fresh oil of lemon. I had a specimen in my possession which was at least six years old, and had been kept for several years in a half-filled vial without opening it; it was of a pale yellowish colour, about the consistency of oil of sweet almonds, and had a peculiar resinous lemon odour. Knowing it to be unadulterated, I was desirous of applying the various tests to it and to compare its behaviour with that of the fresh oil; but I omitted it at the time, and never thought of it again until I came to prepare this paper, when my time was too much occupied to admit of making these experiments. Wherever in this paper I have used oil of lemon, it was the old thick article referred to.

Iodine.—Brisk fulmination, less heat than with oil of bergamot; the residue consists of a viscid mass and a thin liquid, which after some time are miscible to a uniform soft, somewhat liquid mass, with a modified odour. Z. (This reaction refers to the fresh oil.)

The reaction of iodine is a slow radiating motion, without evolution of fumes; the residue is a dark brown resinous mass and a yellow liquid, after a while miscible to a soft sticky mass.

Ether sol. iodine.—Miscible, with some flapping

motion to a uniform yellowish, afterwards more brownish iodine-coloured liquid, with but a minute sediment; the oil thickens, but retains, after some hours, a tar-like fluidity.

Bromine.—Explosion and white fumes; the residue is of a beautiful yellowish red viscid liquid, and a supernatant one of a pale yellow colour, which, in a short time, are miscible to a thin pale yellow oil; odour little modified.

Ether sol. bromine.—Brisk reaction in the centre, around the ethereal solution; at the conclusion of the reaction, the liquid is of a reddish yellow colour; and separates into a lower stratum of a greenish colour and a lighter fluid, which is reddish white and milky.

Oleum Bergami and Limonis, equal parts.—The ethereal solution of bromine causes an irregular motion towards the circumference; the colour is a light brownish yellow, after the reaction of a greenish yellow brown.

Oleum Matricariæ.—Greenish blue, about three years old, well preserved.

Iodine.—Oil from fresh flowers, two years old, evolved a few reddish vapours, with little heat, the blue colour changed through greenish into brown; the residue is tenacious. The residue from the reaction with old oils distilled from dried flowers, is soft, extractive. An oil six years old showed no heat or vapours, but yielded an extractive mass, which, after an hour, could be removed from the glass, as if it had been greasy. Z.

No reaction and no solution is observable, unless the dish is gently heated, when few red vapours are given off, the colour changes to brown, and the consistence to that of extract.

Ether sol. iodine is miscible with iodine colour, afterwards thickening to a black semi-fluid mass, which in thin layers shows a green yellowish colour.

Ether sol. bromine.—Miscible, the colour is green, tinged with brown, the oil is not thickened.

Oleum Matricariæ citratum.—(Pharm. Barrus.) Greenish blue.

Iodine.—Brisk, detonating reaction; the liquid has a reddish brown colour, and contains a dark sediment.

Ether sol. iodine is readily miscible to a yellowish red brown thin liquid, containing a sediment of a nearly black colour.

Ether sol. bromine, though mixing at first with the oil, soon separates into a greenish yellow thin liquid, and a thicker oily sediment of a deep green colour; an excess of the bromine solution has little influence on the supernatant fluid, except by tinging it brown; but the heavy oil is rendered of a greenish brown colour; the two strata are not miscible.

Oleum Melissæ.—Pale yellow, thin, three years old, well preserved.

Iodine, one grain, with two drops of fresh oil, evolves many reddish and grey vapours; the iodine dissolves quickly with considerable heat to a soft extract. Z.

Two grains to six drops produce radiating motion, with scarcely any vapours; the solution has an iodine colour, and is thickened.

Ether sol. iodine.—Spreading; mixing to a light red iodine coloured solution; in six hours, the mixture had a dark brown colour, was of a pitchy consistence, and its odour reminded strongly of jessamine, while by heating, it was altered to an empyreumatic melissæ odour.

Ether sol. bromine.—The oil turns immediately to a bright yellow, and a few white vapours are given out; a quick separation into two layers takes place, the lower stratum assumes a brown colour, the supernatant is yellow, clear. If a larger quantity of the bromine solution is added, the upper layer is soon changed to brownish green, while in the lower liquid, some undecomposed bromine solution is for some time observed in distinct globules within the brown colour.

Oleum Melissæ four parts, Oleum Limonis one part.—This mixture shows a similar reaction to the former, with the ethereal solution of bromine; but the colour deepens in a shorter time, and the supernatant liquid shows a brownish yellow colour, and is slightly cloudy.

Oleum Menthæ piperitæ Americ.—Crude oil, two years old, yellowish, thin.

Iodine shows a slow action, no vapours or heat; the whole is miscible to the colour of burnt terra de sienna, afterwards alters to a blackish brown viscous liquid; the odour is somewhat altered.

Ether sol. iodine.—Spreading; the solution is uniform, and has a deep iodine colour; after twelve hours it was a thick chesnut brown liquid.

Ether sol. bromine.—The mixture shows the following changes of colour: yellowish, reddish, brown, dark brown, umber colour; it thickens in a short time, and its odour is more modified than that of the residue from the iodine reaction.

(To be continued.)

PHYSICAL SCIENCE.

On the Molecular Dissymmetry of Natural Organic Products, by L. Pasteur, *Member of the Chemical Society of Paris.*

(*Continued from page* 133.)

It is necessary to submit here a very remarkable note from M. Mitscherlich, which was communicated to the Academy of Sciences by M. Biot. Here it is:—

"The paratartrate and tartrate (double) of soda and ammonia have the same chemical composition, the same crystalline form with the same angles, the same specific weight, the same double refraction, and, consequently, the same angle of optical axes. Dissolved in water, their refraction is the same. But the dissolved tartrate turns the plane of polarized light, and the paratartrate is indifferent, as M. Biot found through the series of these two genera of salts. But, adds M. Mitscherlich, here the nature and the number of atoms, their arrangement and their distances, are the same in the two compared bodies."

This note of M. Mitscherlich deeply interested me at the time of its publication. I was then a pupil at the Normal School, leisurely meditating on the beautiful studies of the molecular constitution of bodies, and attained, as I thought at least, to a clear comprehension of the principles generally admitted by physicians and chemists. The preceding note disturbed all my ideas. What precision in all the details! Are there two bodies whose properties have been better studied, better compared? But in the actual state of science, can we conceive two substances so similar without being identical? M. Mitscherlich himself tells us what was, in his mind, the consequence of this similitude.

"The nature, the number, the arrangement, and the distance of atoms, are the same." If this is true, what then becomes of this definition of chemical species, so rigorous, so remarkable for the time in which it appeared, given in 1823 by M. Chevreul? "In compound bodies,

species is a collection of beings identical in the nature, the proportion, and the arrangement of their elements."

In short, the note of M. Mitscherlich rested in my mind as an obstacle of the first order to our manner of considering material bodies.

Everybody will understand now, that being prepossessed, for reasons stated, with a notion of a possible correlation between hemihedrity of the tartrates and their rotary property, the note of M. Mitscherlich, of 1844, would recur to my memory. M. Mitscherlich, I at once supposed, was deceived upon one point. He could not have seen that his double tartrate was hemihedric, that his paratartrate was not; and if these things are so, the results of his note are not extraordinary; and I would have in it besides, the best criterion of my preconceived idea about the correlation of hemihedrity, and the rotary phenomenon.

I then hastened to resume the study of the crystalline form of the two salts of M. Mitscherlich. I found, in fact, that the tartrates were hemihedric, like all the tartrates which I had previously studied; but, strangely enough, the paratartrate was also hemihedric. Only the hemihedric faces which, in the tartrate, had all the same direction, were inclined in the paratartrate, sometimes to the right, sometimes to the left. In spite of all that was unexpected in this result, I did not the less pursue my idea. I carefully separated the crystals hemihedric to the right from the crystals hemihedric to left, and observed their solutions separately in a polarizing apparatus. I then saw, with no less surprise than pleasure, that the crystals hemihedric to the right, deviated the plane of polarization to the right; the crystals hemihedric to the left, deviated to the left; and when I took an equal weight of the two kinds of crystals, the mixed solution was neutral to light, by the neutralization of the two equal and opposite individual deviations.

Thus I set out from the paratartaric acid; I obtained in the ordinary way the double paratartrate of soda and ammonia; and after some days the solution deposits crystals, all of which have exactly the same angles, the same aspect, to such a degree that M. Mitscherlich, the celebrated crystallographer, in spite of the most minute and severe study, could not detect any difference between them. The molecular arrangement in the two, however, is entirely different. The rotary power, as well as the mode of dissymmetry, attest it. The two species of crystals are isomorphous, and isomorphous with the corresponding tartrate; but here the isomorphism is presented with a peculiarity without example up to this time; it is the isomorphism of two dissymmetric crystals, which reflect themselves in a mirror. This comparison presents the fact in a very just manner. In fact, if in both species of crystals I suppose the hemihedric facettes prolonged until they mutually meet, I obtained two symmetrical tetrahedrons inverse, and which cannot be superposed in spite of the perfect identity of all their respective parts. Hence I must conclude that I had separated, by the crystallisation of the double paratartrate of soda and ammonia, two atomic groups symmetrically isomorphous, intimately united in paratartaric acid. Nothing is easier than to prove that these two species of crystals represent two distinct salts, from which may be derived two different acids.

It is sufficient to proceed, as in all similar cases, to precipitate each salt by a salt of lead or barytes, and then to isolate the acids by sulphuric acid.

The study of these acids possesses immense interest; I know none more interesting.

But before submitting it, permit me to present here some notes relative to their discovery.

(To be continued.)

On the Light Emitted by Sodium Burning in Air, by M. H. FIZEAU.

WHEN engaged in completing the researches the results of which I hope shortly to lay before the Academy, I have had occasion to observe the fact, which I think has not yet been observed, and which I am now enabled to publish through the kindness of M. H. St. Claire Deville, who has verified it with me in the laboratory of the Ecole Normale, employing bodies the singular purity of which renders the observations more certain and decisive.

We have long known the mono-chromatic lamp of Sir David Brewster, who discovered the remarkable property of marine salt, in communicating to an alcohol flame a yellow colour emitting rays sensibly simple. The wave length of this light has been proved, by MM. Babinet and Delezenne, as sensibly equal to that of the region occupied in the solar spectrum by Fraunhöfer's line D. This remarkable source of homogeneous light is often employed in optical experiments where a mixture of colours, or of different wave lengths, introduces frequently great complication in phenomena; and it would be much more frequently used if the intensity of the light were greater.

From the recent researches of MM. Kirchhoff and Bunsen, the mono-chromatic lamp owes its properties to the presence of sodium, all the compounds of which, when vaporised in flames, give rise to the same phenomenon—the production of a yellow light, which by prismatic analysis is seen to be exactly superposed on the ray D of the solar spectrum. These savans have, moreover, shown that the intensity of this light becomes much greater on placing in the flame a globule of fused marine salt, supported by a platinum wire.

Another modification, which I can only slightly allude to here, but which will be fully described in my next memoir, allows me to obtain a mono-chromatic lamp, the light of which is very much simpler, and with which several important observations may be made which would be impossible with mixed light. I may mention, for example, the phenomenon of Newton's rings observed with this very simple light by normal reflection from the two surfaces of glass of great relative thickness for this kind of phenomena, since the thickness can be brought to 10·058 millimètres, corresponding to a ring of the 52,193rd order.

Unfortunately, however, a light pure enough to give these effects, is less intense than that obtained by a flame of alcohol containing a fused globule of marine salt, or by a flame of alcohol saturated with the same salt. In endeavouring to remedy the want of intensity of this source of light, I was led to try, amongst other reactions, the combustion of sodium in air. It is known with what violence oxygen then unites with the metal, and what an extraordinary evolution of heat and light accompanies the formation of soda.

The very brilliant light thus produced having been submitted to several trials, in which interference effects ought to have been produced, gave results entirely different from those of other flames in which the presence of sodium showed itself in a very constant manner by an emission of yellow light, which, when observed in the spectroscope, showed the double line D standing out very luminously from the dark background.

The observed effects did not accord with the most probable supposition of a great development of the ray D in the flame of sodium; and, indeed, having submitted this light to prismatic analysis, in order to determine its exact composition, I saw with surprise that the spectrum thus produced was continuous from the red to the violet, with the exception of the double ray D, which appeared of a deep black, projected on the brilliant background of the spectrum.

This phenomenon is the exact inverse of that produced by other flames containing sodium. With these, indeed, all the rays of the spectrum are absent, with the exception of those forming the ray D. With burning sodium all the rays of the spectrum are most brilliant, with the exception of the ray D, which is totally wanting.

I may add that this phenomenon only takes place when the combustion is vivid; as the metal commences to burn, the ray D is brilliant on a black ground; as the combustion becomes more active there are seen to be developed on each side of the ray D, which becomes fainter, intense rays, which at first only appear in the vicinity of D, but which spread rapidly through the whole extent of the spectrum with the ordinary colours when the sodium is entirely ignited; there are then only wanting the rays of the double line D, which detaches itself in intense black,—that is to say, with the same appearance that it has in the spectrum formed by the light emanating from the sun.

It is known that in continuation of an important observation of M. Foucault, MM. Kirchhoff and Bunsen have obtained, by means of the absorbed properties of flames, curious phenomena of the inversion of rays of the spectrum; and that these phenomena, in conjunction with a profound study of the spectra formed by different simple or compound bodies placed in a flame, have already yielded, and promise still more, to science brilliant and fruitful deductions. I have sought in vain to connect in a plausible manner the effects observed with burning sodium with the phenomena of absorption just mentioned. I may add, that many other new observations made at the Ecole Normale with M. H. St. Claire Deville and his pupils employing other metals in combustion, such as potassium of a rare purity, lithium, magnesium, and zinc, either alone or mixed with sodium, tend equally to the supposition that these two phenomena are not of the same nature. Finally, none of these metals have hitherto appeared to give corresponding effects to those which I have just mentioned.—*Comptes-Rendus*, liv. 493.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, March 6, 1862.

Dr. A. W. HOFMANN, *F.R.S., President, in the Chair.*

A Paper, by Mr. J. CROLL, "*On Specific Heat in Relation to Chemical Combination*," was read by the Secretary. After alluding to the opinion generally held with reference to the specific heat of compounds, compared with that of their component elements, namely, that a diminution took place during combination unless the resulting compound was a fluid, in which case the specific heat was increased, he stated that he had found this was not correct; for that the specific heat of compound gases and liquids was generally less, and that of solids more, than that of their component elements. A table of the specific heat of different bodies had been drawn up, from which it appeared that, out of 94 solid compounds, the specific heat of 66 had been increased, and of 28 diminished, by combination; out of 28 gases there was an increase with 6, and a decrease with 22; while, out of 33 liquids, there was an increase in 12, and a decrease in 21 cases. In 14 cases the specific heat increased during the passage of the substance from the gaseous to the liquid state, and it was reduced in 11 cases while changing from liquid to solid. From this it was probable that the increase in the specific heat of a compound solid body above that of its elements did not depend simply on its being solid, but, on the contrary, that it was solid because its specific heat exceeded that of its elements. The following considerations would perhaps throw some light on the subject: when any substance is heated, part of the heat is expended in producing expansion, and the other part in raising the temperature, and the sum of these two is equal to the specific heat. If a gas confined under a constant pressure is heated, it will be found to have a greater specific heat than if its volume be constant, the reason being, that in the former case some of the heat is absorbed in producing expansion; for the heat cannot do two things at the same time, and that which produces the expansion cannot affect the thermometer. When heat is applied to a mass of ice the temperature rises to 32° and then becomes stationary; this is the result of the difference in resistance that the heat experiences at the two outlets, the greatest amount passing to that at which there is least resistance; hence the molecular force of a solid body must diminish as the temperature rises; at 32° the molecular force of ice cannot overcome the repulsive force. On heating a gas there is no loss from molecular influences, but, with a solid, part of the heat is taken up in producing molecular changes, and therefore the less heat a solid contains the more does it resemble a gas in this respect, the specific heat increasing with the rise of temperature, from which it follows that the higher the melting point of a solid the less is the specific heat, which is experimentally found to be the case. In those cases of combination in which a change of one of the elements from a solid to a fluid state, or the contrary, took place, a change in the specific heat of the resulting compound could be accounted for. On the whole it would appear that the changes in the specific heat of bodies that occurred during combination were due not only to chemical action but also to molecular changes; the real specific heat of an atom remaining probably the same under all conditions.

The next Paper was by Mr. GREVILLE WILLIAMS, "*On the Indifferent Hydrocarbons Produced in the Destructive Distillation of Boghead Coal.*" Some doubt hung over the constitution of these bodies, Hofmann and Frankland having assumed in their vapour a condensation to two volumes, while MM. Laurent and Gerhardt had doubled the formulæ, to make their vapour equivalent to the usual four volumes. The marsh gas series of compounds was liable to be confounded with the methyl series, on account of the alternate terms of the former being isomeric with the latter. Dr. Hofmann, in his experiments on valerianic acid, had shown that the substance of the formula C_8H_{10}, and those standing higher in the scale, were broken up by heat into substances containing an equal number of equivalents of carbon and hydrogen, the excess of hydrogen passing off either as marsh gas or as water, the oxygen being obtained by the reduction of carbonic acid to carbonic oxide, these latter compounds being more stable, which accounted for the fact that no derivatives of the marsh gas series corresponding to the fatty acids had been obtained. Dr. Frankland had suggested that it was probable that, by regulating the temperature in the distillation of Boghead coal, hydride of amyl might be obtained, this substance being valuable for illuminating purposes. It appeared, however, that if the distillation was conducted at a low temperature, bodies belonging to the benzol, the olefiant, and the alcohol radical series were obtained, or at least bodies isomeric with these last. From the inertness of these substances it was difficult to make

any crucial experiments; but, with one exception, they agreed in the boiling point and other physical properties, and on this account it seemed improbable that they should be only isomeric with this series; besides which, since the olefiant series were obtained, it was highly probable that these, which were formed on the same type, should be found among the products of distillation. It had occurred to him that the point might be determined by trying whether the tar contained a liquid boiling at a lower temperature than propyl, for this could not be ethyl, that substance being a gas. A fluid was obtained by rectification, boiling between 30° and 40° C., whose properties and composition agreed with that of hydride of amyl, showing that these bodies belonged to the marsh gas series. He had hoped that the radicals and hydrides would behave differently under the influence of heat, but this did not appear to be the case.

The President remarked that the subject had attracted great attention, that Mr. Williams had changed his opinion with reference to these substances, but he scarcely considered the experiments conclusive on the point.

Dr. Frankland said that, although the experiments were not conclusive, still analogy would lead to the conclusion that the substances in question belong to the marsh gas family, for that these were more permanent than the radicals themselves, and would therefore be probably found in products produced by destructive distillation.

Dr. Odling considered that the liquid boiling at a temperature below the boiling point of propyl, which Dr. Williams considered to decide the point, might be a mixed radical, a butylmethyl.

Dr. Crace Calvert said that there had lately been introduced from America naphthas of very low specific gravity and excessive volatility; the mobility of the liquids was also very great; they did not appear to be homologous with benzol; for although nitro-compounds might be produced, he could not succeed in obtaining aniline from them.

Dr. Crace Calvert then made a verbal communication of some observations with respect to *Iron-plated Ships*. The difficulty of preserving iron in contact with oak was well known, the acid of the oak acting on the iron and causing it to rust. The plan at present adopted to prevent this action was to place a layer of teak between the iron and the oak; the bolts, however, were still exposed to the action of the oak, so that the protection was not complete. It had occurred to him that galvanised iron might resist the action of the oak, and, to prove this, he had allowed some bolts which had been galvanised to remain in contact with oak for some time, and found that the action was much slower than when iron alone was employed; the iron was also protected in this manner from the action of fresh and salt water. He thought that this might supersede the use of teak, which would be very desirable, as the price of this wood was very high.

Dr. Hofmann then made some remarks "*On some Colouring Matters from Aniline.*" He had, some years ago, obtained, by the action of chloride of carbon on aniline, a compound derived from three equivalents of ammonia, in which the carbon from the chloride of carbon replaced three equivalents of hydrogen. A number of other bodies were produced at the same time, and this substance was obtained in such small quantities that it was impossible to purify it. Since that time, however, this body had been made in larger quantities and by other methods, and he had been furnished by Mr. Nicholson with salts of the body in a very pure state. A great many experimenters had examined this substance, and had arrived at very various results; in fact, they were so discordant, that he had concluded that the descriptions could not relate to the same substance. It appeared, however, that the red colouring matter consisted of only one substance; it had one peculiarity, that, although a basic substance, it would not form a platinum salt, the other salts being formed with the greatest readiness. The acetate was the salt employed for practical purposes, and from this the base might be precipitated by ammonia or potash; a dark red substance being obtained, which, however, was not the pure base, for on boiling the acetate with a large quantity of a solution of ammonia, filtering while hot, and allowing the filtrate to cool, perfectly white crystals of the pure base were deposited; the red precipitate obtained by ammonia had the same composition as this substance, on exposure to air it soon assumed a red tint, and when dissolved in an acid the original colour was reproduced. This base formed beautiful crystalline compounds with acids, and would even displace ammonia from its salts; its composition was represented by the formula $C_{20}H_{19}N_3, H_2O$, this being the hydrate as precipitated from solution; the formula of the chloride being $C_{20}H_{19}N_3, HCl$. It could also unite with three equivalents of hydrochloric acid, but this compound was instable, being gradually decomposed at a temperature of 100°, and the mono-acid salt reproduced. Under the action of reducing agents it lost its colour, and assimilated two equivalents of hydrogen, forming a white substance of the formula $C_{20}H_{21}N_3$; this, which was anhydrous, formed colourless salts which contained three equivalents of acid, and was reconverted by oxidising agents into the former base. Further oxidation gave various derivatives which had not been fully examined; with nitrous acid an explosive substance in which nine equivalents of hydrogen are replaced by three of nitrogen, was formed. Destructive distillation also gave rise to various crystalline bodies. The constitution of this body was, as yet, not definitely settled.

Dr. Crace Calvert inquired what was the cause of the base turning red on exposure to air?

Dr. Hofmann replied that it was probably owing to a small quantity of acid combining with it and forming a salt.

NOTICES OF PATENTS.

1193. *Purifying Lead.* D. Zenner, Newcastle-on-Tyne. Dated May 11, 1861. (Not proceeded with.)

According to this invention the lead to be purified is melted and then subjected to the action of oxygen, chlorine, bromine, iodine, or *fluorine* (!) employed alone or conjointly, or in admixture with atmospheric air, carbonic oxide, or carbonic acid. A current of either of these gases is passed through the melted lead whilst in the furnace, or the fluid metal is caused to descend in a fine stream or shower through an atmosphere impregnated with one or other of these agents.

The fact that melted lead may be purified to some extent by the oxidising action of the air has long been known. In regard to the application of chlorine to these purposes, success will depend upon the nature of the impurities to be removed, but it is not altogether improbable that under special circumstances this gas might be used in the process of lead refining. The suggestion relative to the employment of bromine and iodine, and also of fluorine, which is not yet known in the elementary condition, is obviously premature.

1200. *An Improved Treatment of Natural Phosphate of Lime for several Purposes.* A. C. A. Gerard de Meley, Paris. Dated May 11, 1861.

The object of this invention is to obtain pure phosphate of lime and carbonate of soda from the several natural varieties of crude phosphate of lime. In the ordinary process of manufacturing super-phosphate of lime by digesting bones or other phosphatic matters with sulphuric acid, much phosphoric acid is set free, which the inventor has discovered can be economically employed in the conversion of sea salt into the phosphate, and ultimately

into other salts of soda. His method of proceeding is as follows:—He treats phosphate of lime, in the state of apatite, coprolites, phosphatic nodules, or bones, crushed or ground, with a sufficient quantity of diluted sulphuric acid to liberate the whole of the phosphoric acid contained therein. The materials are left in contact and digested for about twenty-four hours with occasional stirring. The liquid portion is then drawn off; the residue is washed, and the wash water added to the decanted fluid. The more dilute washing liquors are reserved for the purpose of mixing with fresh sulphuric acid in a repetition of the treatment. The decanted liquid contains free sulphuric and phosphoric acids, phosphate and sulphate of lime, and small proportions of the corresponding compounds of iron, which last may be separated in the form of Prussian blue by the addition of ferrocyanide of potassium. This solution is next evaporated, and when sufficiently concentrated, a quantity of sea-salt is added; the alkaline chloride is decomposed with evolution of muriatic acid, which may be collected as usual. The saline mass left in the retort consists of phosphate and sulphate of soda, which, together with a small quantity of soluble phosphate of lime, is dissolved out with water, at the same time that the phosphate of iron and sulphate of lime are left insoluble. To this solution the patentee adds a quantity of lime, regulating the amount in proportion to that of the phosphoric acid in the liquor, and allows the materials to remain in contact for twenty-four hours, during which period they are frequently stirred. By this reaction all the alkali is liberated, and may be recovered from the solution in the form of caustic soda, or it may be converted into carbonate by passing through the crude liquor, the gaseous products of combination generated in the ordinary furnace. If the latter expedient be adopted, it is recommended to wash the smoky gases before employing them as the source of carbonic acid, and to evaporate the solution in order to obtain crystals of carbonate of soda. The insoluble phosphate of lime is to be collected and dried in the same manner as an ordinary precipitate.

It is doubtful whether much advantage would be gained on the score of economy by following the somewhat intricate details of this succession of processes. The manufacture of alkali by the intervention of phosphoric acid is not so likely to be commercially successful as by the direct operation of sulphuric acid.

1214. *Decomposition of the Compounds of Aluminium, and Coating Metals with Aluminium or its Alloys.* T. Bell, Gateshead. A Communication. Dated May 13, 1861.

This invention consists in effecting the reduction of aluminium compounds, particularly the double chloride of aluminium and sodium, by the agency of voltaic electricity; and in employing the same means for the purpose of coating metals with aluminium. By this process the patentee converts the surface of copper into aluminium bronze.

The mineral cryolite, which readily furnishes aluminium under the reducing action of sodium, would probably constitute one of the most available sources for the preparation of this metal by the help of the galvanic battery.

1223. *Manufacture of Steel.* W. Clark, Chancery Lane, London. A Communication. Dated May 14, 1861.

The patentee claims the mode of converting iron into steel by treating it at a high temperature with a mixture of coal-dust and alkaline carbonate, or with other matters containing hydrogen and nitrogen employed conjointly with an alkali. Such combinations are stated to be very efficacious in the removal of sulphur from the iron, so that the metal undergoes a process of purification at the same time that it is converted into steel.

Grants of Provisional Protection for Six Months.

262. Pierre Scheurweghs and Alexandre Joseph Aurele Henry de Boisserolle, Paris, "Certain improvements in treating fatty and oily matters for obtaining their acidification, and in the apparatus employed therein."—Petition recorded 31st January, 1862.

288. William Clark, Chancery Lane, London, "Improvements in processes for preserving and colouring wood, denominated xylochromic and xyloplastic processes."—A communication from Messrs. Heinrich Sperl, Richard Hogen, and Wolfgang Springer, Nuremberg, Bavaria.—Petition recorded 3rd February, 1862.

294. Richard Archibald Brooman, Fleet Street, London, "Improvements in the manufacture of hard and soft soaps, and in the preparation of liquids for washing linen and other textile fabrics."—A communication from Léo Bachellerie, Paris.

328. William Clark, Chancery Lane, London, "Improvements in preserving timber, which are particularly applicable to the timbers of ships or other maritime structures."—A communication from M. Henri de Lapparent, Boulevart St. Martin, Paris.

338. Marc Antoine François Mennons, Rue de l'Echiquier, Paris, "Improvements in the treatment of coprolites and other fossil phosphates of lime."—A communication from Guillaume Louis Edourd Buran and Louis Goupy, Rue du Grand St. Michel, Paris.

340. James Dickson, Tollington Road, Holloway, London, "Improvements in voltaic apparatus and in the production of voltaic electricity."

Notices to Proceed.

2580. William Smith, Salisbury Street, Adelphi, London, "Improvements in the apparatus for and method of increasing the illuminating power of gas."—A communication from Hugo Carstanjen, Cologne, Germany.

2605. Hugh Macmekan, Stratford, Essex, "Improvements in smelting copper, gold, and other ores."—A communication from Rowland Vounder Rodder, Alberton, near Adelaide, South Australia.

2609. Robert Mushet, Coleford, Gloucestershire, "A new or improved manufacture of titanic pig metal or alloy of titanium and iron."

2623. Josiah Timmis Smith, Barrow, Furness, Lancashire, "An improvement or improvements in collecting the inflammable gases evolved from blast furnaces."

2637. Robert Mushet, Coleford, Gloucestershire, "An improvement or improvements in the manufacture of a certain metallic alloy."

2643. George Henry Birkbeck, Southampton Buildings, Chancery Lane, London, "Improvements in processes or means employed for separating or extracting silver from lead."—A communication from Jules Frederic de la Batie, Paris.

2656. Isaac Louis Pulvermacher, Oxford Street, London, "Improvements in apparatuses for the production of galvanic and magneto-electric currents, and in machinery employed in making some of the apparatuses."

2784. George Tomlinson Bousfield, Loughborough Park, Brixton, Surrey, "Improvements in electroplating or depositing metals."—A communication from Jabez Ellis Walcott, Boston, Massachusetts, U.S.—Petition recorded 5th November, 1861.

2822. William Edward Newton, Chancery Lane, London, "Improved apparatus for manufacturing and containing gaseous liquids."—A communication from Pierre Prudence Henri Couillard, Rue St. Sebastien, Paris.—Petition recorded 9th November, 1861.

2924. George Henry Polyblank, Gracechurch Street, London, "A new or improved method of protecting and preserving photographic and other prints, water-colour drawings, and other works of art from injury and decay."—Petition recorded 21st November, 1861.

3150. Emile Cajot, St. Servais, Belgium, "Improvements in the treatment of pyrites for the manufacture of iron."—Petition recorded 14th December, 1861.

23. Hermann Eschwege, Mincing Lane, London, "Im-

provements in treating wood and other vegetable spirit."—Petition recorded 2nd January, 1862.

81. Thomas Ramsay, Newcastle-on-Tyne, "Improvements in the manufacture of coke."—Petition recorded 11th January, 1862.

MISCELLANEOUS.

Chemical Society.—The next Meeting of this Society will take place on Thursday next, when a Paper will be read by Mr. A. H. Church, "On the Isolation of Phenyl."

Royal Institution.—The following Lectures will be delivered in the ensuing week:—Tuesday, March 18, at three o'clock, John Marshall, Esq., "On the Physiology of the Senses." Thursday, March 20, at three o'clock, Professor Tyndall, "On Heat." Friday, March 21, at eight o'clock, F. A. Abel, Esq., "On some of the Causes, Effects, and Military Applications of Explosions." Saturday, March 22, three o'clock, H. F. Chorley, Esq., "On National Music."

Plumbago in India.—The *Bombay Gazette* of the 12th ultimo announces the discovery of considerable quantities of black lead at Sonah. Dr. Thompson, Civil Assistant-Surgeon, Goorgaon, reports that the quality of the samples he has examined is such that they bear a favourable comparison with average specimens from the North of England. By chemical analysis the Indian black-lead was found to consist in 100 parts, of

Carbon	78·45
Silica and alumina	12·98
Peroxide of iron	3·30
Carbonate of lime	·84
Water	4·35
Alkaline sulphates and chlorides	·08
	100·00

The proportion of earthy constituents, 17·2 per cent., is somewhat higher than would be found in first class black-lead; but this amount does not appear excessive in comparison with that contained in the ordinarily good qualities of this mineral.

Bessemer Steel.—The rapidity with which the Bessemer process is being at last adopted is extraordinary. Whatever may be said of the earlier failures in the attempt to produce good malleable iron or steel by blowing a blast of air through a quantity of melted cast iron, it is now certain that ingots of the best quality may thus be produced in from a quarter to half an hour, and, if the iron is taken as it comes from the blast furnace, without any expenditure of fuel whatever. Mr. Bessemer's memorable experiments, made six years ago in Baxter House, were with a good quality of Welsh iron—Blaenavon we believe—which happened to have but little sulphur or phosphorus, and a good quality of metal was certainly made, as many who secured samples at the time can testify. But when the pneumatic process came to be applied to other irons it proved, in several cases, unsuccessful. No particular attention was directed to the impurities of the iron, since there was no *prima facie* reason for supposing that their presence, so long as it did not prevent the production of merchantable iron by the ordinary process, would cause special difficulties in the new. But difficulties there were, and at one time it was believed by most of those who had paid much attention to the new process, that it had failed completely, which was, of course tantamount to believing either that the first successes were achieved by juggling, or that what had once been done could not be repeated. Mr. Bessemer persevered, however, and was able to give a good account of himself and his process, three years ago, at a meeting of the Institution of Civil Engineers. From his explanation it then appeared, for the first time, that only the presence of sulphur or phosphorus, in notable quantities, in the iron, affected the conversion of pig metal into good malleable ingots, in less than half an hour, and without either fuel or manipulation. Great difficulty, it was true, had been experienced in finding a lining material, capable of withstanding the heat of the converting vessels, but it was at last found that the "ganister," which occurs in abundance in the neighbourhood of Sheffield, was perfectly adapted for the purpose, and already have nearly one hundred charges, averaging nearly a ton each, been converted in one vessel at Mr. Bessemer's works, with one lining only. Hematite iron having been adopted, and the refractory properties of ganister sufficiently tested, almost the only difficulties in the way of the new manufacture had been overcome before the meeting, last summer, of the Mechanical Engineers at Sheffield. It was then that Mr. Bessemer was first enabled to bring the action and results of the pneumatic process visibly before a great body of practical men, all of whom had every opportunity afforded them for examining into all its details, which, however, are very few and exceedingly simple. A ton of iron worth under 3*l.* was converted, with a loss of less than 17 per cent., into a material intrinsically worth, in comparison with other steel, 40*l.* or 50*l.* The fuel was only that (say 3 cwt.) employed for melting in the cupola, and the actual cost for labour could not have amounted to as much as one shilling. The product could be graduated to any required hardness or toughness, the blast being first kept on until nearly all the silicon and carbon were burnt out of the iron, immediately after which, the blast being stopped, a measured quantity of melted cast iron, containing an ascertained proportion of carbon, was added. If enough carbon was restored in this way the ingot would be one of highly carbonized, hard, and unweldable steel, having great cohesive strength. If very little carbon were restored, at the conclusion of the process, the ingot would be soft, nearly as ductile as copper, and capable of being readily welded, while its strength would be from one-third to one-half greater than that of the best Yorkshire malleable iron. The converting process being completely under control, hundreds of tons of metal could be produced of any desired working quality.—*Engineer.*

Compound of Iron and Ammonium. — Dr. Meidinger (*Neues Jahrb. für prakt. Pharm.*, Bd. xvi. s. 295) says, that a voltaic current passed through a mixed solution of a protosalt of iron and sal ammoniac produces a bright metallic precipitate which resembles polished steel in appearance. It forms a very thin casing on the pole which scales off as the deposit increases. Carefully rinsed with a good deal of water and dried on blotting-paper, it evolves, when treated with a caustic alkali, a strong smell of ammonia. When heated, the smell is also perceived, but is quickly dissipated. The precipitate when put into water and heated nearly to the boiling point, evolves a good deal of hydrogen. The author has no doubt that the precipitate is an alloy of iron and ammonium; others, perhaps, will think differently. The most ammonia he found in a precipitate was 1·5 per cent.

ANSWERS TO CORRESPONDENTS.

Preparation of Cerium Salts from Cerite.—W. C.—We have found the following plan effectual:—Powder the mineral and dissolve it in aqua-regia, evaporate nearly to dryness, and extract with water; add ammonia till a precipitate just begins to take place, carefully avoiding excess (if excess be added, exactly neutralize it with acetic acid). Add tartrate of soda to complete precipitation, avoiding excess. Collect the precipitate of tartrate of the cerium metals and tartrate of lime, wash well, and extract with ammonia. Tartrate of lime remains undissolved. Filter and re-precipitate the cerium tartrates with hydrochloric acid. Wash, dry, and ignite.

C. R.—The specimen is too small for accurate analysis, but we can find no silver in it.

Acetate of Tin.—A correspondent wishes to know the best plan for making this compound in the liquid state and neutral, about 14 T.

THE CHEMICAL NEWS.

VOL. V. No. 120.—March 22, 1862.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Colouring Matters Derived from Aniline, by Dr. A. W. HOFMANN, *F.R.S.*

(*Continued from page* 142.)

HOWEVER numerous and varied the analytical results which tend to confirm the formula of rosaniline and its compounds, it appeared desirable to corroborate the expression derived from simple analysis, by additional experiments. To this end, I have studied the products of decomposition of rosaniline which are at the same time numerous and interesting. I will only at present point out one or two transformations of this compound which appear worthy of interest, not only because they confirm the formula which I have proposed, but also on account of their throwing light on the nature of the class of substances to which rosaniline belongs.

Action of Reducing Agents on Rosaniline.—This reaction appeared likely to furnish the most simple means of controlling the formula of the colouring matter; an opinion which did not fail to be realised.

Rosaniline is rapidly attacked by hydrogen in the nascent state, or by sulphuretted hydrogen. A solution of the base in hydrochloric acid is soon decolorised upon being left in contact with metallic zinc. The liquid so obtained contains, along with chloride of zinc, the chloride of a new triamine, which is perfectly colourless both in the free state, as in saline combination. I propose for it the name of *leucaniline*. The separation of the new compound from zinc is long and tedious; I prefer, therefore, to prepare it by the action of sulphide of ammonium.

A salt of rosaniline being digested with sulphide of ammonium for some time, yields a fused mass solidifying upon cooling to a brittle, scarcely crystalline body, which consists of leucaniline in an almost pure state. It is not, however, necessary to employ a pure salt of rosaniline for the preparation of this compound. I have generally prepared leucaniline by means of commercial products sold under the name of Fuchsine or Magenta. To purify the product thus obtained, the yellow resinous mass is reduced to powder, washed with water to remove the sulphide of ammonium, and dissolved in dilute hydrochloric acid to separate the sulphur and impurities.

The deep brown solution thus obtained furnishes with concentrated hydrochloric acid an abundant crystalline precipitate, which is brown or yellow according to the degree of purity of the commercial colouring matter which has been employed. Prolonged washings with concentrated hydrochloric acid, in which the precipitate is almost insoluble, enables it to be purified to a certain extent. But in most cases it is necessary to repeat the preceding operation once or twice; that is to say, to dissolve it in dilute hydrochloric acid, and precipitate it with the concentrated acid. If before the last addition of concentrated hydrochloric acid, the solution is heated to ebullition, the liquid remains clear, and the new chloride separates on cooling in the form of crystals. These are rectangular tablets, perfectly well formed, but always very small, and frequently of a dazzling white colour. A re-crystallisation from water, in which they are extremely soluble, is sufficient to purify them. The salt may also be dissolved in alcohol, and precipitated by ether, in which it is perfectly insoluble.

The chloride thus purified furnishes, on the addition of ammonia, leucaniline in the state of a perfectly white powder, which assumes a feeble rose tint when it remains for some time in contact with the atmosphere of the laboratory. It is scarcely soluble in cold, and very slightly so in boiling water, from which it separates on cooling in the form of small crystals. It is very soluble in alcohol; slightly so in ether. I have not succeeded in obtaining it in fine crystals by means of these solvents. The best solvent appears to be a solution of the chloride described above, in which leucaniline is readily soluble, and from which it separates upon cooling in the form of needles interlaced together, which are frequently re-united into rounded groups. Leucaniline can be dried *in vacuo* over sulphuric acid without changing colour. When carefully heated it becomes red, and at 100° it fuses to a deep red transparent liquid, which, upon cooling, solidifies to a mass not so deeply coloured. Leucaniline is anhydrous; the analysis of this substance dried *in vacuo*, and at 100°, furnished results corresponding to the formula

$$C_{20}H_{21}N_3.$$

This formula has been verified by an examination of the chloride, already mentioned, of a perfectly crystallised platinum salt, and finally of the nitrate, which can likewise be obtained in beautiful crystals.

Chloride of Leucaniline.—The preparation of this compound has been already described above. It is tri-acid, and, after desiccation *in vacuo*, retains one equivalent of water. Its formula is

$$C_{20}H_{21}N_3.\ 3HCl + H_2O.$$

This salt cannot be dried at 100°, at least in contact with air; but the water can be removed, although with difficulty, by maintaining it for a sufficient length of time at 100° in a current of hydrogen. I have in vain sought to convert this compound into a salt containing less acid, by boiling its solution with an excess of leucaniline. This solution, upon cooling, deposits the base in magnificent crystals, the tri-acid salt remaining in solution.

Platinum Salt of Leucaniline.—On adding bichloride of platinum to a moderately-concentrated and warm solution of the chloride of the base, there separates upon cooling a very beautiful platinum salt of a brilliant orange-yellow colour, in the form of well-defined prisms, generally grouped into triangular stars. This salt is difficultly soluble in cold water; boiling water decomposes it. At 100° it contains one equivalent of water,

which can be driven off, although with difficulty, at a higher temperature. Many analyses of this magnificent compound led me to the formula,

$$C_{20}H_{21}N_3.3HCl.3PtCl_2 + H_2O.$$

Nitrate of Leucaniline. — Well-formed white needles, soluble in water and alcohol, insoluble in ether. The salt is difficultly soluble in nitric acid; dried *in vacuo* it contains,

$$C_{20}H_{21}N_3.3HNO_3 + H_2O.$$

I have not succeeded in separating the water of crystallisation, the salt being decomposed at 100°.

The salts of leucaniline are in general well crystallised; they are all very soluble in water, and are precipitated from their aqueous solutions upon the addition of their respective acids. The sulphate is remarkable for the facility with which it crystallises. I have submitted leucaniline to the action of bisulphate of carbon, chloride of benzoïle, and many other agents. In each of these cases leucaniline is promptly attacked, giving rise to new compounds, some of which have considerable crystalline powers. The study of these substances does not bear upon the present communication. The present note is only intended to fix the composition of the two new bases and the relation which exists between them. This relation, as a glance at their formulæ will show, is of the simplest description. In the anhydrous state, the two substances contain respectively :—

Rosaniline ..	$C_{20}H_{19}N_3$
Leucaniline ..	$C_{20}H_{21}N_3$

Leucaniline only differs from rosaniline by containing two equivalents of hydrogen more. Between these two bases the same relation is observable as between blue and white indigo :—

Blue indigo..	$C_{16}H_{10}N_2O_2$
White indigo	$C_{16}H_{12}N_2O_2$

As might have been anticipated, leucaniline is easily changed back again into rosaniline by oxydising agents. The reaction succeeds perfectly with peroxide of barium, perchloride of iron, and especially with chromate of potash. On carefully heating the colourless solution of the chloride with one of these agents, the liquid rapidly assumes the beautiful crimson colour of the salts of rosaniline. An excess of the oxidising agent must, however, be avoided, so that the action may not proceed too far, in which case the regenerated rosaniline would be transformed into products of higher oxidation. Rosaniline as well as leucaniline, being submitted to prolonged ebullition with compounds rich in oxygen, changes into a brown amorphous powder, the composition of which is at present unknown.

The two bases which I have now described are the prototypes of two series of homologous colouring matters, which cannot fail to be obtained from the homologues of aniline; toluidine, indeed, produces exactly similar bases. I have not examined in the present memoir the nature of the reaction by which aniline is changed into rosaniline. In most processes the formation of this substance is accompanied by that of several other bases, the study of which is not yet completed. At present I am not in a position to give an opinion on the constitution of the new compounds, however attractive such a speculation may be. It is in the hope of rendering the formulæ of these new bases clearer, that I am at the present time examining the products of their decomposition. This study is not yet completed. I am, however, enabled to state that rosaniline and leucaniline in nitric solution are each attacked very energetically by nitrous acid, producing new bases, the platinum compounds of which are distinguished by their fulminating properties, which are manifested by contact with boiling water and even with ammonia, at the ordinary temperature. With the other derivatives of rosaniline I ought also to mention a magnificent crystalline base which is associated with aniline amongst the products of the dry distillation of the colouring matter.—*Comptes-Rendus*, liv. 428.

On Soil-Analysis, by Professor S. W. Johnson, *of Yale College.**

In no country has there been so much popular appreciation of practical science as in the United States of America. Scarcely one of the States is without its volume or volumes of Geological and Natural History Reports, and though some of them have been content to confine the work to the merest outline of the general and industrial geology of their territory, and have expended but a few hundreds of dollars in the undertaking, others, like New York, have embraced all the branches of Natural Science in their survey, have prolonged the work of exploration or elaboration through many years, and have devoted money to these objects with unsparing hand.

The results of these surveys as they stand recorded in the numerous volumes published by the States and by the General Government, are of very unequal merit, as might be expected from the wide range of country explored, from the various degrees of interest and appreciation governing the many Legislatures which have authorised these labours, and from the exceedingly unequal ability of the individuals charged with their execution.

These explorations have originated in all cases with our scientific men. It is their influence either brought to bear immediately upon the legislative bodies, or exerted less directly through cultivated and public-spirited persons to whom the possible benefits of geological surveys have been explained—that has accomplished this vast work.

The enterprises of which we speak being sustained pecuniarily at the expense of the people, and depending from year to year in many cases upon the popular vote, it has been not only politic but right to exhibit at the outset the prospects of pecuniary return for the required outlay of means, as an inducement to support such undertakings. It has been no less proper in presenting the results of the surveys, to lay stress on the discoveries having industrial bearings which are the fruits of the work.

In those States where large quantities of metallic ores occur, the interests of capitalists engaged in mining has often sufficed to inaugurate a Geological Survey. In other States the agricultural sentiment has had to be operated upon.

Great results have been promised to agriculture from the applications of geology and chemistry, and a great deal of labour been performed in the attempt to satisfy the hopes that have been thus excited.

The chief object of the present notice is to inquire what has been really accomplished for the good of the farmer, by the scientific surveys that have been hitherto prosecuted in this country.

The labours of Dr. Peter in connection with the Kentucky and Arkansas Surveys being the most recent

* From the *American Journal of Science*, xxxii., 233.

and extended attempts of this kind, we shall make them the basis of our inquiries.

If we except a few pages of general remarks on the theory of vegetable nutrition, &c., which while useful to the practical readers of the "Report," contain no new facts or principles,—the whole effort of Dr. Peter has been concentrated on the analysis of soils, marls, rocks, and ashes. He publishes in the four "Kentucky Reports" analyses of 375 soils, and in the "Arkansas Report," 187; in all, 562 soil analyses. Besides, we find the results of examinations of 145 rocks, shales, &c., and of 38 ashes of plants, making a grand total of 795 agricultural analyses.

The agricultural fruits of the surveys of Kentucky and Arkansas are then to be sought in these analyses.

It certainly will strike all that the amount of work performed by Dr. Peter is unusually great. It is now but six years since the Kentucky survey was commenced, and in that time the Doctor has not only analysed 795 soils, but has executed 516 analyses of ores, slags, mineral waters, and coals, making an average of two analysis for every three days of this whole period. This labour, Dr. Peter states, he has accomplished with the help of one intelligent assistant, and by a special organisation of his laboratory and his operations, whereby the utmost economy of time was secured. We have had such experience of the advantages of a similar system, that we are not prepared to doubt that the chemist who adopts a plan of analysis which fully satisfies him, and from which he never departs, may execute such an amount of work. At the same time, we must bear in mind that the only control Dr. Peter offers for the accuracy of his results is, that the sum of the weights of the separate ingredients equals their original conjoined weight, no time being allowed to repeat a determination, or to prove the purity of a precipitate.

The Analytical Process followed in these analyses is not by any means so minute and full as we should be warranted to expect, when their author declares (Fourth "Kentucky Report," p. 57) that "such a work, to be eminently useful, must be thorough and exhaustive;" for soluble silica, chlorine, nitric acid, and ammonia, are not at all estimated, and the condition of the iron, whether protoxide or peroxide, is not noticed. It is worthy of notice that carbonic acid and lime are always present in atomic proportions in the soils latterly analysed, no excess of either ingredient being mentioned in the results. Carbonic acid, however, is not noticed in the description of the analytical process, and that figuring in the analyses does not appear to have been directly estimated, but to have come from the oxalic acid of the reagent shelf.

If, as might easily happen, the contrary not being proved, a portion of the lime dissolved by hydrochloric acid exists in these soils as silicate, sulphate, or phosphate, then the assumption that it is united to carbonic acid introduces an error into the summing up (which in many cases is exactly 100) and shows that a quantity of some other ingredient has been overlooked.

For the estimation of phosphoric acid, a highly modified form of Sonnenschein's process is employed, but our author does not give the figures which prove that his changes are improvements.

Admitting, however, that the analyses are correct, we next inquire what is their value?—what useful deductions from them appear in these "Reports"?

In the introduction to Vol. I. "Kentucky Survey," p. 13, Dr. Owen says:—"By consulting the numerous interesting results obtained by the chemical analyses of the soils embodied in the pages of this report, abundant evidence will be gathered of the vital necessity of wide dissemination among the farming community, of the knowledge to be obtained by a correct insight into their chemical constitution." In the same volume, page 373, Dr. Peter remarks that he was impressed "that when the composition of our Kentucky soils and minerals in general, is once accurately established, their applications to our wants and uses would be obvious to all well-informed persons. He has, therefore consumed the time mainly in the analyses, and made up his report principally of the results."

In the agricultural section of the "Arkansas Survey," p. 47, Dr. Owen says:—"Principally from chemical soil-analyses can the agriculturist form an intelligent opinion as to the comparative fertility of soils, and their suitability to the growth of certain plants, as well as judge what applications may be required in the way of lime, bone-earth, plaster of Paris, ashes, or salts of potash, soda," &c.

Dr. Peter, in the same volume, page 166—7, observes: "It is believed that by no other mode than by chemical analysis or by the more tedious and laborious method of actual experience, in cropping for a series of years and publishing a record of the same, can the actual nature, capabilities, and value of the various soils of a State be presented to the public; and that by instituting this geologico-agricultural survey, the State of Arkansas not only aids materially in the progress of the general science of the civilized world, and that of the soil in particular, but takes the most effectual mode of making known to the enlightened immigrant her agricultural riches. In this she has followed the wise lead of the older State of Kentucky, in which, since the institution of her Geological Survey, the value of the land in the regions examined and reported on has been very greatly enhanced."

In the "Agricultural Geology of Kentucky," Second Report, p. 9, Dr. Owen says:—"Placing implicit reliance on the capabilities of chemical science to indicate by the analyses of soils, the ingredients removed by the cultivation and harvesting of successive crops, it was hoped that by collecting samples of the virgin soil, and of the same soil from an adjacent old field, that not only the different substances assimilated out of the soil could be ascertained, but also the proportion of these so that the farmer might know precisely what must be restored to the land to bring back its original fertility."

These quotations sufficiently show what were the opinions which led our author to devote such an amount of labour to the analysis of soils, and indicate in general, what results were expected.

In the Second "Arkansas Report," p. 49, *et seq.*, Dr. Owen "proceeds to explain in what way soil-analysis becomes of value to the farmer." He desires "to call particular attention to this subject, because the opinion has been expressed even in this year (1860), and by those having a high standing in the scientific world, that chemistry is incapable of conveying any useful information to the farmer by analysing his soil."

On the six following pages of the Second "Arkansas Report," and on page 30 of the Fourth "Kentucky Report," Dr. Owen gives the most complete *résumé* of the teachings of soil-analysis which we are able to find in the five volumes before us, and as these are his latest writings on the subject, and as he then had the data of 389 analyses, viz., of 187 Arkansas soils, and 302 in the three volumes of the "Kentucky Report,"—

these being referred to on the pages we are quoting from,—we are warranted in considering what he has here presented, as embodying the strong points in favour of soil-analysis. We will notice them separately as gathered from both "Reports."

1st. "Any one who will take the trouble to inspect the analyses of the 187 Arkansas soils will see that the relative proportions of the eleven mineral constituents of these soils is very accurately given."—*Second Arkansas Report*, p. 49.

If we admit fully that Dr. Peter's analyses represent with fair accuracy the composition of the two grammes of soil he experimented with in each instance, we do not, therefore, allow that the composition of "these soils" considered as representing geological formations§, or large agricultural districts, or even single fields, is "very accurately given."

Here at the outset the distinguished gentlemen who have conducted the "geologico-" and "chemico-agricultural" part of the Kentucky and Arkansas surveys have taken for granted, what, being an error, overturns their whole reasoning, and renders their soil-analyses comparatively worthless.

(To be continued.)

On Nitrate of Ethyl, by M. Carey Lea, *Philadelphia.*

Action of Reducing Agents.

Acetic Acid and Iron.—Acetic acid, mixed with nitrate of ethyl, was made to act on iron filings. On the application of a gentle heat, a violent action set in, and most of the nitrate of ethyl was driven off undecomposed. The tube of a Liebig's condenser was, therefore, attached so that the distillate might constantly flow back, and heat was applied until the whole of the nitrate of ethyl was acted on. A considerable quantity of deutoxide of nitrogen was evolved and some nitrite of ethyl. Finally caustic soda was added and the gaseous products were conducted into water. A very considerable quantity of ammonia was thus obtained, but no ethyl bases could be distinguished.

From a considerable number of experiments, I am disposed to think that the ethyl bases are produced in but a limited number of reactions, and that in many cases when they would seem to be possible products, ordinary ammonia is alone formed. For example, it appeared not improbable that by acting on dry ethylosulphate of ammonia* $C_4H_5(NH_4)O_2 2SO_3$, with caustic lime, ethylamine would be formed simultaneously with hydrated sulphate of lime $CaOSO_3 + 2HO$, according to the equation,

$$C_4H_5(NH_4)O_2 2SO_3 + CaO = (CaOSO_3 + 2HO) + \left.\begin{matrix} C_4H_5 \\ H_2 \end{matrix}\right\} N,SO_3, \dagger$$

but experiment showed that this was not the case, ammonia alone was disengaged.

Sodium.—The evolution of both deutoxide of nitrogen and nitrite of ethyl indicate that by the reaction of iron and acetic acid the nitric acid in combination with the ethyl may be deprived not only of one, but even of three atoms of oxygen. In view of this easy and far-reaching reduction, it is curious to find that nitrate of ethyl is scarcely acted upon by metallic sodium, which even retains its lustre for some time when immersed in the ether. If a few drops of alcohol be added to the nitrate of ethyl, the sodium attacks the alcohol, and when this is exhausted the sodium remains in the liquid for hours, as brilliant as a piece of silver.

Production of a Saccharoid Substance.—When nitrate of ethyl is made to act upon ammonia in sealed tubes, secondary products appear to be also formed. When the hydrochlorates of ethyl bases obtained in this way are evaporated down previously to final distillation with caustic alkali, they are brownish, and have a strong odour of burnt sugar. The residue, after distillation, was examined for sugar or allied substance, but it could not then be detected and the odour had disappeared.

Boutlerow has lately described‡ a remarkable reaction which indicates the probable production of a sugar for the first time by complete synthesis. It was obtained by the action of alkalies on dioxymethylene, $C_2H_4O_2$. It seems possible that dioxyethylene might, in the reaction of nitrate of ethyl on ammonia, be found in small quantities, and lead, when the crude contents of the pressure-tubes are first distilled with caustic alkali, to the formation of a substance analogous to Boutlerow's methylenitane in the ethyl series.—*American Journal of Science.*

On Ferruretted Hydrogen, by MM. Carius *and* Wanklyn.

This compound is not a gaseous body, and must not be confounded with Dupasquier's ferruretted hydrogen,—a gaseous compound, which, as has been since shown, is only a mixture of pure hydrogen, sulphuretted, phosphuretted, carburetted hydrogen, &c.; it is, on the contrary, solid, and occurs in the form of a black powder, gradually disengaging hydrogen when heated, but remaining tolerably stable at the ordinary temperature, in the absence of water.

The authors have not been able to determine the elementary composition of this hydride, but they have found that it behaves in the presence of distilled water as it does under the influence of heat, liberating pure hydrogen and forming protoxide of iron, which is perfecly soluble in hydrochloric acid, and forming with it protochloride of iron.

The authors obtained this hydride when seeking for an entirely different substance. They prepared it by treating iodide of iron, placed in ether at the bottom of a tube, by zinc-ethyl diluted with its volume of ether. A gaseous mixture is then disengaged containing ethylene, C_4H_4; the reaction is moderated with a freezing mixture. The black residue is washed with ether, and then constitutes the hydride of iron in question.—*Ann. der Chem. und Pharm.*, cxx. 69.

TECHNICAL CHEMISTRY.

On Mineral Oils and the Minjak Lantoeng of Java, by Dr. Bleekrode.§

At the commencement of the year 1861, the papers were filled with marvellous accounts of springs and wells yielding immense quantities of mineral oil in North America. In March of the same year, purified mineral

* By exposure over sulphuric acid *in vacuo* for several weeks, crystals of ethylosulphate of ammonia, near an inch in length and diameter, were obtained.

† Ethylamine is capable of forming an anhydrous carbonate, and probably also an anhydrous sulphate.

‡ *Bulletin de la Société Chimique de Paris*, No. v., Séance du 26 Juillet, 1861.

§ The Author, Professor at the Royal Academy at Delft, and one of the most distinguished scientific men in Holland, died at Delft on January 3 last. The present article, communicated by him to the *Repertoire de Chimie Appliquée*, was one of the last published by him.

oils from America, limpid as water, could be obtained at the establishment of Mr. James Aladden, of London, at 1f. 50c. the litre.

Mineral oils have as yet been little studied. Their chemical composition varies in the same degree as their origin, in the successive geological stages of the earth's crust. Mineral oils are not of the character of tar, but they much resemble in their composition the paraffin oils prepared by Mr. James Young's system, by the slow distillation, at a low temperature, of Boghead or Cannel coal. A specimen of mineral oil brought from Banjoemas, in Java, scarcely differs from this. But this resemblance ought not, however, to lead one to the hypothesis of attributing the origin of mineral oils to the distillation of coal, bituminous schists, or lignites, and certainly not to the opinion of American geologists, ascribing them to the decomposition of animal substances in the Devonian and Silurian epochs. Pure mineral oil may be completely distilled at temperatures between 360° and 400° C., and it would, consequently, require a depth of from 10 to 12 kilomètres to attain this intensity of terrestrial heat.

The mineral oil of Rangoon, obtained from wells in the vicinity of the river Irawaddy in the Burman Empire (Burmese naphtha, or Rangoon tar), has become an article of regular importation to England, amounting in 1857 to 29,811 litres, and in 1858 to 17,118 litres. Mr. Warren De la Rue and Dr. Hugo Müller have studied its chemical composition, and they have described it as a mixture of hydrocarbons, without any combination of oxygen. Rangoon oil contains 10 to 11 per cent. of paraffin; it has a density of 0·880 at a temperature of 40°, and then consists of a liquid as limpid as water. Mr. Gregory was the first to point out the identity of this substance, formerly designated by M. Christison under the name of petrolene, with the paraffin of Reichenbach.

This paraffin has received the name of *Belmontine*, from the name of the locality in which the manufactory is situated. It is the well-known ingredient of the stearic candles of Price's Candle Company. In this establishment the oil is distilled by steam; a mixture of hydrocarbons, having a density of 0·827 to 0·861, and boiling between 211° and 270°, separates without a trace of paraffin. This has been called *Sherwoodole*, and was recommended by the late Dr. Snow as an anæsthetic; it is also a solvent for caoutchouc, and can replace benzole for removing grease stains. The heavier oils are called Belmontine oils; they can be purified with sulphuric acid, a substance resembling asphalte separating. Distillation above 221° C. yields paraffin oils, which crystallise on cooling.

The pure paraffin has received the name of Belmontine. It is very remarkable that its fusing point is 60° C., whilst paraffin prepared from Boghead fuses between 42° and 48° C., and that prepared from turf at 47° C. We particularly notice this difference, since an error has arisen on this subject among recent writers who have noticed the account of M. Wagenmann, engineer at Neuwied (Dingler, *Polytechn. Journal*, clii. 312): this author published the notes of his visit to the manufactory of Price's Patent Candle Company. He commenced by calling the manufacturer "Belmont," and then proceeded to say that Belmontine fused at 28° C. M. Wagner, the editor of the German edition of "Muspratt's Chemistry," has repeated this error.

M. Vohl has published the analysis of a mineral oil brought from the East Indies (probably from Rangoon). The density was 0·885 at the temperature of 14° C. (*Dingler's Journal*, cxlvii. 374). Distillation and rectification produced—

40·705 per cent of photogene, or oil of 0·830 sp. gr.
40·999 per cent. of gazogene oil for lubricating machinery.
6·071 per cent. of paraffin fusing at 60° C.
4·600 per cent. of asphalte.
7·610 per cent. of loss.

The author includes among the substances constituting the *loss* the creosote and carbolic acid. There is reason to doubt the presence of these substances in true mineral oil. They have not been detected in the Java oils; Messrs. Warren De la Rue and Müller have not found them; also M. Haase, in decomposing the mineral oil of Carpathia, particularly notes their absence. It is therefore an especial characteristic of natural mineral oils, that they contain neither creosote nor carbolic acid.

The mineral oil of Gallicia, or Carpathia, was investigated by M. Haase in 1859; its density was 0·875. After a light, limpid oil, of a density of 0·74, was separated, the residue had a density of 0·83. Distillation produced—

50·0 per cent. of photogene, or oil of 0·815.
33·3 per cent. of solar oil of 0·850.
13·5 per cent. of red brown paraffin oil.

By rectification with sulphuric acid and potash, there were produced—

14·7 per cent. of loss.
33·7 per cent. of photogene of 0·810.
38·8 per cent. of solar oil of 0·845.
13·6 per cent. of paraffin oil of 0·875.

The mineral oils of Sehnde, near Hildesheim, in the kingdom of Hanover, have been studied by MM. Bussenius, Eisenstuck, and Helsmann, from the year 1858 to 1860.

We know of one analysis only (incomplete) of the natural mineral oils of North America, published by Mr. Dugald Campbell in the *Technologist* for March, 1861, p. 249. The density of American oil from Boston was 0·840 at a temperature of 16°. Fractional distillation produced four sorts of oils, of specific gravities 0·826, 0·838, 0·833, and 0·846; the boiling-point of the first was found to be 182° C. Mr. Campbell mentioned another specimen, of a density of 0·900; but that was impure, by containing water and earthy substances. Distillation produced equal quantities of oils of 0·867 and 0·872, with a loss of 7½ per cent.

Mineral oils are found in many islands of the Indian Archipelago. They are there known under the name of Minjak Lantoeng at Java, or Minjak Linji at Sumatra. As they are much used by the natives, they are regularly collected and sold in the markets of the towns and principal villages.

The localities where these oils rise spontaneously in natural fissures or artificial excavations, are ordinarily surrounded by warm or saline mineral springs; the earth belongs to the tertiary formation, at least the upper deposits below the alluvial strata. Volcanoes and volcanic rocks form the true geological constitution of the country.

A specimen of oil from Palantoengan, in the Residency of Samarang, has the consistency of tar, its colour resembles wood-tar; its density is 0·955 at 16° C.

A specimen of Tjiakijana, in the district of Pourbolingo, in the Residency of Banjoemas, is as liquid as water, with a deep green colour by reflection; its density is 0·804 at 16° C.

It is very remarkable that the deep green colour is only produced by reflection; a liquid layer, half a centimètre thick, is of a reddish brown, and a very thin layer is of a clear yellow, both by reflected and refracted

light. Concentrated sulphuric acid precipitates from it a black asphalte-like mass, the supernatant liquid then reflecting a beautiful apple-green colour, while the refracted light is yellow. This oil, without a particular or disagreeable odour, may be considered as a solution of paraffin in liquid hydro-carbons. At a few degrees above zero it acquires the consistency of butter, the paraffin crystallising. Young's paraffin oil, prepared from Boghead coal, has the same properties.

The mineral oil from Palantoengan of 0·955 specific gravity, contains only 3·8 per cent. of mineral substances, no chlorides, a little sulphate of lime, and the rest silicates. On treating it with one-tenth of sulphuric acid, according to Saussure, 15 to 18 per cent. of a black, asphalte-like mass, containing paraffin, is precipitated. Distillation at 320° C. produces a reddish-brown paraffin oil, and upon heating the residue above this temperature paraffin distils over almost pure.

It is very difficult to separate hydro-carbons of a constant boiling-point so as to isolate them. At temperatures between 190° and 230° nearly one-third separates as a slightly reddish yellow oil, having a density of 0·845 at a temperature of 16°, retaining, however, an oil boiling at 250°—260°. On raising the temperature to 350° C., one-fourth more oil distils over, forming a liquid of 0·918, containing much paraffin. The lightest oil distils at 80°—90°, beginning to volatilise at 60°; its density is 0·786; it is very clear, and has the characteristic odour of fetid lime-stone, or the lime-stone of the Belgian coal formation. The quantity of this oil amounts to 4 per cent; it dissolves iodine, phosphorus, caoutchouc, &c. Concentrated nitric acid changes it into a heavy, yellow, aromatic oil, smelling of cloves.

The other specimen from Tjiakijana consists of 40 per cent. of paraffin, and 60 per cent. of a clear oil of 0·780 specific gravity at the temperature of 16° C.; its boiling-point varies between 90° and 170° C. Spontaneous evaporation produces a mass of the consistency of yellow butter; distillation gives a residue of paraffin. The distilled oil also dissolves iodine, phosphorus, &c., and is changed by concentrated nitric acid into a heavy yellow oil smelling of cloves.

For want of a sufficient quantity of this remarkable oil, it has not been possible to study it in detail. I must, however, repeat that it is the first example of a natural solution of paraffin in oily hydro-carbons.

PHYSICAL SCIENCE.

On the Molecular Dissymmetry of Natural Organic Products, by L. PASTEUR, *Member of the Chemical Society of Paris.*

(*Continued from page* 150.)

THE announcement of the preceding facts naturally placed me in relation with M. Biot, who was not without doubts about their exactness. Charged with reporting upon it to the Academy, he caused me to go to his place to repeat the decisive experiment under his eyes. He sent to me the paratartaric acid which he had previously studied with particular care, and found to be perfectly neutral to polarised light. I prepared in his presence the double salt of soda and ammonia, which he also desired me to procure. The liquor was left in one of his closets to slowly evaporate, and when it had furnished about thirty or forty grammes of crystals he requested me to go to the College of France for the purpose of collecting and isolating, under his eye, by examining their crystallographic characters, the right and left crystals, desiring me to declare anew, if I affirmed that the crystals which I should place to his right would deviate to the right and the others to the left. That done, he said he would take charge of the rest. He prepared the solutions in strong proportions, and at the moment of observing them in the polarising apparatus he again invited me into his cabinet. He first placed in the apparatus the most interesting solution,—that which ought to deviate to the left. Even without any measuring, he saw, from the aspect alone of the tints of the two images, ordinary and extraordinary, of the analyser, that there was a strong deviation to the left. Then, with very visible emotion, the illustrious sage took my arm and said:—"My dear child, I have loved science so much all my life, that this makes my heart beat."

You will excuse, gentlemen, these personal recollections, which will never be effaced from my mind. In our times, with our habits, they should be excluded from a scientific memoir, but they seem to me proper in an oral exposition; and perhaps the biographic interest of similar recollections will constitute one of the advantages of the kind of teaching which the Chemical Society now inaugurates.

Besides, there is more here than personal recollections. The emotion of the sage in M. Biot was mingled with the internal pleasure of beholding his own anticipations realised. For more than thirty years M. Biot had vainly endeavoured to induce chemists to participate in his conviction, that the study of rotary polarisation afforded one of the surest means of obtaining knowledge of the molecular constitution of bodies.

Let us return to the two acids which furnish the two kinds of crystals so unexpectedly deposited by the crystallisation of the double paratartrate of soda and ammonia. Nothing, I repeat, is more interesting than the study of these acids. In fact, one of the two, that which is derived from crystals of the double salt hemihedric to the right, deviates to the right, and is identical with the ordinary tartaric acid. The other deviates to the left like the salt which furnishes it. The deviation impressed by these two acids upon the planes of polarisation is rigorously the same in absolute value. The right acid follows in its deviation particular laws which no active body had yet presented. The left acid offers them in the most faithful manner in inverse sense, without a suspicion of the smallest difference.

And the proof that paratartaric acid is the combination, equivalent to equivalent, of these two acids, is, that if we mix, as I am about to do before your eyes, solutions somewhat concentrated of equal weights of each of them, their combination is accompanied by a disengagement of caloric, and the liquid solidifies at once by an abundant crystallisation of paratartaric acid, identical with the natural paratartaric acid.*

Relatively to their chemical and crystallographic properties, whatever may be done with one of these acids can be repeated with the other under the same conditions, and in all cases we obtain identical products, but not superposable,—products which are as like as is the right hand to the left. The same forms, the same faces, the same angles, hemihedric in both cases. The only dissimilarity is in the right or left inclination of the hemihedric facettes, and in the direction of the rotary power.

It is manifest from all these results that we have to deal with two isomeric bodies, whose general relations of similitude and molecular dissimilitude are known.

* This beautiful experiment called forth applause from the audience.

Bear in mind the definition of chemical species just now mentioned, namely, the collection of all individuals identical by the nature, the proportion, and arrangement of elements. All the properties of bodies are the functions of these three terms, and the object of all our efforts consists in recurring by experiment on properties to the knowledge of these three things.

In isomeric bodies, the nature and proportion (of constituents) are the same. The arrangement alone differs. The great interest of isomerism has been to introduce into science this principle,—that bodies may be and are essentially different by that alone, that the arrangement of atoms is not the same in their chemical molecules.

But no isomeric bodies existed whose relations of molecular arrangements were known. This gap is filled for the first time by the discovery of the constitution of paratartaric acid and the relations of constitution of the right and left tartaric acids. We know, in fact, on the one hand, that the molecular arrangements of the tartaric acids are dissymmetric, and on the other that they are rigorously the same, with the sole difference of presenting dissymmetries in opposite directions. Are the atoms of the right acid grouped according to the spire of a dextrose helix, or placed at the summits of an irregular tetrahedron, or disposed according to such or such determined dissymmetric assemblage? We are unable to reply to these questions. But what cannot be doubted is, that there is a grouping of atoms according to an order dissymmetric to a non-superposable image. What is not less certain is, that the atoms of the left acid precisely realise the inverse dissymmetric grouping of this one. We also know that paratartaric acid results from the juxtaposition of these two groupings of inversely dissymmetric atoms.

From this moment the ascertaining the resemblances and the chemical and physical differences which correspond to these arrangements whose relations are known, affords particular interest and gives to the molecular mechanism assured bases. It permits us to establish the connection of physical and chemical properties with the molecular arrangement which determines their proper existence, or inversely it permits us to recur from properties to their first cause.

These general relations of properties and corresponding atomic arrangements may be summarily stated as follows:—

1. When the elementary atoms of organic products are dissymmetrically grouped, the crystalline form of the body manifests this molecular dissymmetry by non-superposable hemihedrity.

The cause of hemihedrity is then recognised.

2. The existence of this same molecular dissymmetry is translated beyond by the rotary optical property.

The cause of rotary polarisation is also determined.†

3. When non-superposable molecular dissymmetry is found realised in opposite directions, as happens in the two, right and left, tartaric acids and all their derivatives, the chemical properties of these identical and inverse bodies are rigorously the same; whence it follows that this mode of opposition and of similitude does not alter the ordinary play of chemical affinities.

I ought, however, to say that upon this latter point an important restriction should be placed, eminently instructive. Time would fail me to-day to develope it at leisure and properly. It will find a place in the next lecture.

† Fresnel, by one of those views of genius of which he has had so many, has in a manner presented this cause of rotary polarisation. He expresses himself thus in one of his Memoirs in the *Annales de Chimie et de Physique*, vol. xxviii., 1825:—"Rock crystal presents optical phenomena which cannot be reconciled with the complete parallelism of the molecular lines, and which would seem to indicate a progressive and regular deviation from these lines in the passage from one slice of the medium to the next."

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Lecture by Dr. W. Odling, *F.R.S.*, "*On Mr.* Graham's *Researches in Dialysis*," *delivered at the Royal Institution on Friday, February* 14, 1862.

(*Continued from page* 136.)

We now come to consider this property of diffusibility, as to whether or not it bears any definite relation to the other properties of bodies. Can we, from knowing the chemical or physical constitution of a body, predicate its diffusibility? Is, in fact, its diffusibility associated with any definite chemical or physical property which it possesses? Well, it certainly does not seem to be associated with chemical constitution in any way, as we find, for instance, whether we perform the experiment by means of jar or vial diffusion, that sugar, which is a very complex organic body, has much the same diffusibility as sulphate of magnesia, which is a simple mineral salt. It is also found that picric acid, which is very complex in its constitution, has almost the same diffusive power as common salt, which belongs to the most simple series of salts with which chemists are acquainted. We do find, however, that bodies which are isomorphous in form have much the same diffusibility; but that groups of equi-diffusive substances are much larger than those of isomorphous compounds. Take the common salt group, for instance: chloride of sodium, bromide of sodium, and iodide of sodium, which have a similar chemical composition, and are isomorphous in form, have a similar diffusibility; but nitrate of soda, which is dissimilar in composition, and heteromorphous in form, is nevertheless equi-diffusive with the members of the common salt group.

Going a little further, however, we find that although it is difficult to predicate of saline substances, which shall be the most diffusive, yet if we compare highly diffusive substances, such as salt, and sugar, and sulphate of magnesia, with feebly diffusive substances such as albumen and gum, and caramel and tannin, one broad distinction becomes apparent, which is that bodies which are readily crystallizable are also readily diffusible; whereas bodies which ordinarily exist in the amorphous state, and which, more especially, are characterised by their property of forming gelatinous hydrates,—such substances, for instance, as jelly and gum, and caramel,—are feebly diffusive; and this has led Mr. Graham to establish a distinction between bodies which differ thus from one another. They are divided by him into two classes,—crystalloids, or highly diffusive bodies, on the one hand, and colloids, or gelatigenous or feebly diffusive bodies, on the other. I shall have occasion to refer more particularly to the distinctions between crystalloids and colloids further on.

We have next to consider what is the nature of the force which impels the particles of salt, for instance, to spread themselves out among the particles of liquid? Of course we may attribute the action to a self-repulsion of the particles of the salt, on the one hand, and of the particles of the water, on the other; or, if we please, we may attribute it to a kind of attraction existing between the particles of the salt and the particles of the water,—a mutual attraction of the particles of the water for those of the salt, and of the particles of salt for those of the water; and, simply for the sake of explaining and colligating our phenomena, we will assume this latter hypothesis;

for I do not suppose that by attributing the action either to self-repulsion or mutual attraction, I am really doing anything towards explaining the causation of the phenomena. We will, then, assume diffusion to be an attraction of the particles of the salt for the particles of the water. On this understanding, then, we must say that the particles of a crystalloid have a much greater attraction for the particles of water than have the particles of a colloid substance. Now, if a salt really has this superior attraction for molecules of water, we can understand how it may be not only able to unite diffusively with free water, but also with water that is in a low form of combination as it exists in a soft solid, for instance, such as jelly. Jelly is a substance which contains no free water, no liquid water; the whole of the water existing in it is in a state of combination. The substance is a soft solid, not necessarily wet, for it does not wet the hand. Now, a crystalloid body, such as common salt can not only unite diffusively with free water, but it can also unite diffusively with water that is already in a state of loose combination in the form of a soft solid. Here is an experiment made in a precisely similar manner to our experiment on jar diffusion, except that the diffusion of chloride of copper in this instance, instead of being allowed to take place into free water, is allowed to take place into a soft solid. This is a jelly made of Japanese gelose; and here, you see, we have a diffusion of the green chloride of copper taking place into the mass of jelly precisely as it did into the water. The table shows the diffusion of chloride of sodium into gelose; here are the results. Without going into all the details there given I may say this much, that the diffusion of common salt into this substance gelose is almost as great as into free water. At any rate, the diffusion of common salt into gelose in eight days is greater than the diffusion of common salt into water in seven days. But although the introduction of gelose, or a gelatinous substance, does not interfere in any appreciable way with the diffusion of a crystalloid body, it arrests almost completely the diffusion of a colloid body. A colloid body has very little tendency to unite diffusively, even with free water, and is quite incapable of uniting diffusively with water in any kind of combination, however feeble.

We have already seen that by diffusion into water we may effect the partial separation of a highly diffusive substance, such as common salt, from a feebly diffusive substance, such as albumen; but we may effect a much more complete separation if, instead of using water, we cause the diffusion to take place into or through a soft solid; for such a substance, as I have said, scarcely impedes the diffusion of the crystalloid, but almost arrests the diffusion of a colloid. For instance, if we have a soft solid, such as a piece of vellum, or a piece of animal membrane, on a layer of mucus, interposed between a colloid solution and a quantity of water, the colloid, in order to get into the water, must pass through the membrane,—must unite diffusively with the combined water of the membrane itself, and that it is incapable of doing A crystalline body can pass through, and a colloid cannot. Now, this constitutes dialysis. Dialysis is the separation of a crystallised body from a colloid body through a membrane which will allow the crystalloid to pass through, but will not allow the colloid, because he crystalloid has a strong power of uniting diffusively with water, whereas the colloid has a very feeble power. The crystalloid can unite diffusively with the water that is in combination with the soft solid septum, so as to reach the external water, but the colloid cannot.

We will now refer to the experiment with which we began the lecture. Into this dialyser we introduced some Magenta, which is a crystalline body, and you perceive that a diffusion through the membrane into the external water has taken place to a very considerable extent. Into this [calling attention to the second jar] we introduced the red colouring matter of blood, and here there is not the least evidence whatever that any colour has passed through. Those who are sitting on my left hand may perhaps see an apparent coloration, which arises from the colouring matter contained in these two other jars which are standing near being seen through; but not the least shade of colouring matter has really passed through the membrane in this instance. Now, in this case [reverting to the third dialyser which had been charged at the commencement of the discourse] we took a mixture of the red crystalline body Magenta, and the brown colloid or non-diffusible body caramel; and you will find that whereas there is no browning whatever of the liquid in the jar a considerable quantity of the crystalline body has already passed through the dialyser and will continue to pass through. The membrane arrests the caramel and allows the Magenta to pass.

I want to impress upon you that this process of dialysis is altogether different from filtration. Filtration refers to the passage of masses through appreciable pores. Here there are no pores. The phenomena of dialysis are not molar but molecular. I may be able to show you that, by a very simple contrivance which we have here. Two small gas jars have been fitted up, one as a dialyser, with an impervious septum,—impervious to the passage of liquid mechanically; the other with a septum of filtering paper. In this last case only, we shall get filtration. We will first pour a little liquid into this colloid septum, and on passing the hand beneath it we find it perfectly dry; there is no water passing through. The membrane, as a mere membrane, is impervious to the passage of liquid. It will allow chemical action to take place, or that low form of chemical action which we have spoken of as diffusion, but it will not allow of filtration. Now this one [the other small jar] is covered with thick filtering paper, and we will pour the water into it in a similar manner, and you see in this case we have filtration or a mechanical passage of masses of water through the pores of the paper, whereas in this case (referring to the former) there is no passage whatever.

I would here direct your attention for a few moments to some of the results of dialysis. This (pointing to the table given below) represents the dialysis through parchment-paper, for twenty-four hours, of 10 per cent. solutions of the different substances shown in the first column:—

	Grammes diffused.
Gum arabic	0·029
Starch sugar	2·000
Cane sugar	1·607
Milk sugar	1·387
Mannite	2·621
Glycerine	3·330
Alcohol	3·570
Starch sugar (Second Experiment)	2·130
Chloride of sodium	7·500

These experiments were made in the order shown, with one and the same dialyser which was constructed in this manner (referring to a gas-jar-dialyser on the lecture table). After each experiment the dialyser was put into water for some time, and the septum thoroughly washed. You see from the table that gum-arabic scarcely diffused at all—at any rate it yielded less than 3 hundredths of a gramme of diffusate, while under precisely the same conditions, the starch sugar yielded exactly two grammes of diffusate. The quantity of cane-sugar that passed through was rather less than that of the starch-sugar, of the milk-sugar rather less than of the cane-sugar; of the mannite again, rather more, and of the glycerine and alcohol still more. And now the experiment was repeated with starch-sugar, in order to see whether any alteration had taken place in the septum of parchment-paper through which all these dialyses had taken place, and you perceive that practically there was no alteration in the result. Then, chloride of sodium was employed, from the 10 per cent. solution of

which seven and a-half grammes of salt diffused, showing how much more readily that body passed through than did the gum-arabic and the substances previously experimented upon. Other dialytic septa than parchment-paper may be employed, a layer of mucus spread between two folds of calico for instance, with scarcely any difference in the relative diffusibilities being manifested.

We will now turn our attention to some of the various colloid bodies which Mr. Graham has succeeded in preparing by dialysis, and some of his results are really most extraordinary — most wonderful. Here is a substance existing obviously in the crystalline form, namely, quartz or silica. But this body, in common with very many others, is capable of existing in the colloid as well as in the crystalloid form, as we shall presently see. Now, chemists by means of strong alkalies and acids are capable of getting this quartz into solution, and have been for many years past; but Mr. Graham has shown us how to obtain a solution which contains nothing but quartz and water. He took silicate of soda, a soluble crystalline salt, made by fusing quartz with carbonate of soda at a red heat, which, in common with other crystalloids, diffuses very readily. He acidified the aqueous solution of this salt with hydrochloric acid, whereby the state of the constituent silica was changed from the crystalloidal into the colloidal. The acidified solution was then placed on a dialyser, which was floated on a basin of water, and, after twenty-four hours, 36 per cent. of the hydrochloric acid had passed through, while, after four days, not a trace of either hydrochloric acid or chloride of sodium was left. Nothing but silica and water remained on the dialyser,—nothing but an aqueous solution of this hard substance quartz. Here is some of the liquid so obtained, the solution of sand in water. The colloidal silica procured in this way, or co-silicic acid as it has been termed by Mr. Graham, affords a very good illustration of the properties of colloidal bodies in general, of their non-crystalline habit, of their low diffusibility, of their chemical inertness and high atomicity, and above all of their mutability. The 5 per cent. solution of silica obtained at once by dialysis, is quite transparent and mobile. It may be boiled in a flask and considerably concentrated without undergoing any obvious change; and even a 14 per cent. solution is perfectly limpid. But aqueous co-silicic acid, in common with all colloidal solutions, after a longer or shorter time, according to its purity and dilution, undergoes a spontaneous change. It becomes at first opalescent and viscous, and eventually sets into a firm, insoluble jelly. Hence, Mr. Graham speaks of two colloidal states, the peptous, or dissolved, and the pectous, or gelatinised. The jelly of silica gradually shrinks and exudes pure water. When dried *in vacuo* over oil of vitriol at ordinary temperatures, it forms a lustrous, transparent, glassy mass, which, however, is not anhydrous. The residue left by its ignition has a specific gravity of 2·2, that of crystalline silicic acid being 2·6 It seems probable that the colloid may become gradually changed into the crystalloid form of silica. The pectization of a solution of co-silicic acid may be produced at will. When the liquid is boiled, for instance, in an *open* vessel, a ring of insoluble silica forms round the margin of the liquid and causes the whole to gelatinise. Again, pectization is effected in a very short time by the addition of a solution containing even $\frac{1}{10,000}$ of any alkaline or earthy carbonate. I have here a solution of colloid silicic acid, that is a solution of metamorphosed sand in water. I now add to it a little carbonate of potash solution, and in performing the experiment here, upon a large scale, I will add an excess of the carbonate in order to accelerate the action. The liquid is at present gelatinising perceptibly. We will leave it quiet for a few moments, and you will then see what an alteration has taken place in its character. Now, our jelly is formed. If I were to give it a little more time it would set more firmly, but as it is I shall be able to show you, by inverting the flask, that the contents are no longer liquid, but gelatinous. (The flask containing the gelatinised silica was here inverted without any spilling of its contents.)

In its chemical relations the solution of co-silicic acid, in common with other colloid liquids, is characterised by its inertness. It does not manifest any decided chemical properties, and its reactions, such as they are, take place very slowly. It enters into its combinations, moreover, with a very high atomic weight, so as to form a very high per-centage of the compounds which it forms. Thus, its solution when tested with delicate litmus paper shows but a very feeble acidity, not greater than that of carbonic acid water. When placed in the mouth it is at first quite tasteless, and we can readily understand why this should be — for the substance being colloidal cannot dialyse through the colloid membrane of the tongue so as to affect the gustatory nerves. But although quite tasteless at first, it produces, after some time, a sort of acrid feeling in the mouth, owing, doubtless, to some alteration in the state of the silica. The acid reaction of 100 parts of dissolved co-silicic acid is neutralised by 1·8 parts of anhydrous potash, and by corresponding quantities of soda and ammonia. When added to an excess of lime water, co-silicic acid produced a gelatinous precipitate containing six per cent. of the base. These co-silicates are, however, very variable in their composition and very unstable. Co-silicic acid, moreover, gives precipitates with several colloid solutions, those of alumina, peroxide of iron, gelatine, and albumen for instance. Its action on gelatine closely resembles that of tannin, which is a similar colloid acid. Thus a piece of skin immersed in a weak solution of co-silicic acid becomes after a time thoroughly tanned or converted into leather; and the process of tanning with co-silicic acid further resembles that of common tanning in the very gradual manner in which the combination takes place, and in the indefinite character of the compound produced. But my time is getting on so rapidly that I must not devote any more of it to silica. Here, however, is another of these colloid solutions to which I should like to direct your attention for a few moments, because it is the newest of them all. In Mr. Graham's paper in the *Philosophical Transactions* he observes, that though he was able to obtain stannic acid, the analogue of silicic acid, in the colloidal state, he could only get it in the pectous, not in the soluble, form of the colloidal state. Precipitated peroxide of tin or stannic acid, though insoluble in water, is readily soluble in a solution of bichloride of tin; when the liquid so produced is submitted to dialysis the whole of the chlorine passes away, but a jelly instead of a solution of stannic acid is left on the dialyser. But Mr. Graham has since found that by acting upon this jelly with a dilute alkali, and then submitting it to a further dialysis, it gradually becomes liquid. This last process he terms peptizing or digesting. Here then we have a solution of co-stannic acid, which has been obtained by peptization, and which we can now very readily pectise by adding a minute quantity of hydrochloric acid. Here you see we get the pectization. The whole liquid is becoming perfectly gelatinous, and this soft amorphous form of stannic acid contrasts remarkably with the hard crystalline native tinstone which I hold in my hand.

I will next bring under your notice the aqueous solution of colloidal peroxide of iron. This oxide, in the native form, is known as hæmatite, a specimen of which I have here before you. Now, this oxide, under ordinary circumstances, is quite insoluble in water, though readily soluble in acids. On dissolving it in hydrochloric acid, for instance, we obtain a solution of perchloride of iron, a crystalline body which diffuses with the greatest facility. But when hydrated peroxide of iron is digested in this solution it becomes gradually dissolved, and we have formed a basic colloidal chloride, its basicity being probably dependent on its colloidal nature, which we know produces a high atomicity. Now, when this liquid is

dialysed, the hydrochloric acid passes away, and a pure aqueous solution of peroxide of iron is left on the dialyser. And mark this curious action. When we have the oxide of iron dissolved in an equivalent quantity of hydrochloric acid, the crystalloid chloride of iron is formed, which dialyses without decomposition, the oxide not separating from the acid. But when we introduce a larger quantity of oxide of iron, we form a colloid compound, and the hydrochloric acid can then dialyse completely away from the oxide, which will remain in aqueous solution. With a small quantity of oxide, the acid and base go through together; with a larger quantity of oxide, the acid passes away, and the colloid base is left behind. Here I have a colloidal solution of peroxide of iron which was prepared in the manner I have just described to you, and which I will now proceed to pectize by the addition of a little common salt. If I were simply to pour some of this colloidal liquid into a glass, and then add the reagent to it, there would be no effect recognisable at a distance; but I shall, I hope, be able to render the result visible to all of you by means of the electric lamp which Dr. Tyndall has kindly arranged for me. We have the solution of colloidal oxide of iron in this glass tray; and I wish first to show you its colour and transparency. [A beam of light was thrown through the solution on to a screen.] You see that the liquid has an orange-brown colour, and is perfectly transparent. I will now add a little common salt, and you perceive that it instantly produces a perfect pectizing or gelatinizing of the mass.

While we have the electric lamp in order, I will direct your attention to another colloidal substance. I have here some Prussian blue, a compound that is usually quite insoluble in water. It is, however, capable of being dissolved very readily in oxalic acid, and when the oxalic acid solution is allowed to dialyse, the oxalic acid passes through and the Prussian blue is left in the form of an aqueous colloidal solution, capable of being gelatinised like the silica and oxide of iron solutions. In this tray we have our solution of colloidal Prussian blue (a beam of light was then thrown through it on a screen). You perceive it is of a blue colour and perfectly transparent, ready, however, to pectize on the addition of a small quantity of some foreign substance. We will take, in this instance, a drop of dilute sulphuric acid. We are operating with a somewhat weak solution in order to allow the light to pass through, and the pectization of a weak solution is not instantaneous. But in the course of a minute or so it will, I have no doubt, be visible. Even now it is beginning. The pectization is gradually taking place, and you can now see clearly that the liquid is no longer homogeneous, but that the precipitated colloid or gelatinous prussiate of iron exists in suspension throughout. The appearance will become more characteristic in a moment or two. (Here the changes became clearly perceptible to the audience.) In the one moment that remains to me I will show you another experiment of a similar description, made with a very beautiful substance, not the blue prussiate of iron, but the red prussiate of copper, which is, under ordinary circumstances, an insoluble compound. It is readily thrown down by adding a little prussiate of potash to a salt of copper, as you perceive. On dilution, the colour and character of the precipitate are rendered very apparent. Here we have the prussiate of copper thrown down of a chocolate red colour. If we pour the whole on to a filter, a colourless liquid passes through as you see, while the chocolate body being insoluble remains on the filter. But this body, though ordinarily insoluble in pure water, is soluble in a solution of oxalate of ammonia, and when this mixed liquid is submitted to dialysis, the oxalate of ammonia passes through the membrane, while this curious liquid, an aqueous solution of red prussiate of copper, remains on the dialyser. This solution has some of the characters of Mr. Faraday's gold liquids; although the great mass of the prussiate is in a state of real solution, a minute portion seems to be only in the state of suspension, and hence a curious red opacity is manifested when the liquid is examined by reflected light. I shall be able to show you the colour and transparency of the solution by means of the electric lamp (a beam of light was here thrown through the liquid). At present you perceive the solution is perfectly transparent, homogeneous, and of a splendid red colour. But this body is pectized with very great ease, and on adding a drop of dilute sulphuric acid you see an immediate formation of large flocks of gelatinous substance, which now completely obstruct the passage of light, whereas the clear solution, as you saw, allowed light to pass through it with the greatest readiness.

Now, this discovery by Mr. Graham of dialysis, of the capability which some bodies have, and others have not, to pass through membranes, and of the broad distinctions between the two classes of bodies, bids fair to open out an entirely new field of inquiry. The constant intervention of colloid septa in so many of the phenomena of animal and vegetable life, gives to the subject of dialysis a high physiological interest, and it will, doubtless, exercise an important influence in the progress of physiological research. It will also serve to explain many of the phenomena of inorganic Nature, more especially in relation to bodies which are now crystalline, but were once colloidal. It will also be of the greatest service in its applications to toxicology, pharmacy, and practical chemistry, by enabling us to effect a most perfect separation of different substances which could not be separated by any other means. It is not for me to express the obligation which science is under to Mr. Graham for investigating this important subject so laboriously and successfully, but I cannot conclude without expressing publicly my personal thanks to him for the opportunity he has given me, and the pleasure he has afforded me, of bringing this subject under your notice, and for his kindness in providing me with these very beautiful illustrations of dialytic phenomena.

SOCIETY OF ARTS.

*Wednesday, February 5.**

Dr. A. W. Miller, *F.R.S., Professor of Chemistry at King's College, in the Chair.*

On this occasion Dr. Crace Calvert read a lengthy but valuable Paper, "*On Improvements and Progress in Dyeing and Calico-Printing since* 1851." We may cull much interesting matter therefrom, which will be, perhaps, best presented in the form of a condensed review.

After some introductory observations, Dr. Calvert commenced his list of dyestuffs with a notice of madder and the several preparations derived from this well-known root, much stress being laid upon the researches of Dr. Schunck, which prove, Dr. Calvert considers, the principle called *Rubian* to be the real and ultimate source of the colouring matter, or *Alizarine*. According to this theory, —for at present it cannot be rationally looked upon in any other light,—but a small portion of *alizarine* is naturally present in madder, the rubian it contains, being however readily converted into the former under the influence of *erythrozym*, a peculiar ferment, or of alkalies or acids, according to the following equation:—

$$\underset{\text{Rubian.}}{C_{56}H_{34}O_{30}} = 4\ \underset{\text{Alizarine.}}{(C_{14}H_3O_4)} + 14\ \underset{\text{Water.}}{HO}$$

"This result," the Lecturer proceeded to say, "satisfactorily explained the change of madder into garancine by the action of sulphuric acid on that root, from the fact that rubian was susceptible of conversion, under the same

* Notices of Papers read at earlier meetings during the present Session have not appeared in this Journal, the subjects on which they treated not being within the province of the Chemical News.

influences, not only into alizarine, but also into two valueless substances, called rubiretine and verantine. This led Dr. Schunck, in conjunction with Mr. Simon Pincoff, in 1852, to the production of a most important dyeing material, called by them *commercial alizarine*. But, to enable you to understand in what this product differs from garancine, and also its mode of preparation, it is necessary that I should state that the verantine and rubiretine are not colour-giving principles, and that they interfere with the beauty and brightness of the fine shades of purple given by alizarine, which, according to Dr. Schunck, is the only colour-giving principle contained in madder.†

"*Garancine*, which, even before 1851, was extensively used for producing red, purple, and chocolate upon calico, was obtained, as you are aware, either by mixing together at an ordinary temperature equal weights of madder and sulphuric acid, then adding water, when the garancine was produced, requiring only to be thoroughly washed so as to remove the acid;—or by mixing the roots with one-third their weight of sulphuric acid previously diluted with water, and carrying the whole to the boil for one or two hours, washing the residue repeatedly, and using, in the last operation, some alkaline carbonate. Although garancine thus prepared gave colours similar to madder, yet they were wanting in solidity. This effect, especially as regards purples, was overcome by Messrs. Pincoff and Schunck, by taking principally garancine prepared as above, but thoroughly depriving it of acid, and submitting it to the action of high-pressure steam, when the substance called verantine is decomposed or modified so as not to interfere with the purple dyeing power of alizarine. The advantages possessed by this product, which is now so extensively employed in calico-printing, that several millions of pieces have been dyed with it, are, as stated by Messrs. Pincoff and Schunck, as follows:—It produces good lilacs economically and without soaping; great promptitude and regularity in the production; facility of producing combination of lilacs with catechu, lilac and chocolate, and lilac and orange, which results cannot be obtained so satisfactorily with madder or garancine; production of lilac shades graduated *ad libitum*. Lastly, economy of mordaunts. I shall again refer to this in speaking of calico printing."

Dr. Calvert next proceeded to the improvements of Mr. Lightfoot in the manufacture of garanceux, and also M. Emile Kopp's process for producing alizarine from garancine, by subjecting the latter to the action of saturated and super-heated steam, which carries off the colouring matter, so that both can be condensed together. The "*flower of madder*," of which MM. Julian and Roquer (introduced in 1852) is prepared by causing the ordinary roots to ferment, and then washing away all soluble matters with water. The patent process of Mr. E. Meklow is in many respects similar to the preceding. Some interesting details following here, we cannot do better than quote the Lecturer's own words:—

"M. Emile Kopp has published a very interesting paper on the subject of madder, in which he proves that, if the madders of Alsace are treated with a weak solution of sulphurous acid, and to this solution, which has a fine golden colour, he adds three or four per cent. of hydrochloric acid, and heats the whole to a temperature of 150°, a red colouring matter is precipitated, which he states to be pure *purpurine*. If, after the separation of this precipitate, the liquor is again carried to the boil, a new colouring matter is formed, which he calls *green alizarine*, and finds to be composed of pure alizarine and a dark green resin, which he considers is produced by the decomposition of chlorogenine.

"As a *résumé* of my observations on madder, I may state that the only two madder products which have received extensive application since 1851 are commercial alizarine and flower of madder.

"*Indigo*.—I have the pleasure again to draw your attention to a series of researches by Dr. Edward Schunck, and to enable you to appreciate the value of his discoveries in connexion with this important dyestuff, it is necessary that I should first state that chemists held two differents opinions as to the condition in which the colouring matter existed in the indigo plants. Thus, Chevreul, Girardin, &c., considered that the indigo contained in the plant was in the form of white, or de-oxygenated blue indigo; whilst Giobert and others believed that it did not pre-exist in the vegetable, but was formed during the process of fermentation, which is usually employed for the extraction of the colour from the *Isatis tinctoria* and *Indigofera anil*. Serious doubts having arisen in the mind of Dr. Schunck, whether either of these theories correctly explained the state in which indigo existed in the indigo plant, he undertook a long series of researches, by which he was enabled to show, with a positive certainty, that the *Isatis tinctoria* contains a substance easily soluble in hot and cold water, alcohol, and ether, and which, by the action of strong mineral acid, yields indigo blue. Further that the formation of the colouring matter from it can be effected without the intervention of oxygen or of alkalies, and that the latter, indeed, if allowed to act upon it before the application of acid, entirely prevents the formation of the colouring matter; viz., indigo blue. To ascertain whether the substance which he calls *indican*, pre-existed in the plant, Dr. Schunck operated as follows:—He digested in ether some perfectly dry leaves of the *Isatis tinctoria*, removed the ethereal solution, and having exposed it to spontaneous evaporation, it left a green syrupy residue, from which water extracted *indican*, for by the action of boiling sulphuric acid, it yielded an abundance of indigo blue. To obtain the *indican* in a high state of purity, he found it necessary to treat the leaves with alcohol and ether, and to submit the extract to various chemical operations, to get rid of all impurities, so as to obtain *indican* as a yellow transparent glutinous substance, of a slightly bitter and naseous taste. This substance presents the remarkable property (similar to that of rubian in madder) of being susceptible under the influence of a ferment in the plant, or of acids, of yielding indigo blue and sugar, as seen by the following chemical formula:—

$$\left.\begin{matrix}1\text{ eq Indican } C_{52}H_{31}NO_{34}\\ 4\text{ eqs Water } \quad H_4\ O_4\end{matrix}\right\} = \left\{\begin{matrix}C_{16}H_5NO_2 \text{ 1 eq Indigo blue}\\ C_{36}H_{30}\ O_{36} \text{ 3 eqs Sugar}\end{matrix}\right.$$

$$\overline{C_{52}H_{35}NO_{38}} \qquad \overline{C_{52}H_{35}NO_{38}}$$

"To obtain this interesting decomposition with acids, it is simply necessary to heat the *indican* with strong sulphuric or hydrochloric acid, when the indigo blue precipitates, while the sugar remains in solution. But *indican* is so liable to undergo modifications, that if the action of the acids be continued, besides the indigo blue an indigo purple is formed, called by Dr. Schunck *indirubine*."

Dr. Calvert followed up this section of his subject by giving a kind of *résumé* of the indigo manufacture, which, for the readers of the Chemical News, may be safely omitted. Orchil, and its obvious colouring principles, was next noticed: we are all, however, sufficiently acquainted with the researches of Schunck, Kane, Stenhouse, and, more recently, the experiments of M. Marnas (of the firm of Guinan, Marnas and Bonnet, of Lyons), which latter resulted in the production of the beautiful and well-known *French purple*, in 1856—the same year which, curiously enough, saw the discovery of *aniline purple*, by Mr. W. H. Perkin.

A brief, and certainly imperfect notice of catechu was then presented; the aloe colours, and the lac dye, of Messrs. Haworth and Brooke, obtained by extracting lac with ammonia, and precipitating the colouring matter, which is a fine red.

† Dr. Schunck also obtained as products of decomposition of rubian, rubianine and sugar. Those who are interested in these chemical researches will find them fully detailed in the *Transactions* of the Royal Society.

As regards chlorophyll, So-kow murexide, and last, but most decidedly not least, the coal-tar colours, Dr. Calvert shall speak for himself:—

"*Chlorophyll.*—For many years attempts were made to fix upon fabrics the green colouring matter of leaves, but unsuccessfully, until, in 1854, MM. How, and Mann, and Cordillott, of Mulhouse, succeeded, by the following simple process, in obtaining on silk, wool, and cotton, fine green, brilliant, and solid colours:—After having boiled a quantity of grass so as to remove everything soluble in boiling water, it was heated with a hot caustic lye of 1·03 sp. gr.; this alkaline solution being then neutralised with hydrochloric acid, a fine green precipitate was thrown down. This precipitate was then dissolved in a solution of caustic lye, to which had been previously added some phosphate of soda and oxide of tin. This mixture, properly thickened with gum, was printed and fixed by steaming. No doubt, by a slight modification in the *modus operandi*, this colour might be applied to dyeing.‡

"*Chinese Green, called Lo-kao.*—In 1851 and 1852, public attention was drawn, by several English gentlemen, to samples of a green colouring matter, imported from China; and in 1853, Messrs. Guinon, of Lyons, imported such quantities of the material as to enable them to dye silks for the requirements of the trade. The silks so dyed by them, under the names of *Vert-Venus*, *Vert-Azof*, and *Vert-Lumière*, were especially admired, from the beautiful green shades they assumed in artificial light; and although the price of the dye fell from 21*l.* per pound in 1853, to 4*l.* in 1860. these beautiful shades of green (especially under artificial light) have almost disappeared from the market, owing to the two following reasons:—First, their want of stability; and, secondly, because Messrs. Guinon, Marnas, and Bonnet have found the following means of producing, at less cost, shades of green which also maintain this character under the influence of artificial light, *i.e.* by first dyeing their silks in Prussian-blue, and then dyeing them in an acidulated bath of carboazotic, or picric acid. It is an interesting fact to observe that, while the greens produced with indigo and picric acid appear blue in artificial light, those produced as above with Prussian blue and picric acid appear green under the same conditions. I cannot leave this interesting subject without making two further remarks:—First, Lo-kao is the only substance with which I am acquainted capable, with proper reagents, of producing the seven colours of the spectrum; secondly, that, thanks to the advanced state of chemical and botanical science, we have succeeded in producing, in Europe, the identical substance imported only a few years ago, as a great novelty, from China—and for which, but for those sciences, we should still probably have remained tributary to that Empire. Thus M. Charwin, of Lyons, has been able to obtain Lo-kao from a weed indigenous to Europe, viz., *Rhamnus catharticus*, for which he has received, from the Chamber of Commerce of Lyons, a gold medal worth 6000 francs.§

"*Murexide, or Roman Purple.*—The colour to which I am now about to draw your attention furnishes another example of the assistance which the progress of chemical science has rendered to the art of calico printing. In 1776 the illustrious Swedish chemist, Scheele, discovered, in human urine, uric acid. In 1817, Brugnatelli found that nitric acid transformed uric acid into a substance, which he called *erythric acid*, but which was subsequently called, by Wöhler and Liebig, *alloxan*. In 1818, Dr. Prout found that the latter substance gave, when in contact with ammonia, a beautiful purple red colour, which he called purpurate of ammonia—the product known by the name of *murexide* since the researches of Liebig and Wöhler, published about 1837. These discoveries remained dormant in the field of pure science until the year 1851, when Dr. Saac observed that when alloxan came in contact with the hand it tinged it red. This led him to infer that alloxan might be employed to dye woollens red, and further experiments convinced him that if woollen cloth were prepared with a salt of tin, passed through a solution of alloxan, and then submitted to a gentle heat, a most beautiful and delicate pink colour resulted. In 1856, MM. Depouilly, Lauth, Meister, Petersen, and Albert Schlumberger, applied it as a dyeing material to silk and wool, and succeeded in obtaining red and purple colours, by mixing the murexide with corrosive sublimate, acetate of soda, and acetic acid. For printing, a mixture of murexide with nitrate of lead or acetate of zinc, properly thickened, is applied on cotton fabrics, which are then allowed to dry for a day or two, when the colour is fixed by passing them through a mixture of corrosive sublimate, acetate of soda, and acetic acid. The Roman purple style of printing has been carried out extensively by Messrs. Edmund Potter and Co.; Boyd, Sons and Hamel, and James Black and Co. No doubt you would wonder whence such quantities of uric acid, or murexide could be drawn to supply a demand like that which has arisen. This result has been achieved by the following process of extracting uric acid from Peruvian guano. Guano is treated repeatedly with hydrochloric acid, until all soluble matters are removed by heat and washing. The insoluble mass, which consists chiefly of sand and uric acid, is carefully treated with nitric acid of specific gravity of 1·40. When the action of the acid is completed, the mass is treated with warm water, and thrown on a filter. The filtrate which has a yellowish colour, and contains alloxan, &c., is evaporated carefully to such a degree, that when left to cool it becomes a brownish red or violet solid, called by the inventor, *carmin de pourpre*, which is the substance chiefly used for printing, as above described. It is to the enterprising commercial spirit of Mr. Robert Rumney, chemical manufacturer, of Manchester, that is due the extensive production and application of murexide in this country.

"Doubtless you are aware that alum and cream of tartar are used largely as mordaunts in the dyeing of silk, wool, and cotton, and that the latter substance has much risen in price, owing to the failure of the wine crops of late years; therefore, any process for economising the use of cream of tartar is a matter of importance. I am happy to state that Mr. Kuhlmann has, within the last few weeks, published in the 'Memoirs of the French Academy,' a paper in which he furnishes a means of attaining that end. Having first confirmed a most important observation of Mr. Chevreul's, viz., that when cream of tartar is used as a mordaunt it is decomposed into tartaric acid, which adheres to the fibre, and into a neutral tartrate which remains in solution and is lost, and that if, on the contrary, instead of using the cream of tartar as a mordaunt, it is first decomposed into tartrate of baryta, and that this salt be used as a mordaunt, in connexion with a little hydrochloric acid, the two equivalents of the tartaric acid of the cream of tartar become available, and consequently a saving of one-half the quantity of cream of tartar formerly used is effected.

"In the hope that it may prove interesting to the members of this Society, I will now give some details respecting a few new processes for dyeing silk, before proceeding to treat of coal-tar colours.

(To be continued.)

‡ Any one who wishes for further information on the green colouring matter from plants, will find a most interesting Paper by M. Fremy, published in the *Comptes-Rendus* of the French Academy, for 1860, volume l., in which that chemist shows that chlorophyll is composed of two colouring matters called phylloxanthine and phylloxcyanine.

§ For full details on this subject, see Report presented to the Chamber of Commerce, at Lyons, by the Rev. M. Hélot, M. Persoz, &c.

PHARMACEUTICAL SOCIETY.

Wednesday Evening, March 5, 1862.

Mr. Peter Squire, *President, in the Chair.*

The first Paper read was "*On the Manufacture of Kelp*," by Mr. E. C. Stanford, F.C.S. The manufacture of kelp, he said, was begun about the middle of the last century, principally for the sake of the carbonate of soda it contained; but when the high duties were removed from foreign barilla, British kelp deteriorated in value, and was scarcely worth making. At the beginning of this century, Highland kelp was worth from 20*l.* to 22*l.* a ton, and the produce of Scotland was 20,000 tons per annum. When the duty was taken off foreign barilla, the price of Scotch kelp fell to 3*l.* a ton, and very little was made. The discovery of iodine by Courtois in 1812, and the extensive applications which that body has since received, have given a fresh impulse to the manufacture of kelp, at present the only commercial source of iodine. The present method of manufacturing kelp is very imperfect. Carried on in remote districts of the Hebrides, and on the coast of Donegal, it has never received the attention it demands, and the object of the Author is to improve the method by which it is obtained. In Scotland at present the weeds are collected, dried in the sun, and then burned in shallow pits, the fire attaining a very high temperature, so that the salts are fused, and the mass forms a vitreous conglomerate, which is broken up by throwing water on it. In Ireland the weed is burnt in small heaps at a lower temperature, and the result is a more porous mass, which is richer in iodine than the Scotch kelp. Professor Graham pointed out that Guernsey kelp is the richer source of iodine, in consequence, Mr. Stanford said, that the *Laminaria digitata* is the only weed burnt in that island. The Author then read some statistics showing the amount of kelp produced in Scotland to be 4350 tons, and in Ireland 6080 tons annually, the value of which may be estimated at 40,000*l.* This quantity is the produce of about 200,000 tons of wet weed. In France about 24,000 tons are made, mostly on the coast of Normandy. The lixiviation of British kelp is carried on principally at Glasgow, where a single lixiviator, Mr. Patterson, operates on as much as 8000 tons a-year. The process is as follows:—The solution obtained by lixiviation is evaporated; sulphate of potash first deposits in small crystals, and then chloride of sodium. These are collected separately, and then the solution is cooled in iron pans, where, after two or three days, crystals of chloride of potassium are obtained. This process is repeated with the mother liquor, and after the second deposit of chloride of potassium, the dark-coloured mother liquor (containing sulphides and iodides) which remains is treated with oil of vitriol. Sulphur deposits, from which the liquor is decanted, and then distilled with binoxide of manganese to obtain the iodine. The method of obtaining the iodine has been improved by Messrs. Stephanelli and Doveri, who evaporate the liquor to dryness, mix the residue with binoxide of manganese, and distil off the iodine from earthen retorts. By this means the use of sulphuric acid is avoided. The principal disadvantages of the present system of making kelp, Mr. Stanford said, were the loss of iodine and potash by volatilization at the high temperature developed in burning the weeds, and the reduction of the alkaline sulphates to sulphides. The loss of iodine, he stated, was equal to the present yield. Other evils are the unavoidable mixture of clay and stones with the kelp, which sometimes amount to as much as 50 per cent., and the entire neglect of the winter weeds, which being mostly deep-sea weeds, are the richest in iodine. Mr. Stanford's method is to collect and dry the weeds, press them into cakes, and carbonize them at a low red heat in iron retorts, collecting the products of distillation. The charcoal left in the retort is then lixiviated like ordinary kelp, the solution so obtained being quite colourless, the mother liquor, after the separation of the sulphate and chlorides, containing no sulphur compounds. The products of distillation consist of ammonia, acetone, and wood spirit (which the Author could not separate), a light volatile oil, paraffine oil, a colouring matter, gas and tar. The charcoal after lixiviation may be used as fuel to heat the retorts; and, finally, the ash left after the combustion of the charcoal contains from 15 to 25 per cent. of phosphates of lime and magnesia, and is therefore a valuable manure. Mr. Stanford concluded an able and interesting paper by stating that he had patented his process in England and France, and expressing a reasonable hope that the present wasteful method of obtaining but a part of the valuable materials from sea weeds would soon be abandoned.

In reply to Mr. Hanbury, Mr. Stanford said there was no reason why the weeds thrown up on the South Coast of England should not be used. In the Isle of Wight and parts of the coast exposed to the Atlantic the deep sea weeds were thrown up, which he had shown to be the most valuable.

The next Paper was "*On Vesicating Collodions*," by Mr. Tichborne. After remarking on the disadvantages of the *Emplastrum cantharidis* and the vesicants in general use, the Author said that he believed that *Collodium vesicans* was not more generally employed because there was no authorized formula for its composition. Ordinary contractile collodion, he said, however, was worse than useless as an excipient for cantharidin. It forms an impermeable tissue which protects the skin from the action of the vesicant. Examined by the microscope, such a tissue is seen to be of a homogeneous structure, with ridges and some partially disorganised fibres of cotton. The addition of glacial acetic acid to collodion destroys the contractility, the ridges and filaments are not seen, and the tissue formed is porous, dries slowly, and is a fit vehicle for a vesicant. Glacial acetic acid is also a solvent for cantharidin. Mr. Tichborne recommends the following formula for a vesicating collodion: Take of cantharides 6 ozs., ether 12 ozs., glacial acetic acid 2 ozs., gun cotton ½ oz., methylated spirit 7 ozs. Exhaust the cantharides with the ether and acetic acid in a percolation apparatus, and displace the ether retained with the methylated spirit. Afterwards add sufficient ether to make fifteen ounces and dissolve the gun cotton. The *Mylabris*, which is a better vesicant, may be used instead of the cantharides, if desired. When used, paint the part over with a good thickness of the collodion, passing the brush over and over again until about half a drachm is applied to the square inch. Then cover the part with oil silk or gutta-percha two or three inches larger than the surface to be blistered; this hastens the action of the vesicant. In about ten or fifteen minutes the collodion may be wiped off with ether, and the blister rises directly. Strangury, the Author said, never followed the use of cantharides in this way. A cheaper vesicating collodion may be made with volatile oil of mustard—one drachm of the oil being used with seven drachms of collodion and thirty drops of acetic acid. This preparation, however, is more painful and not so certain as the former, and is rather a rubefacient than a vesicant.

Mr. Deane said it had been a mistaken notion for years that dilute acetic acid would dissolve cantharidin, and, therefore, an acid had been used for the acetum cantharidis which was only two-thirds the strength that ought to have been used. Beaufoy's acid, one to eleven, he had found would dissolve cantharidin.

Mr. Attfield pointed out that bromacetic and chloracetic acids were two powerful vesicants which might be found useful.

Professor Bentley said that he had found out accidentally, and by personal experience, that a solution of podophyllin in spirit rapidly blistered. He spilt a little

of such a solution over his wrist, and in ten minutes found a large blister. He thought this solution might come into use, if no difficulty should be found in healing the blister.

(To be continued.)

THE CAVENDISH SOCIETY.

The Master of the Mint, *President, in the Chair.*

The Fifteenth Anniversary Meeting of this Society was held at the rooms of the Chemical Society, Burlington House, on Saturday, March 1.

The Secretary read the following Report of the Council:—

"The proceedings of the Cavendish Society being at present limited to the completion of the translation of 'Gmelin's Chemistry,' there are but few subjects to which the Council have to refer in this Report. They regret the delay that has occurred in bringing out the volume for 1861, a delay which they had not anticipated, and which has arisen from causes over which they have had no control. The new German edition of Gmelin has for several years been produced in parts, which have issued from the press with a degree of regularity that seemed to justify the conclusion that the English Editor might produce a volume of the translation of the usual size every year, and founded upon this conclusion, the fifteenth volume of the work was promised, and is due for the year 1861. A portion of the matter for this volume, all in fact that remained of the last published German part, has for many months been prepared for the English edition; but as this was insufficient to complete the volume, it was necessary to wait for another German part, which, although expected six months ago, has but recently arrived. This will furnish the matter required to make up the volume, and no time will be lost in supplying it to the members who have subscribed for 1861.

"In a note appended to the part of the German work just received, a promise is given that the entire work shall be completed in the course of the present year. Should this promise be fulfilled, the sixteenth volume, which will be issued for 1862, and the index to the entire work constituting a volume which will immediately follow, may be expected in the course of 1863. Considerable progress has already been made in the preparation of the index, which will greatly enhance the value of the work for the purpose of reference.

"The Council are happy to be able to announce the republication of the first volume of the Handbook, which was undertaken by Mr. Harrison without involving the Society in any responsibility. In the reprinting of this volume the matter has been carefully read and corrected by the Editor. A few complete sets of the work may now be obtained from the agent, and members and others who have sets with the first volume deficient may remedy the defect.

"The financial position of the Society will, the Council trust, be considered satisfactory. There are no outstanding debts or liabilities, while the balance at the Bank is sufficient to meet the cost of the completion of the volume due for last year. There is also a considerable stock in hand of some of the Society's works. The Council congratulate the members upon this position of the affairs of the Society, and they hope the steady and liberal support hitherto given to it will not be withdrawn until the objects for which it was founded shall be realised, one of the most important of which was the production of an English edition of 'Gmelin's Chemistry.'"

The financial statement appended to the Report, showed that the balance in hand at the previous anniversary meeting was 283*l.* 4s. 1d., and the amount received during the year was 498*l.* 11s. 4d, while the expenditure during the same time had been 406*l.* 6s. 5d., leaving a balance in hand of 375*l.* 15s.

The Report was received with satisfaction by the meeting, the usual resolutions were unanimously passed, and the new Council and Auditors elected.

CORRESPONDENCE.

Crystallised Platinum.

To the Editor of the Chemical News.

Sir,—Observing in a recent Number of your Journal a notice from Dr. Phipson of crystallised platinum obtained by a lengthened exposure to the action of warm nitro-hydrochloric acid, and having some time since observed a similar surface action produced in a few hours by boiling scrap platinum in the same acid, I have looked for a piece of it, which I enclose, and place it and this memorandum at your disposal. I am, &c.

Alfred Noble.

Chemical Notices from Foreign Sources.

MINERAL CHEMISTRY.

Method of Obtaining the Whole of the Sulphur from Metallic Sulphides.—Simple roasting we know separates only a part of the sulphur from sulphides; but Brunfaut suggests (*Chem. Centralblatt*, No. 2, 1862) a plan by which the whole may be obtained. He mixes the sulphide with 10 per cent. of coke or small coal, and during the latter part of the roasting passes first, for an hour and a-half, a current of superheated steam, and then, for the same time, a current of heated air, through the oven. The gases and vapours are passed into condensing chambers where at first the sulphur deposits. Afterwards when the steam or air is passing the sulphuretted hydrogen and sulphurous acid formed are collected in separate chambers, and after passing through a layer of coke to stop any solid particles are led separately into a gas holder, having a partition and dipping into water. Here they are submitted to pressure, and then they react on each other, and the sulphur is separated:—$2HS + SO_2 = 2HO + 3S$. The other gases present—nitrogen, carbonic acid, and carbonic oxide—do not interfere with this reaction. With good contrivances, and working two or three ovens at a time, we dare say this might be a valuable process.

MISCELLANEOUS.

Royal Institution.—Tuesday, March 25, at Three o'clock, J. Marshall, Esq., "On the Physiology of the Senses." Thursday, March 27, at Three o'clock, Professor Tyndall, "On Heat." Friday, March 28, at Eight o'clock, Admiral Fitzroy, F.R.S., "An Explanation of the Meteorological Electric Telegraphy and its Basis, now under trial at the Board of Trade." Saturday, March 29, at Three o'clock, Professor H. E. Roscoe, "On Spectrum Analysis."

ANSWERS TO CORRESPONDENTS.

Chrome Iron Ore.—C. L. Oudesluys.—Will our correspondent favour us with the method which he employs in analysing this mineral? The discrepancies between the American and English analyses make us anxious to see the process followed. The specimens of asbestos paper have arrived safely. We will report on them shortly.

Dissolving India Rubber. — A Subscriber. — Either chloroform, bisulphide of carbon, or benzol may be used for this purpose.

Carbolic Acid.—H. Bell.—We are unable to give our correspondent more information than the paper alluded to contains. The address of the manufacturer is there given. See note on p. 19 of this Volume.

E. P.—Apply to Mr. Brooman, Patent Office, Fleet Street, London. The other information we cannot give.

I. G. B.—The process known as Liebig's, which is described in most recent works.

G. Edmonds.—Precipitate the tartaric acid as a lime salt and separate the acid from this with sulphuric acid.

Books Received.—"On the Therapeutic Law of Specific Remedies." By A. De Noé Walker, M.R.C.S.E. London: G. Clayton, 315, Regent Street. "Report on the Past, Present, and Future of the Royal Institution." By the Honorary Secretary. London: W. Clowes and Sons.

THE CHEMICAL NEWS.

VOL. V. No. 121.—March 29, 1862.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On Soil-Analysis, by Professor S. W. JOHNSON, *of Yale College.*

(*Continued from page* 158.)

YEARS ago, following the teachers of agricultural chemistry in this country and England, we believed that soil-analyses were adapted to be of exceeding use to farmers. Having practised analytical chemistry sufficiently to undertake the work, we proceeded, when on a vacation visit, to collect some farm soils for the purpose of applying our skill and knowledge. On putting down the spade and post-augur into the drift overlying the lowest Silurian of Northern New York, we were at once struck with the difficulty of procuring an average specimen. The soil, for a depth varying from two to six inches, was quite fine, but below that depth largely mixed with gravel. On comparing different samples taken from a small area, it was plain that the soil was not a fit subject for analysis. The relative quantities of organic matters as indicated by the colour of the surface of small stones,—some quartz and granitic, others slate and limestone of several geological members,—were astonishingly variable. Here we found the soil sandy, there it was clay. To take a sample from one place was to do obvious injustice to the sixty-acre field. To take it from a dozen places would not render the selection of a fair sample any more certain. Then as to depth —was it proper to go down six inches, one foot, or how far? Had the field been a bed of iron ore, assays of a dozen samples taken from different parts would have indicated very satisfactorily the general value of the deposit, would have served as data for buying and selling the property, because the worth of an unworked bed of such ore depends less upon its contents of iron than upon external circumstances which affect the extracting of the metal. Had the field been covered with rich dressed copper ore to the depth of six inches, it would have been necessary to divide it up into small parcels of a few tons, average these carefully, and as carefully assay each one. No one would risk purchasing a hundred thousand tons of copper ore on the analysis of one or of a dozen samples, because it is impracticable to intermix or average such a mass of material as that a dozen samples shall accurately represent it.

We hold it, therefore, as the first objection to soil-analyses that to procure a specimen which accurately and certainly represent a field or district, is practically impossible in the majority of cases, and, if possible, requires a series of analyses to prove the fact.

This argument applies with the greater force when we consider how small a proportion of the ingredients of a soil are of any immediate use in feeding crops. The really active nutrient matters of a soil are not reckoned by per cents. or by tenths of per cents., but by the minutest fractions.

A heavy crop of thirty-seven bushels of wheat, grain and straw included, removes from an acre of land but 300 lbs. total of mineral matters. According to Dr. Peter's weighings on some of the Kentucky soils, we may assume that, taken to the depth of a foot, an acre of soil weighs 3,000,000 lbs. All that is removed by the heaviest wheat crop then in one year is but $\frac{1}{30000}$, or 0·0033 + per cent.

It follows that the annual removal of the heaviest crop of wheat from a soil for 100 years diminishes its mineral matters by less than 0·4 per cent. If then, in the selection of a sample, the average composition is departed from to the amount of 4 parts in 1000, the analysis may misrepresent the soil, by the value of 3700 bushels of wheat per acre, or by what represents, so far as mineral ingredients can, the fertility of a century.

What freaks and accidents is not the soil-analyst the sport of? A bird, squirrel, or dog relieving Nature at the spot where he collects his sample, innocently magnifies the phosphoric acid or alkalies of the surrounding thousand acres a hundred fold. The soil gathered towards the end of a long rain, whereby its soluble matters are carried deep into the subsoil, is declared poor, by analysis, whereas, if taken after a fortnight of drought, it might appear extraordinarily fertile. Boussingault found in his rich garden soil in June, during wet weather, 0·00034 per cent. of nitric acid. In the following September, after a period of dryness, it contained 0·0093 per cent. or twenty-seven times as much as in June. This ingredient is indeed more liable to fluctuation in amount than any other, both because it is formed in the soil, and because it is not subject to the absorbent action which the soil exercises over most other of its soluble constituents; but the same variation occurs among the other ingredients according to the direction of the capillary movement of the soil-water, though in less degree.

Independently, however, of all considerations and calculations like the above, we have proof—evidence, at least, that supports these considerations, and has never been publicly refuted—that it is practically impossible to obtain average specimens of the soil. I refer to investigations made as long ago as 1846—9 under the direction of the Prussian "Landes Oekonomie Collegium," and reported by the distinguished Magnus. The account of these experiments is given in detail in Erdmann's *Journal für Praktische Chemie*, vol. xlviii. pp. 447 *et seq.*

The "Landes Oekonomie Collegium" at that time carried on systematic experiments in agriculture at fourteen distinct stations scattered through the Prussian domain. The trials which we now speak of, were made for the ostensible purpose of studying the exhaustion of the soil by cropping. The plan was to analyse the fourteen soils, the history of which for years previous was accurately known, then crop them with rape until "exhausted," then compare together the original com-

position of the soils with their composition after exhaustion, taking into account as well, the composition of the crops removed. The research began with collecting and analysing the soils. In order to meet as far as possible the difficulties of securing average specimens, equal portions of the soil of each field were taken with the spade at ten or twelve different points, and thoroughly intermixed; each sample was then passed through a sieve, the holes of which were two square lines in area, so as to remove all coarser stones, then again well worked over to complete the mixture. Of each sample three separate portions were analysed, in most cases by different operators. The analyses were made by, or under the guidance of the ablest chemists of Germany. They were made according to a prescribed scheme, and that there should be no reason to slight the work, the work was paid for. It is true that analytical chemistry was not so advanced in 1846 as now. It is true that the methods then practised for estimating phosphoric acid and some other substances were not as perfect as they now are; but for the most part the analyses then made are as accurate as any that could be executed to-day. It cannot be supposed for a moment that analysts like Rammelsberg, Bödecker, Genth, Debus, Knop, Heintz, Krocker, Marchand, Weidenbusch, Sonnenschein, Varrentrap, Weber, &c., &c., would by fault of method, or by carelessness, return anything but results that were accurate, as far as it was possible to make them such. We cannot suppose that their determinations of lime, oxide of iron, potash, and sulphuric acid, substances estimated then by the same methods that are now employed, would vary if they were supplied with homogeneous material to operate upon. But let us look at some of their figures. We tabulate a number of them taken at random:—

Soil of		Lime.	Potash.	Sulphuric acid.	Phosphoric acid.
Eldena,	*a.*	0·39	0·93	0·08	0·06
	b.	0·75	2·06	——	0·17
	c.	0·25	0·12	0·02	0·40
Beesdau,	*a.*	0·802	3·825	——	0·042
	b.	0·039	0·490	——	0·046
	c.	0·715	0·792	0·004	0·007
Neuensund,	*a.*	1·692	3·531	0·050	0·051
	b.	0·614	1·289	0·038	0·010
	c.	0·728	1·243	0·241	0·121
Turwe,	*a.*	2·312	1·112	0·040	0·057
	b.	2·67	1·14	0·03	0·20
	c.	3·391	0·201	0·022	0·014
Frankenfelde	*a.*	0·420	1·155	0·016	0·004
	b.	1·081	——	——	0·418
	c.	0·461	1·456	0·015	0·071

If we run over these figures and those of the entire series of analyses, we find that different determinations disagree to such an extent as to make it the sheerest folly to base any calculation of the value of the soil upon analysis. Some of the analyses agree sufficiently to show that accordant results are possible if uniform material be taken; but the grand result of the investigation is that the difficulties of getting a uniform material are exceedingly great. Again, we must remember that in the case before us, the examinations of each soil were made upon portions of one carefully mixed sample. What would have been the result had each chemist received a sample collected separately from all the others, and from different parts of the field!

Dr. Peter mentions these analyses of the Landes Collegium, and quotes a few of the results on page 187 of the Third "Kentucky Report." He believes, however, that these discordant results do not invalidate soil analyses when made, as they may be made, with "means and appliances now at the service of the analytical chemist," and thinks "this statement, however hazardous it may seem, will be found to be sustained" in his "Report."

In the "Report" before us, however, we do not find anything to sustain Dr. Peter's view. He gives, so far as we have discovered, no duplicate analyses, to show what accuracy his methods admit of on the same sample, much less does he prove by analyses of specimens separately gathered from the same field, that it is easy to procure an average material for analysis. Until this proof is produced, the evidence is in favour of our view.

(To be continued.)

On the Analysis of Cast Iron, by M. W. Weil.*

Certain difficulties always attend the estimation of the carbon in cast iron. Part of the carbon is precipitated during the solutions of the cast iron, while another part is disengaged in the state of carburetted hydrogen. As it is difficult to pulverize cast iron, it is generally filed, which, however, introduces extraneous particles of iron. By observing the following precautions, cast iron may easily be analysed:—Take the cast iron as an electrode of the positive pole in a Bunsen battery, and place it in an acid liquid; the other electrode is platinum. Hydrochloric acid acts as a solvent better than sulphuric acid, which becomes passive with the iron. When the current is too strong, ferric chloride is formed; carbon is deposited at the negative pole, and carbonized hydrogen is disengaged. The current must then be moderated by immersing the platinum electrode more or less deeply. In a few hours five or six grammes of cast iron may be dissolved in this way, the carbon remaining and preserving the original form of the cast iron.—*Repertoire de Chimie*, iv. 63.

TECHNICAL CHEMISTRY.

Process for Testing the Purity of Beeswax, by M. Robinaud.†

This process is founded on the property possessed by pure beeswax, of leaving half its weight as residue when treated by fifty times its weight of rectified ether.

The substances with which beeswax is adulterated,—for example, suet, stearine, vegetable wax, &c.—dissolve almost completely in the above proportion of ether. (Pure vegetable wax leaves only five per cent. of residue insoluble in ether.)

The process is conducted in the following manner:—To discover the pulverulent, mineral and vegetable impurities sometimes fraudulently added to wax, such as fecula, talc, &c., treat several grammes of wax with rectified spirit of turpentine with heat; the whole should dissolve; if there is a residue, throw it on a filter, wash with the spirit, and determine the weight and nature of the adulteration. Then treat 1 gramme of wax with 50 grammes of ether; filter, wash the residue with cold ether, then dry in the air and weigh. Subtract the weight of the residue obtained by treating with spirit of turpentine, and by doubling the remainder we have the per-centage of the beeswax. Suppose, for example, a wax which would leave 5 per cent. of residue insoluble in spirit of turpentine, and of which

* *Zeitschrift des vereins Deutscher Ingenieurs*, 1861, p. 224.

† *Bulletin de la Société de Pharmacie de Bordeaux.*

1 gramme would give 45 centigrammes of residue insoluble in ether. Subtracting the 5 of impurities, we obtain 40 of residue attributable to the beeswax, which shows that the wax examined contained 80 per cent. of it, and, consequently, 15 per cent. of vegetable wax, stearine, or suet, and 5 per cent. of other vegetable or mineral matters.

In a mixture of wax and paraffin, the presence of the paraffin can be shown, according to M. Landolt (Dingler, *Polyt. Journal*, vol. lx. p. 224), by treating the whole with fuming and hot sulphuric acid, which chars the wax, and does not attack the paraffin, which floats, and when cold becomes a white translucent layer.—*Repertoire de Chimie*, iv. 62.

On the Employment of Glycerine to Prevent the Efflorescence of Salts on Dried Carmine of Indigo, by M. POHL.*

CARMINE of indigo (sulphindigotate of soda) is generally found in commerce in a state of a semi-liquid paste, because in the dry condition it becomes after a time covered with an extremely abundant white efflorescence. This efflorescence is due to the sulphate of soda with which the paste is necessarily impregnated, since the sulphindigotate of soda is insoluble only in a concentrated solution of alkaline salts, and dissolves, on the contrary, very readily in pure water. The carmine cannot be freed from the alkaline sulphate by washing.

Its pasty condition, more or less watery, presents, however, some serious inconveniences, both as respects it portability, and because this state facilitates the admixture of products of inferior colouring power.

M. Pohl finds that if from three to four per cent. of glycerine are added to the carmine of indigo paste, it can be dried and preserved for an indefinite time without becoming covered with saline efflorescence, the glycerine not in the least injuring the brilliancy or purity of the colour.

PHARMACY, TOXICOLOGY, &c.

Research on the Separation, Composition, and Physiological Source of Normal Urinary Pigment, by CHARLES R. C. TICHBORNE.

IN conducting a series of experiments upon urine, my attention has been attracted to its colouring matter, which, although frequently the subject of investigation, has not hitherto given the return which the importance of all such research merits; for although chemico-physiological inductions are generally very speculative, such inductions when drawn from experiments which, although however small, are carefully performed, must add to the aggregate of this branch of science, so vast in its import and yet so difficult in its development. Vital phenomena have been, and must still be, explained in a great measure by theoretical inferences drawn from physical and chemical analyses; for it is seldom that we can bring synthesis to bear in these investigations. Such inferences we are compelled to view as provisional truths, until, in the course of further examinations, fresh explanations become more probable from newly-observed results.

I instituted experiments, original and otherwise, for isolating the subject of the present paper, preparatory to investigations hereafter to be detailed. I thus arrived at the conclusion that the subjoined *modus operandi* was the best calculated to separate it free from the other extractive matters and in its primitive condition. This separation would at the first glance appear comparatively easy, but in its practical carrying out it presents innumerable difficulties. Its amorphous character and perfect neutrality as regards forming definite compounds with other substances almost entirely preclude the possibility of defining whether it is an individual compound or a conglomeration of products which we have isolated. Indeed, I am inclined to think that this insensibility to determinate form, and, therefore, difficulty attendant upon its isolation, will account for the great discrepancies manifested by previous experimenters,—*i.e.*, I consider that there may perhaps be several colorative substances in normal urine, such as Dr. Marcet's acid,* supposed to be identical with the colouring matter isolated by Dr. Harley.† Such ingredients, although occurring in proportional small quantities, would be sufficient to greatly alter the ultimate analysis, accordingly as they are extracted or not; but that the colour of healthy urine is mainly due to some effete product as constant in composition as urea or any other excretory substance, cannot be doubted.

The kidneys, being the great emunctories of excessive water, the diversity of shade exhibited by individual samples of urine is easily accounted for by dilution; but the absolute amount of colouring matter voided in a given space of time is on an average most constant. This is easily illustrated by the following simple experiment:—On taking a glass tube of some considerable length, and closing one end with a white disc, upon which is a black figure, we get an instrument by which we can measure the relative intensity of shade presented by different liquids. The side of the tube being graduated into equal degrees, we pour into it a filtered sample of the mixed urines passed in twenty-four hours, until, on looking perpendicularly into the tube, and so through the urine, we lose sight of the figure at the bottom. We then read off the number of divisions that the urine reaches to, for future comparison. On repeating this experiment several times, I found that the results from one person coincided in a most remarkable manner.

Before proceeding to describe my method of separating the pigment, I may remark, that in all the following experiments a mixed urine was used, collected from sundry persons, and at all hours, each instalment being examined previous to its use and found to present the ordinary appearance of health,—*i. e.*, it contained urina chyli and urina sanguinis, and might be considered an average specimen of normal human urine. Several gallons of this mixture, having a specific gravity of 1·019, were then evaporated at a temperature not exceeding 82° C., until the liquid had a density, when filtered from the deposited salts, of 1·163. A solution of ammonia-sulphate of copper was then added in excess, which is perceived by the change of colour, the addition of the ammoniacal salt at first producing a green solution, which is rapidly converted into a bluish one when it contains an appreciable excess. The whole was now found to be decidedly alkaline. It was divided into two equal bulks, one of which was as nearly neutralised as possible with diluted sulphuric acid, and again mixed with the remaining half. A basic precipitate was thus formed of the cuperous salt, and containing almost the whole of the colouring matter, together with some of the

* Dingler, *Polytechnisches Journal*, vol. clx. p. 392.

* *British and Foreign Medico-Chirurgical Review.*

† Dr. Harley's "Researches on the Colouring Principle of Urine." Paris. 1852.

protein compounds and a little urea.‡ After standing some time, it was thrown upon a paper filter and washed with distilled water. The moist precipitate was then decomposed as nearly as possible with diluted sulphuric acid, which eliminates the pigment. Strong spirits of wine was then added to the brownish syrupy mass, and the whole then warmed nearly to the boiling point. On cooling, the supernatant fluid was filtered from the deposited sulphates and evaporated to dryness. The residual matter, when now treated with ether, containing about one-tenth of alcohol, yielded the following impurities,—viz., the remainder of the urea, a greenish fatty body, and a red colouring matter, soluble in ether,§ but I did not get a sufficient quantity for analysis. Although the pigment does not dissolve to any sensible extent in the above mixture of ether and alcohol, it becomes soft and semi-fluid, and is best worked by stirring it about at the bottom of a capsule with a spatula. By the repetition of this operation three or four times, the foreign bodies are disposed of; and on being re-dissolved in water and again treated with ammonia, sulphate of copper, &c., as in the first part of the process, a spirituous solution of the pigment was got, which yielded it on evaporation as a brown amorphous mass. This, on moistening, evolves a smell of stale urine. It is perfectly insoluble in ether, nearly so in alcohol, but more soluble in rectified spirits, and is dissolved almost to any extent in water. It is very hygroscopic, therefore requires great care in desiccating for analysis. It was carefully tested for urea, creatin, uric and hippuric acids, but gave no indications of the presence of these substances. Treated with a mixture of ether and alcohol, it gave but a trace of extractive, which presented all the characteristics of the original pigmentous matter. It was almost entirely precipitated from its solution by basic acetate of lead,—one part dissolved in 2000 parts of water, gave a solution having the same intensity of tint presented by the sample of mixed urines as originally used, and by increasing the proportion of the colouring matter, or by dilution, every shade of ordinary normal urine could be imitated, so that we may presume that this substance is the chief chromatic material, and that the difference in colour presented by urina chyli, urina sanguinis, or urina potus, and not in an abnormal state, is due to the amount of pigment present. That other colorative substances invariably do exist, the author has no doubt, but their existence is proportionally too small to much affect the physical appearance, and, therefore, will not be taken into consideration in the further discussion of this subject. On incineration, the pigment tumefies and evolves a smell indicative of the presence of nitrogen. In the first experiment it yielded ·4 per cent. of ash, which large amount was probably impurities from the employment of too weak a spirit. This ash gave on analysis a trace of iron; but as this matter was more fully investigated, we will leave it for the present, and detail the experiments *in extenso* in their proper place.

The carbon and hydrogen were determined by combustion with oxide of copper, the evolved gases being passed over an ignited coil of copper at the anterior end of the tube. The combustion was conducted very slowly, until it had proceeded over the whole length of the oxide of copper, when the heat was raised and a gentle stream of oxygen passed through the tube. The mean of three experiments gave the per-centage as 67·80 for carbon, and 4·23 for hydrogen. The nitrogen determination was arrived at by MM. Will and Varrentrapp's method, and gave likewise as the result of three analyses 8·56 per cent. Therefore the centisimal composition must be represented as follows:—

C . . .	67·80
H . . .	4·23
N . . .	8·56
O . . .	19·41
	100·00

This disagrees with Scherer, and would induce us to form other views than his as regards its source. He gives the subjoined as the composition of the pigment extracted by basic acetate of lead:—

C . . .	58·43
H . . .	5·16
N . . .	8·83
O . . .	27·58
	100·00

He views this substance as the result of the destructive assimilation of old blood corpuscles; and the circumstance that there has been iron found in it would seem at first to corroborate this supposition, but, as will be presently shown, the presence of that element is purely accidental. Instead of placing it below hippuric acid as regards its connection with the elimination of carbon (for it is so placed by Dr. Bird), it is a more highly carbonised product.‖

	Bile, Dr. Kemp.	Urinary Pigment, Tichborne.	Hippuric Acid, Theory.¶	Urinary Pigment, Scherer.
C .	68·40	67·80	63·53	58·43
H .	10·13	4·23	4·70	5·16
N .	3·44	8·56	8·24	8·83
O .	18·03	19·41	23·53	27·58
	100·00	100·00	100·00	100·00

Hence, then, we are led to infer that the pigment is a further product of the metamorphosis of hippuric acid; indeed, this latter compound, minus the elements of water, gives a centesimal result, which is most extraordinarily near to the urinary pigment analysis:—

	Hippuric Acid, HO.	Colouring Matter of Urine.
C . . .	67·08	67·80
H . . .	4·35	4·23
N . . .	8·70	8·56
O . . .	19·87	19·41
	100·00	100·00

And, therefore, the formula should be represented by $C_{18}H_7NO_4$.

The hypothesis of its source being hippuric acid is confirmed in a great measure by recent physiological observations. It is generally allowed that we view that acid as a derivative of some of the non-nitrogenous compounds, and is, according to Liebig, always present in a normal human urine, but in such small quantities as to escape observation; whilst in cases of a vegetable diet the amount is greatly increased, and in gramivorous

‡ Ammoniacal nitrate of copper has been used by M. de Luna for decolorising solutions of urea.

§ Probably urrosacine, the substance previously mentioned.

‖ Viewing this pigmentous matter as a colloidal substance, it is my intention, at some future period, to submit it to some experiments with the dialyser; but I have not as yet put them into operation, as the idea was only entertained some few days ago. By this means Professor Graham succeeded in getting a caramel from sugar even more highly carbonised than that obtained by Gélis. Therefore, I look forward with some anxiety to see the results which the colouring matter will give when submitted to the action of a septum.

¶ Anhydrous hippuric acid.

animals is met with in large quantities; or, in other words, when the diet is altered and an increase made in the farinaceous food, a like increase is perceptible in the elimination of hippuric acid. When we take into consideration the large proportion of non-nitrogenous pabulum consumed in the ordinary human regimen, and also viewing the above acid as produced by the starchy products assimilating certain of the protein substances, we are justified in looking for the excretion of a considerable amount of hippuric acid, which, if not present, has probably undergone some further metamorphosis, and has in all probability been principally eliminated as the colorative matter. Purpurine, the name given by Dr. Bird to the pink colouring matter produced by the action of hydrochloric acid upon the ordinary pigment, and also a frequent product of disease, is most likely also a substance which, in the latter case, owes its origin directly to hippuric acid. The close approximation existing between that acid and purpurine has been noticed by Dr. Bird.**

As before mentioned, iron was detected in the first experiment, and as that element has been pronounced to be an invariable constituent of the colouring matter, its presence becomes an argument in favour of its destructive assimilation from old blood corpuscles. Now, blood corpuscles often present themselves in apparently healthy urine, and are, no doubt, generally the cause of iron being present in the pigmentous substance. A series of different urines, when submitted conjointly to microscopical and chemical examinations, gave the following results:—

Experiment 1 presented under the microscope not the slightest trace of nitric corpuscles, nor could any altered form or *débris* of such bodies be found. The pigment, on ignition and application of suitable tests, gave no indication of iron.

Experiment 2.—A sample of urine, containing blood corpuscles more or less irregular in form, the colouring matter gave decided indications of iron.

Experiment 3.—A sample similar to No. 1 in its microscopical appearance, but in which the presence of iron was evident.

Experiments 4 and 5 were both free from corpuscles, and gave no indication of iron.

Or, arranged in juxta-position,—

	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.
Corpuscles in Urine.	Absent.	The *débris* quite palpable.	Absent.	Absent.	Absent.
Iron in Pigment.	Absent.	Present.	Present.	Absent.	Absent.

From which we must infer that the iron is introduced by the corpuscles, and does not stand in any relation to the colouring matter, because in three of these experiments it was absent, and, although present in the other two, the corpuscles being there in one case would naturally account for it, if only on the principle of exosmosis, whilst the disparity exhibited in density between the liquor sanguineous and urine would soon be quite sufficient to produce complete disorganisation, and to destroy all trace of the corpuscles when submitted to microscopical examination. We are, therefore, justified in pronouncing the presence of iron as quite accidental to the pigment.

In conclusion, the deductions capable of being drawn from the foregoing experiments may be epitomised as follows:—

Firstly. That although, from want of geometrical formativeness, it is difficult to substantiate whether the pigment is a definite compound, we are induced to consider it as such, from the circumstance that its properties are marked and decided when treated with the general solvents, it being, at the same time, impossible to isolate any other substance by their means.

Secondly. From experiments detailed we see that the idea of the elimination of the colouring matter from worn-out blood corpuscles (as stated by Scherer and Harvey), and which is not rendered probable from its composition, is in no way carried out by the presence of iron, the appearance of the latter being quite accidental and foreign to the pigment.

One objection to the theory of the metamorphosis of hippuric acid into the colouring matter is, that we should have a substance inorganic and crystalloidal in its character, and, therefore, peculiarly suited for elimination from the system, becoming amorphous before its final expulsion. That this amorphism is necessary for the immediate formation of organism or direct assimilation I have no doubt, and that inorganic or geometrical construction is opposed to vital formativeness; but there is nothing to show why, when out of the pale of secretive operations, the ultimate products should not be highly carbonised amorphous compounds, as such like substances so frequently accompany and are met with in similar artificial processes. As regards the origin of the pigment, although the hypothesis mooted ought to be taken with caution, unless we could follow out all the changes of secretion and excretion connected with it, and also practically demonstrate the existence of each change, its being a decomposition product of hippuric acid must be accepted as a fact unless further data should render a change of explanation more beautiful.

It is by this means that physiology, *per se*, has made such gigantic strides in the last epoch of science. It is the course of science, speculative and practical, running conjointly, that has produced the rapid development of all knowledge, but more particularly of this branch, and which, in so short a space of time, has accumulated the mass of information which only forms as yet an incomplete foundation of an immense pile,—the cosmos or general superstructure.

** "Urinary Deposits," p. 111.

PHYSICAL SCIENCE.

On the Filtration of Air, and the Influence which it Exerts on Fermentation, Putrefaction, and Crystallisation, by M. H. SCHROEDER.

THIS paper is in continuation of some researches published some time ago by the same author. M. Schroeder found that the ebullition of a fermentescible or putrescible liquid, in a flask closed with a plug of cotton, generally checked fermentation. However, milk, yolk of egg, and broth containing meat, gave him different results.

On repeating these experiments, he found that, in order to prevent, by means of a plug of cotton, the putrefaction of the above-named substances, they ought to be previously raised to a temperature higher than that of boiling water. If this precaution were taken, and if the substances were then introduced into a flask, and the liquid boiled during a very short time, the flask being then closed with cotton wool before the temperature lowered, no change would be produced.

The same result is obtained by very considerably prolonging the ebullition.

When the liquid is only boiled for a short time, the

formation of *mycodermes*, which are produced in the open air, is not noticed on its surface. Fermentation, however, takes place, and it may, therefore, be concluded that the ferment capable of inducing putrefaction is contained in the state of germ, in milk, meat-broth, and yolk of egg, and is not destroyed at a temperature of 100° C.

The author has attempted to transport the ferment which provokes putrefaction into different liquids. He performed this operation with very great precaution under a bell-jar filled with coal gas. He found that this particular ferment was not developed in liquids susceptible of nourishing the ordinary vegetable ferments, such as beer-yeast, but that it was propagated, on the other hand, in the presence of albumen, casein, yolk of egg, and urine, and induced the putrefaction of these liquids even when they had been previously heated to temperatures much exceeding 100° C.

After having quoted a certain number of experiments performed on supersaturated saline solutions, M. Schroeder finishes by stating a certain number of conclusions, of which these are the principal:—

There exists a series of phenomena of fermentation and putrefaction which are due to germs existing in the atmosphere; such are the alcoholic and lactic fermentations, mouldiness, and the putrefaction of urine.

Animal or vegetable substances contained in a flask closed by a plug of cotton are protected against all fermentation, when the germs which they contained have been destroyed by ebullition.

All germs carried in the air are destroyed by a short ebullition. However, milk, yolk of egg, and meat contain germs which are only destroyed by prolonged ebullition, or by a temperature higher than 100° C.

The germs contained in these latter substances are susceptible, even after an ebullition which has not been long continued, of inducing putrefaction, by developing and taking the form of *vibrios*, large in size, but sluggish.

This putrefactive ferment is of animal character. It lives at the expense of albuminoid matters, and cannot multiply under conditions which are favourable to the development of vegetable ferments.

The crystallisation of supersaturated solutions is provoked by a kind of induction exerted by the surfaces of solid bodies.

The crystallisation of more soluble hydrated bodies is due to a less energetic induction than that which provokes the deposition of less soluble hydrated bodies.

The strongest induction is exercised by the surfaces of crystals of the same nature as the dissolved salt. The inductive force of the films of air formed on the surfaces of solid bodies is less. These films are destroyed by heat or by prolonged immersion, and only re-form very slowly in filtered air.

The crystallisation of more soluble hydrated bodies is only induced very slowly by a crystal of the same body introduced into the liquid with all the necessary precautions; whilst the presence of a crystal of a less soluble hydrated body induces an immediate crystallisation of the solution.

Supersaturated solutions isolated from the outer air, whilst still warm, by a plug of cotton are preserved a very long time, because the cotton prevents the access of all the solid particles floating in the air. Simple shaking is without action on supersaturated solutions, at least if it does not bring them into contact with certain parts of the flask, which already possess an inducing power.—*Annalen der Chemie und Pharmacie*, cix. 35.

PROCEEDINGS OF SOCIETIES.

SOCIETY OF ARTS.

Wednesday, February 5.

Dr. A. W. MILLER, *F.R.S., Professor of Chemistry at King's College, in the Chair.*

(*Continued from page* 166.)

"*Catechu Black.*—The silks are first passed into a solution of salts of peroxide of iron, then into a hot soap solution containing an excess of soap, from whence they are passed into a slightly acid bath of prussiate of potash. The silks which have thus been dyed Prussian blue, are dipped in a solution of persalt of iron, having a specific gravity of 1·15, the object of which is to give an iron mordaunt to the silk. They are then thoroughly washed and passed into a bath of catechu for organzine at 203°, and for tram at 172°, the silks being worked in this bath until it is cold, so as to saturate thoroughly the iron mordaunt with the colouring principle of catechu, and thus produce a black. They are then wrung on the peg and exposed to the atmosphere for twenty-four hours, after which they are passed into a soap solution at 150°, washed thoroughly, and the organzine is then dipped in a bath of weak acetic acid, and the tram in one of weak hydrochloric acid; finally, the silks are passed through an emulsion of oil, well worked on the peg and allowed to dry. These last operations are intended to remove, by means of the fatty matters, the harshness which the silk would otherwise possess.

"The following is a process for preparing dyed silks, so that when woven into fabrics these will be fit for taking the moiré antique:—Two parts of pure olive oil are mixed with one of concentrated sulphuric acid, and agitated until sulphurous acid begins to be liberated. It is then well mixed with fifteen parts of lukewarm water, and the whole further diluted with boiling water. The silks are then passed into this bath and then into a second similar bath, to which has been added a little free vitriol. After this, they are successively dipped in a hot bath containing a little citric acid, then into the previous No. 2, to which is added a little sulphate of alumina and a little black dye to restore to the silk any colour it may have lost during the former operations. The silks, after having been dried in the air, are ready for weaving. The object of these processes is to introduce into the silk fatty acids, the property of which is to communicate to the silk a great degree of softness, and adapt it to receive, by intense pressure, the intended moiré.

"I will now describe a process for dyeing silks white: The silks, after having been boiled, are first passed into a slightly ammoniacal bath, and from thence into another of water, in which has been dissolved a little French purple, and lastly, into another bath containing lukewarm water, to which is added, in successive portions, some carmine of indigo, and the silks are then dried. Many of you will doubtless remember that in my papers read here in 1851, I explained that, when the three primitive colours of the spectrum are mixed in due proportions, they produce white if reflected, and black if absorbed. The French purple gives the red; the carmine of indigo, blue; and the silk itself the required yellow.

"Several improvements have also taken place in the production of maroons, greens and Prussian blues, but time will not allow of my laying the details before you.

"*Colours derived from Coal Tar.*—These colours are as interesting for their beauty and brilliancy as for the source from whence they are derived, and present a remarkable instance of the valuable services which abstract science so frequently renders to the material interests of society. How little did even chemists dream that a substance first perceived by Unverdorben in the year 1826, then named

aniline by Fritsche, and discovered in coal tar about 1841, by Dr. W. A. Hofmann, would lead to the production of such magnificent colours as aniline purple, magenta, &c. There can be no doubt that it is to the interesting and learned researches of Dr. Hofmann, on aniline, that we owe the possession of these splendid colours, and further, it was one of his pupils, Mr. W. A. Perkin, who produced for the first time, on a commercial scale, aniline, and then the splendid purple colour which it is susceptible of yielding. Before describing to you the process patented by Mr. Perkin in 1856 to produce his purple, allow me to lay before you an outline of the present plan followed for obtaining aniline. A carburetted hydrogen, which I mentioned to you in one of my previous papers, called 'Benzine' (C_{12} H_6), and obtained by the careful distillation of purified coal naphtha at a temperature of about 186°, is treated with strong nitric acid, when a violent action ensues, which gives rise to nitro-benzine or C_{12} H_5 NO_4. To convert this compound into aniline, one hundred parts of nitro-benzine are mixed with an equal quantity of acetic acid, and 200 parts iron filings, heat is produced, and the following chemical action ensues :—

$$\underset{\text{nitro-benzine.}}{C_{12}H_5NO_4} + 2\,\underset{\text{water.}}{HO} + 4\,\underset{\text{iron.}}{Fe} = 2\,\underset{\text{oxide of iron.}}{(Fe_2O_3)} + \underset{\text{aniline.}}{C_{12}H_7N}$$

"The whole is then introduced into a retort, and the raw product which passes from it is mixed with a little alkali or lime, and again distilled, when aniline is obtained. This important substance is, as you perceive, a colourless fluid, which boils at 359°, has a decided alkaline reaction, and a sp. gr. of 1·028. The following is the process described by Mr. Perkin for preparing his purple. Solutions containing equivalent proportions of sulphate of aniline, and bichromate of potash are mixed and allowed to stand till the reaction is complete. The resulting black precipitate is then thrown on to a filter and washed with water until free from sulphate of potash; it is then dried. This dried product is afterwards digested several times with coal tar naphtha until all resinous matter is separated, and the naphtha is no longer coloured brown. After this it is repeatedly boiled with alcohol to extract the colouring matter. This alcoholic solution when distilled, leaves the colouring matter at the bottom of the retort as a beautiful bronze-coloured substance, which may be considered as Mr. Perkin's commercial aniline purple. This colour, which can also be produced by oxidising aniline by other metallic salts, is slightly soluble in water, freely soluble in alcohol, and presents the remarkable property of not being affected by light, alkalies, or acids. To dye wool or silk with it, it it simply necessary to add a hot water bath, slightly acidulated with tartaric acid, some of the alcoholic solution of aniline purple, and to work the silk in the said bath, wringing and washing it; the purple shade thus produced can be modified with roseine, Prussian blue, or sulphate of indigo. To dye cotton so as to resist the action of soap and light, the process is modified so as to form on the cotton fibre an insoluble compound of colouring matter with tannin and a metallic base. To effect this the cotton is passed for an hour or two into a bath containing a tanning substance, and then into a weak solution of stannate of soda, wrung out, passed into an acid liquor, rinsed in water, and then, like silk, dipped into an acidulated bath of purple aniline. Also cotton prepared with a basic salt of lead, or as for Turkey red, will take up aniline purple. On the 12th January, 1861, another interesting process to obtain aniline purple was patented by Mr. Adam Girard. Pure red aniline (known in this country as Magenta), is mixed with an equal weight of aniline, and the mixture heated for several hours to 329°, when the mass is changed to a fine purple colour, requiring only to be mixed with water and hydrochloric acid, to remove any aniline or red dye in excess, leaving the purple insoluble, but on being well washed with water, this becomes soluble in alcohol, acetic acid, wood naphtha, and boiling water slightly acidulated with acetic acid.

"Whilst on this mode of producing purple aniline, I may say that blue dye may also be obtained with the above insoluble purple residue by boiling it several times with hydrochloric acid diluted, say ten parts of commercial acid to 100 parts of water, when the purple is converted into a blue dye.

(To be continued.)

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, March 4, 1862.

Dr. J. P. Joule, *F.R.S., President, in the Chair.*

Mr. Robert Rawson, Honorary Member, read the First Part of "*A Memoir of the late Professor Eaton Hodgkinson, F.R.S.*" Thanks having been given to Mr. Rawson, on the motion of Mr. Binney, seconded by Dr. Smith,

The Rev. Mr. Kirkman said, "It is no ordinary renown that the late Professor Hodgkinson secured to himself, and for ever will reflect on Manchester. It is a great thing to outstrip one's cotemporaries in commercial competition, and to accumulate fairly-gathered wealth; it is a great thing to win University distinctions, and to grow old in the plenitude of College dignities; it is a great thing to wear civic honour, and to enjoy, for any good reason, a social celebrity; but Hodgkinson aspired to more. He thirsted for enduring and world-wide fame, and he has won it. How few are there of the young men of this ardent and crowded Lancashire, even of those who may achieve what are esteemed the greatest successes of life, who will find, when their hair is grey, that their names are printed year by year as seals of science in the industrial manuals of all civilized nations! Hodgkinson diminished instead of increasing his little patrimony, in the search of truth equally abstruse and beneficent; but this proud memory he has left behind him—that, go where you will in any clime of earth, if you meet a man fit to give counsel about constructions, and to take the command of labour, he will have in his breast-pocket a little book, in which many of the most precious oracles and most pregnant formulæ are stamped with the great name of Hodgkinson. And this is fame—fame that will grow higher and speak louder, as the world grows happier and wiser. For ages and for ages the discoveries of Hodgkinson will economise millions of treasure to the Governments and the nations of the globe, and prevent the destruction of innumerable lives."

A Paper by Professor W. Thomson, LL D., F.R.S., Honorary Member, was read, entitled "*Observations on Atmospheric Electricity.*" I find that atmospheric electricity is generally negative within doors, and almost always sensible to my divided ring reflecting electrometer. I use a spirit lamp, on an insulated stand a few feet from walls, floor, or ceiling of my lecture room, and connect it by a fine wire with the insulated half ring of the electrometer. A decided negative effect is generally found, which shows a potential to be produced in the conductors connected with the flame, negative relatively to the earth by a difference amounting to several times the difference of potentials (or electro motive force) between two wires of one metal connected with the two plates of a single element of Daniell's. I have tested that the spirit lamp gives no *idio-electric* effect amounting to so much as the effect of a single cell. The electric effect observed is, therefore, not due to thermal or chemical action in the flame. It cannot be due to contact electrifications of metallic or other bodies in conductive communication with the walls, floor, or ceiling, because the potentials of such must always fall short of the difference of potentials produced by a single cell. I have taken care to distinguish

the observed natural effect from anything that can be produced by electrical operations for lecture or laboratory purposes. Thus I observe generally in the morning before any electrical operations have been performed, and find ordinarily results quite similar to those observed on the Monday mornings when the electrical machine has not been turned since the previous Friday. The effect, when there has been no artificial disturbance, has *always been found negative, except two or three times*, since the middle of November, but trustworthy observations have not been made on more than a quarter of the number of days. A few turns of the electrical machine, with a spirit lamp on its prime conductor, or a slightly charged Leyden phial, with its inside coating positive put in connection with an insulated spirit lamp, is enough to reverse the common negative indication. Another very striking way in which this may be done is to put a *negatively charged Leyden phial* below an uninsulated flame (a common gas burner, for instance). The flame, becoming positively electrified by induction, keeps throwing off, by the dynamic power of its burning, portions of its own gaseous matter, and does not allow them to be electrically attracted down to the Leyden phial, but forces them to rise. These, on cooling become, like common air, excellent non-conductors,* and, mixing with the air of the room, give a preponderance of positive influence to the testing insulated flame (that is to say, render the air potential positive at the place occupied by this flame). Half an hour, or often much more, elapses after such an operation, before the natural negatively electrified air becomes again paramount in its influence on the testing flame. That either positive or negative electricity may be carried, even through narrow passages, by air, I have tested by turning an electric machine, with a spirit lamp on its prime conductor, for a short time in a room separated from the lecture room by an oblique passage about two yards long, and then stopping the machine and extinguishing the lamp, so as to send a limited quantity of positive electricity into the air of that room. When the lecture room window was kept open, and the door leading to the adjoining room shut, the testing spirit lamp showed the natural negative. When the window was closed, and a small chink (an inch or less wide) opened of the door, the indication quickly became positive. If the door was then shut, and the window again opened, the natural effect was slowly recovered. A current of air, to feed the lecture room fire, was found entering by either door or window when the other was shut. This alternate positive and negative electric ventilation may be repeated many times without renewing the positive electricity of the adjoining room by turning the machine afresh. The out of doors air potential, as tested by a portable electrometer in an open place, or even by a water dropping nozzle outside, two or three feet from the walls of the lecture room, was generally on these occasions positive, and the earth's surface itself, therefore, of course, negative;—the common fair weather condition, which I am forced to conclude is due to a paramount influence of positive electricity in higher regions of the air, notwithstanding the negative electricity of the air in the lower stratum of the earth's surface. On the two or three occasions when the in-door atmospheric electricity was found positive, and, therefore, the surface of the floor, walls, and ceiling negative, the potential outside was certainly positive, and the earth's surface out of doors negative, as usual in fair weather.

* I find that steam from a kettle boiling briskly on a common fire is an excellent insulator. I allow it to blow for a quarter of an hour or more against an insulated electrified conductor, without discovering that it has any effect on the retention of the charge. The electricity of the steam itself, in such circumstances, as is to be expected from Faraday's investigation, is not considerable. Common air loses nearly all its resisting power at some temperature between that of boiling water and red hot iron, and conducts continuously (not, as I believe, is generally supposed to be the case, by disruption) as glass does, at some temperature below the boiling point, with so great ease as to discharge any common insulated conductor almost completely in a few seconds.

MICROSCOPICAL SECTION.

February 17, 1862.

Professor Williamson, *F.R.S., President of the Section, in the Chair.*

Soundings were acknowledged from Captain W. B. Hall, of the P. and O. S. S. *Tagus*, taken off Ushant, Coast of France, and from Captain J. R. Husband, ship *Florence Nightingale*, taken off the Coasts of Patagonia and Tierra del Fuego.

Professor Williamson called the attention of the Section to the new rotifer (*Cephalosiphon Limnias*), recently discovered by Mr. H. J. Slack, in a pond at Hampstead, and an account of which appeared in No. 1 of the *Intellectual Observer* of the present month. Attention was specially directed to the fact, that the animal only possesses *one* of those organs that have been designated "respiratory tubes," "calcars," and "tactile organs;" whereas the Floscularian Rotifera, when furnished with them have two. It remains to be ascertained from a study of the embryo, whether this is the typical condition in the Cephalosiphon, or whether there were primarily two, the missing one having been suppressed during the development of the embryo, as sometimes occurs amongst higher animals.

The Secretary read a Paper, by Mr. Thomas Davies of Warrington, "*On Crystallisation.*" Mr. Davies treats more particularly upon some of the double salts, which show beautiful combinations of form and colour by polarised light; and upon his method of obtaining determinate flower-like forms, surrounded by a film of the uncrystallised salt. The novelty of the Author's system, consists in the following particulars:—he makes a nearly saturated solution, say of the double sulphate of copper and magnesia; he dries rapidly a portion on a glass slide, allowing it to become so hot as to fuse the salt in its water of crystallisation; there then remains an amorphous film on the hot glass. On allowing the slide to cool slowly, the particles of the salt will absorb moisture from the atmosphere, and begin to rearrange themselves on the glass, commencing from points. "If then placed under the microscope," says the Author, "we shall see points starting up here and there, and from those centres, the crystals may be watched as they burst into blossom, and spread their petals on the plate." Starting points may be made at pleasure by touching the film with a fine needle, to enable the moisture to get under it; but this treatment renders the centres imperfect. If allowed to go on, the crystals would slowly cover the plate, or if breathed upon, they form immediately; whereas, if it is desired to preserve the flower-like forms on a plain ground, as soon as they are large enough, development is suspended, by again applying gentle heat; the crystals are then covered with balsam and thin glass, to be finished off as usual. The balsam must cover the edges of the film, or moisture will probably get under it, and crystallisation go creeping on. Many crystals which produce similar forms, cannot be preserved in balsam; in the hyposulphite of soda they are very fine, and the Author is endeavouring to preserve them in castor oil.

Mr. Sidebotham referred to the vegetable forms produced by Mr. Petschler, with bichromate of potash in gelatine, exhibited at the British Association Microscopical Soirée. Since then, flower-like shapes had been obtained from nitrate of silver amongst the ramifications of the bichromate; as it is an interesting subject, he should endeavour to bring it before the next meeting.

The discussion "*On the Cause of Metallic Lustre upon the Wings of the Lepidoptera*" was resumed. Mr. Latham stated that he saw no reason to alter the views he had expressed; but since the last meeting Mr. Watson had called his attention to an article in the "Annales des Sciences Naturelles," for February, 1835, by Bernard Dechamps, "Sur les Ailes des Lepidopteres," which con-

tains much information upon the scales and the cause of their brilliance; he (Mr. Latham) had translated and printed extracts from the Paper; copies were handed round to the members present, and may be had (gratis) at the Society's rooms, or from the Secretary of the Microscopical Section.

A Paper was read by Dr. Thomas Alcock, "*On the Tongues of Mollusca.*" The Author remarked upon the great variety and beauty of these objects, and pointed out their scientific value as a help in the classification of shells. On investigation the tongues were shown to arrange themselves into four groups, according to the pattern or type of the lingual dentition, and these groups were stated to correspond with four of the orders established by Cuvier, on the characters of the breathing organs. The four orders illustrated were the Pectinabranchiata, the Scutibranchiata, Cyclobranchiata, and the Pulmonata; and he believed that, on the evidence of the teeth, it will be necessary to re-establish the order Cyclobranchiata as distinct, instead of including it in the Scutibranchiata, as is done by our latest authorities. The Author had examined many specimens of Buccinum undatum of both sexes, a series of which were exhibited to prove that the number of points on the central teeth in this species varies from five to seven, but without reference to sex. The very close agreement both in the lingual teeth and in the general internal anatomy of Fusus and Buccinum was mentioned, and a doubt was expressed as to the propriety of their wide separation in our present systems of classification. The association of Fusus with Murex, and of Purpura with Buccinum, was also commented upon, and shown by the evidence of the tongues, as well as the general anatomy of the animals, to be clearly incorrect. In conclusion, some remarks were made on the method used by the Author, of extracting the tongues from the different kinds of mollusca, illustrated by specimens, some of which were many times longer than the bodies of the animals, being coiled up near the neck, and brought forward as the teeth are worn away. The paper was illustrated by beautifully-executed drawings of the different types, and a series of dissected animals.

Professor Williamson asked the Author if he proposed to include the Chitons in the order Cyclobranchiata.

Dr. Alcock was satisfied the Chitons ought not to be so placed; but, judging from the general character of their teeth, he thought they might possibly remain with the Scutibranchiata, or perhaps it would be necessary to establish a new order expressly for them.

Professor Williamson was glad to learn that this was the result of Dr. Alcock's observations; for the animals certainly appeared very distinct, and the Chitons, which he remarked have the form of a gigantic wood-louse, were evidently not Cyclobranchiata, as they have a separate gill down each side of the body.

On adjourning to the microscopes, Dr. Alcock exhibited mounted specimens of tongues from thirty different species of Gasteropoda, with the shells from which they were extracted. One of the lingual ribands was two and a-half inches long.

Mr. Latham exhibited scales from the wings of the Catarrhactes Papua, a Penguin from the Falkland Islands, which appear to be intermediate between feathers and scales.

Mr. Edward Lund exhibited two forms of stands for holding a microscope, condenser, lamp, &c.; one in the form of a tray fixed on four castors, to run upon an ordinary table, and the other a small round turn-table, thirty inches high and fifteen inches diameter. The lamps were for paraffin oil, fitting into wooden cups secured to the stands.

Chemical Society.—The Anniversary Meeting of this Society will be held on Monday, 31st inst.; and the next Ordinary Meeting on Thursday, April 3.

PHARMACEUTICAL SOCIETY.

Wednesday, March 5, 1862.

Mr. Peter Squire, *President, in the Chair.*

(Continued from p. 168.)

A Paper, by Mr. Marriage, was read, "*On the Estimation of Tannic Acid in Galls.*" The Author makes use of a standard solution of ammonio-sulphate of copper, which gives an olive-green precipitate with tannic acid. He dissolves 334 grains of sulphate of copper, and having added sufficient ammonia to re-dissolve the oxide, makes up the solution to a decigallon. Two septems of this solution will precipitate one grain of tannin. In testing, the Author operates on 100 grains of galls, and then half the number of septems gives the per-centage of tannic acid.

Mr. Wood doubted the value of the process as a general method of estimating tannic acid. Sulphate of copper gives a precipitate with so many vegetable infusions. He thought the gelatine process, as improved by Mulder, very superior. The addition of alum caused the precipitate to coagulate and go down readily; and in this process there was a definite reaction.

Professor Redwood said the substance of the next Paper announced—"*On Henbane,*" by Mr. Procter—was a statement of the discrepancies existing between various writers as to the doses and effects of the several preparations of the Hyoscyamus Niger.* Mr. Procter also urged the need of a revised Posological Table.

The Chairman said that the Pharmacopœia Committee had decided not to give any table of doses with the Pharmacopœia. When a pharmaceutist was in doubt about a dose in a prescription, he should always apply to the prescriber. The meeting then adjourned.

CHEMICAL SOCIETY.

Thursday, March 20, 1862.

Dr. A. W. Hofmann, *F.R.S., President, in the Chair.*

Mr. A. Church read a Paper "*On the Isolation of Phenyl.*" The Author stated that he had already pointed out the method of preparing chlorinetted compounds of benzol, and it was from one of these that he had attempted to obtain phenyl. In the commencement it was necessary to prepare pure benzol; this was accomplished by agitating commercial benzol with oil of vitriol and bichromate of potash, rectifying, distilling from ethylate of potassium and free potassium, and again rectifying. The compound thus obtained solidified wholly in a freezing mixture, when mixed with from three to four times its volume of sulphuric acid it dissolved, and this solution, when mixed with an alkali, furnished almost the theoretical amount of sulphobenzolate of the base.

In order to prepare the chlorinetted compound, some bichromate of potassium was placed in a large glass balloon, and hydrochloric acid added, as soon as an action begins, the pure benzol was poured in, and the whole allowed to digest for some days, after which a thick oily body will be found to have formed, from which any unaltered benzol may be separated by distillation in a water bath. The oily liquid obtained in this manner appeared to be a mixture of the two bodies $C_6 H_5 Cl$ and $C_6 H_6 Cl_3$ The effect of an alcoholic solution of potash was then tried, in the hope of removing from the latter compound an equivalent of hydrochloric acid, and so converting it into the former; The potash, however, removed all the chlorine from the body, forming carbolic acid. By moderating the action of the potash solution, a more successful result was obtained. On exposing the substance produced in this manner to the

* We have heard of a hospital patient who ate about half a pound of extract of henbane by the teaspoonful in mistake for a cough linctus, and was neither better nor worse for the substitution.—Ed. Chemical News.

action of an amalgam of potassium, a mixture of four oily bodies was formed; these were separated by fractional distillation and analysed, their composition was as follows, beginning with the most volatile compound; $C_{12}H_{10}$; C_6H_6; C_6H_4; $C_{12}H_{10}$; the first and last being isomeric with phenyl, the second being benzol, and the third possibly the phenylene of the series corresponding to ethylene in the alcohol series.

The President inquired whether the specific gravity and vapour density of these bodies had been determined.

Mr. Church replied that these were the points to which he should next turn his attention.

The next Paper was by Dr. Letheby, "*On the Application of Voltaic Electricity to the Detection of Aniline.*" Nitrobenzol, when administered internally, becoming reduced while in the system to aniline, it was desirable to possess a delicate test for this latter substance. The addition of an oxydizing agent to a solution of aniline, gave rise to the production of a blue colour; but this test was rather unsatisfactory if very small quantities of the base were present. This being the case, it occurred to him that the nascent oxygen arising from the positive pole of a voltaic battery might be advantageously employed; and to prove this, a small quantity of aniline was dissolved in sulphuric acid, and exposed to the action of the pole: a blue colour was produced without difficulty. The blue substance, which was insoluble in water, alcohol, ether, &c., was dissolved by concentrated sulphuric acid, and when exposed to the action of a reducing agent, such as a mixture of sulphate of iron and lime, became changed to a colourless body. It was intended to investigate the composition and properties of this substance.

Dr. Odling suggested that this blue compound might possibly be indigo, or a homologue of indigo, its properties bearing out such a supposition.

The President remarked that although there might be reasons for supposing that this substance was homologous with indigo, still those who had experimented on the subject were of a contrary opinion.

NOTICES OF BOOKS.

Report on the Past, Present, and Future of the Royal Institution, chiefly in regard to its Encouragement of Scientific Research. By the Honorary Secretary. London: W. Clowes and Sons.

We give the following abstract of a Report which has just been made to the Committee of Managers of the Royal Institution:—

The uncertain and insecure position of the Institution, in consequence of the probability of its Professors being tempted away, and the evidence that they had been obliged always to give a portion of their time elsewhere, because they could not live upon what they received from it, was so clear, that the Secretary was asked to make a Report to the Committee of Managers on the state of the Institution, looking at it from this point of view only.

In this Report it is shown—1st. What the Institution was originally, and how it gradually changed its objects; 2nd. What fixed sums were and are paid to our Professors; 3rd. How great the general changes are in the encouragement and extension of natural knowledge since the Institution was founded.

The conclusion arrived at is, that though the Institution may partially instruct a comparatively small number of persons by first-rate lectures, still, from the extension of education in natural knowledge, it has no longer the function it performed almost exclusively in the days of Young and Davy. By Davy's and Faraday's researches the laboratory of the Institution has become the most celebrated in the world. Original investigation has become its work; and to secure scientific discovery hereafter, instead of paying Professors as they were paid sixty years ago, they must be enabled to resist the temptations which the Government and private Societies may offer; temptations which are increasing daily, and which a few years back did not exist.

The first Section of the Report explains what the Institution was originally, and how it gradually changed its objects.

On March 3, 1810, previous to the Royal Assent being given to the Act of Parliament for enlarging the powers granted to the Royal Institution, Sir H. Davy gave a Lecture on "The Plan for Improving the Royal Institution, and Rendering it Permanent." He says—"The first plan (in 1799) of the Royal Institution was that of a school for promulgating the knowledge and use of important mechanical inventions, for connecting the views of men of science and artisans, and for laying open the general principles, and teaching the applications of all improvements in the sciences in their connexion with the arts of life."

"The establishment of a collection of models in the arts failed," "and the object which at first was only secondary, that of teaching the principles of the sciences and the application of the sciences by courses of public lectures, soon became the prime object."

In this form the Royal Institution "has not only assisted the progress of genuine science, but has likewise diffused a general knowledge of the advantages and importance of scientific pursuits."

"Besides the diffusion of knowledge, the new plan will also embrace a design for the promotion of knowledge by experiments and original investigations." Again, he says—"In the original plan of the establishment the prime object was the foundation of a collection of models, and the diffusing the knowledge of useful mechanical inventions, but this idea being found impracticable, and new and more exalted objects having arisen from this foundation," the Royal Institution fortunately took the form of a body for promoting experimental science, and for diffusing every species of philosophical knowledge." And again—"The (new) object of the Royal Institution is to promote the progress of science."

In the second section it is shown what fixed sums were and are paid to the Professors of the Institution.

When in September, 1799, Dr. Thomas Garnett was engaged by the Managers of the Institution as Professor and Public Lecturer in Experimental Philosophy, Mechanics, and Chemistry, at a salary of 300*l.* and apartments, there were no lectures given in London on these subjects except in the Medical Schools. In 1597, Lectures on Astronomy were first delivered at the Gresham College, and the Bakerian Lecture (established in 1775) was read at the Royal Society, but no public instruction was given in science in London. The Gresham Lectures were in Latin, and the Bakerian was given to the Fellows of the Royal Society.

Dr. Thomas Young was appointed Professor in July, 1801, at a salary of 300*l.* and rooms; and his course of thirty-one lectures on Natural Philosophy in 1802, and that of sixty lectures in 1803, mark the time when the highest class of lectures on scientific subjects was first given to the general public in London.

In February, 1801, Sir H. Davy was engaged as Assistant-Lecturer in Chemistry, and Assistant-Director of the Laboratory, at a salary of 105*l.*, one room, coals, and candles. In 1802 he was made Professor of Chemistry, with 200*l.*, and in 1803 with 300*l.*; and in 1805 he was made Director of the Laboratory, with an additional 100*l.* per annum; so that sixty years since our Professors at the Royal Institution, Young and Davy, received 300*l.* and 400*l.* per annum for teaching the principles of science, when no other lectures in London were given for diffusing useful knowledge, and long before Government had made

the discovery "that the prosperity and riches of the country were intimately connected with the progress of the arts and sciences."

In 1807, Davy's grand discoveries began to show that scientific research would become the glory of the Institution.

In April, 1813, a year after his marriage, he resigned his Professorship and Directorship of the Laboratory, and was made Honorary Professor of Chemistry. On the 28th of August, 1813, he writes to a friend—"Having given up lecturing, I shall be able to devote my whole time to the pursuit of discovery."

In 1813, Mr. Faraday was appointed Assistant in the Laboratory at 25s. per week, with two rooms. At that time the Royal Institution was renowned throughout Europe for Davy's electro-chemical discoveries.

In 1816, Mr. Faraday's salary was raised to 100*l.* per annum.

In 1825, he was appointed Director of the Laboratory; the funds of the Institution could not admit of an increase of his salary.

In 1833, he was chosen for the Fullerian Professorship of Chemistry by Mr. Fuller. This was endowed with 100*l.* per annum.

In 1853, the amount he received was 300*l.* per annum as Superintendent of the House and Director of the Laboratory.

The electrical discoveries which have been made by Mr. Faraday in the Institution began to be published in 1831, and are not yet ended. His first chemical paper was published in 1816. He has worked long and much for the love of the Institution, and little for its money. For forty years, from 1813 to 1853, his fixed income from the Institution was not more than 200*l.* per annum.

In 1853, Professor Tyndall was elected to lecture on Natural Philosophy for 200*l.* per annum.

In 1859, he received 300*l.* per annum.

Thus the Royal Institution, from being a Society for the promotion of useful knowledge by instruction, became and remains a Society for the promotion of the progress of science by experiment. The amount it has been able to give its Professors for either object has been the same. And after sixty years of grand discoveries, including the laws of electro-chemical decomposition; the decomposition of the fixed alkalies; the establishment of the nature of chlorine; the philosophy of flame; the condensibility of many gases; definite electrolytic action; the science of magneto-electricity; the twofold magnetism of matter; the magnetism of gases; the action of magnetism and electricity on polarised light; and the radiation and absorption of heat by gases and vapours; but little more can be done for the discoverers than was done at the beginning of the century.

The third section shows the encouragement and extension which have been given to natural knowledge, chiefly since the Royal Institution was founded.

During the last half century the extension of instruction in natural knowledge has been very great. Its progress may partly be measured by the amount of encouragement given to scientific men for publicly and privately promoting the education in this knowledge during this century, and partly by the changes which are being made in the Universities and Schools.

The following Statement of the Sums paid for Scientific Objects, or to Scientific Men, by the Government, is taken from the "Civil Service List," for March, 1860:—

	£
Royal Mint—Master and Worker	1500
Science and Art Department—Secretary and Superintendent	1200
,, Assistant Secretary	775
Queen's College, Cork—President and Director of Irish Museum	1200
Director-General of Geological Survey, and Director of Geological Museum	1100
Astronomer Royal, House and	1000
British Museum—Superintendent of Nat. Hist. Department	800
,, Keeper of Zoology	500
,, Keeper of Mineralogy	500
,, Keeper of Botany	500
,, Keeper of Geology	500
Director of Kew Botanic Gardens	800
Registrar of London University	800
Superintendent of Coining	700
Woolwich Arsenal—Chemist	700
,, Assistant	280
Inland Revenue Laboratory—Principal	600
,, Assistant	300
Queen's College, Belfast—Vice-President	500
Superintendent of Nautical Almanack	500
School of Mines—Lecturer on Chemistry and Chemist	300
,, Lecturer on Physics	200
India Office—Reporter on the Products of India	500
School of Mines—Lecturer on Natural History, and Naturalist to Geological Survey	625?
School of Mines—Lecturer on Geology, Local Director of Geological Survey	550?
Geological Survey, Ireland—Local Director, Lecturer on Geology, Museum of Irish Industry	650?

The following summary of the sums required for the Department of Science and Art for the years ending March 31, 1861 and 1862, shows well what the Government now does as compared (not with the time when the Royal Institution was founded, but) with the year 1843, when the Grant commenced with 2008*l.* for the Museum of Economic Geology:—

	1861-2. £ s. d.	1860-1. £ s. d.
For General Management in London	4,705 0 0	4,560 0 0
For Schools of Art and Science in the United Kingdom, South Kensington Museum, Library, &c.	76,405 0 0	77,415 0 0
For School of Mines and the Geological Museum, Jermyn Street, London	6,387 2 6	6,417 2 6
For Geological Survey of the United Kingdom	10,798 1 6	10,317 19 6
For Industrial Museum for Scotland, including the Natural History Museum, Edinburgh	1,931 12 0	1,943 16 0
For Royal Dublin Society	6,000 0 0	6,000 0 0
For Museum of Irish Industry and Provincial Lectures in Ireland	4,956 16 0	4,996 16 0
Royal Hibernian Academy	300 0 0	300 0 0
	£111,483 12 0	£111,950 14 0

The sums actually expended in the year ending March 31, 1861, for Scientific Education and connected matters, and National Collections, were 221,851*l.*:—

	£
British Museum	92,800
National Gallery	10,348
Scientific Works and Experiments	3,488
Science and Art Department	97,472
Museum of Practical Geology	6,705
*Royal Society	1,000
Geographical Society	500
Portrait Gallery	1,538

The extension of education in Natural Knowledge, since the founding of the Royal Institution, is also shown by the changes which are taking place in the Universities and Schools quite independently of the Government.

In the University of London degrees have been granted in Natural Knowledge since 1860.

At Oxford there are Professors of Chemistry, Physics and Geology receiving now a fixed income of 310*l.* each, which it is proposed to increase to 400*l.* yearly.

There is a Professorship of Natural Knowledge at Magdalen College, which soon will receive 600*l.* yearly.

The Linacre Professor of Physiology will soon be paid, by Merton College, 800*l.* yearly.

At Cambridge a Chemical Laboratory has been opened at St. John's College; 150*l.* with the fees are given to the Director, who is a Fellow of the College.

And the Professorships of Chemistry, Anatomy, Botany, Geology, Mineralogy and Mechanics, have recently been raised to 300*l.* a year each, besides fees for lectures, the amount of which varies.

* See subsequent Note.

Letters printed in the Appendix to this Report well show how instruction in Natural Knowledge is extending, slowly but surely, even in our public schools.

From these facts it is evident that "science and useful knowledge" are now "promoted, diffused, and extended" very much more than in 1800, when the Royal Institution of Great Britain was established by charter for these objects. It still does important work in teaching; but if this were its only work it would, before long, be obliged to yield to other more richly-endowed places of instruction; for its Professors would be paid more for teaching elsewhere. As an Institution for instruction alone, it could not keep its Professors. The antecedents of the Royal Institution and the opportunities for experimental research make them wish to stay, but Government and private Institutions tend to draw them away.

Three or four hundred a year, with or without rooms, is the sum which the Institution gave to Young and Davy, and does give now to Dr. Faraday and to Dr. Tyndall; whilst the Government can give places at the Mint, the School of Mines, and elsewhere, of eleven, twelve, or fifteen hundred a year to those who have worked or are working for the promotion of education in science.

(To be continued.)

Metallurgy. By John Percy, M.D., F.R.S. London: John Murray, Albemarle Street.

(second notice.)

According to the general plan of this work, which was briefly sketched in a former notice, we proceed with the history of the metals copper and zinc, and their alloys, brass and sheathing metal. Such is the importance attached by the Author to this division of the subject that he has devoted no less than 277 pages to the consideration of copper alone. The account of this metal commences with a brief historical sketch of its early extraction and application; then follows a description of the physical properties of copper and of the compounds formed by the union of this metal with oxygen, sulphur, and the several fixed acids, such as boracic, silicic, &c.; the chemical changes brought about during the process of roasting the disulphide of copper, and the fundamental reaction which lies at the basis of copper smelting,—viz., the elimination of sulphur by the mutual decomposition of the oxides and the disulphide, whereby the metal is reduced and the gaseous elements expelled in the form of sulphurous acid. Seven examples are quoted as furnishing illustrations of the different reactions which may occur under varying circumstances. Thus, by melting together disulphide and protoxide of copper in the proportion of one equivalent of the former and two of the latter, the whole of the metal, amounting to four equivalents, will become reduced, with elimination of sulphurous acid; or, by employing the red oxide of copper in place of the black protoxide, the same result will follow, as appears by the following comparison:—

$$Cu_2S + 2CuO = Cu_4 + SO_2.$$
$$Cu_2S + 2Cu_2O = Cu_6 + SO_2.$$

The sulphate of copper exerts a similar reducing action upon the disulphide when mixed in the proportion of equal equivalents, thus:—

$$Cu_2S + CuO,SO_3 = Cu_3 + 2SO_2.$$

Dr. Percy proceeds with the description of a number of reactions of minor interest, which have reference to the modifications introduced by the presence of lead, manganese, iron, &c., and in the next place details the results of a series of original experiments upon the reduction of the sulphide by fusion with other metals, whereby the Author demonstrates that, as a general rule, the sulphur is shared with the second metal, a certain quantity of copper becoming at the same time reduced and alloyed with the remainder. A very instructive example of this kind of reaction is one wherein a definite alloy of copper and tin is produced of the following formula:—

$$2Cu_2S + Sn_2 = CuSn + \left[\begin{matrix}Cu_2S + Cu \\ Sn\end{matrix}\right\} S.\right]$$

The foregoing introductory observations prepare the student for a very lengthened discussion of the qualities of ingot copper termed "tough-pitch," "overpoled," and "dry copper." These several kinds of metal appear to differ from each other chiefly in respect to slight variations in the proportion of oxygen; thus, overpoled copper is shown by Mr. Dick's analytical results to be in that condition wherein the copper is associated with a minimum of oxygen, and whilst there is no conclusive evidence of its actually absorbing carbon in the operation of stirring the fluid metal with small coal and green wood, it appears likely that the foreign metals which exist in small quantities as impurities in the copper are by this process completely reduced, and exert an injurious influence upon the tenacity of the metal cast under these circumstances. It seems more probable, however, that the admixture of a certain proportion of suboxide is necessary in the ordinary tough-pitch copper, and that its presence confers upon the metal malleability and other well known and much valued working qualities. Copper wire is always drawn from this description of cake, and furnishes, according to Mr. Dick's experiments, on the average about three per cent. of suboxide. Such wire, heated for some time in a current of pure and dry hydrogen gas, became extremely brittle with partial loss of metallic lustre, and afforded that proportion of water from which the amount of oxygen was deduced. In a similar manner the suboxide contained in characteristic specimens of "dry copper" was found to amount to ten per cent. and upwards.

Admitting the preceding conclusions to be correct, the Author remarks that "it seems probable that the proportion of dioxide of copper required to produce the highest degree of malleability in tough-pitch copper will vary, to a certain extent, with the nature of the foreign metals, or other matters which may be present in commercial copper. But this is a point which can only be determined by very careful experiments. It need scarcely be remarked that, as copper passes from the state of tough-pitch to that of overpoled to the maximum, the proportion of dioxide is gradually reduced, until at last not a trace remains; so that copper much overpoled, but not to the maximum, may yet retain a sensible amount of dioxide."

The causes of rising and sinking in the mould at the time of casting the ingot copper is made the subject of some speculative remarks which appear to indicate the want of more definite experimental results before these curious phenomena can be said to have received a full explanation. Whether the vesicular character of certain kinds of copper is due to the disengagement of gas at or near the moment of solidification is a point which the Author reserves for future investigation; its effect in reducing the specific gravity is so marked that one small ingot had a density only of 7·85 instead of 8·958, which appears to be the highest result yet obtained for a sound casting of pure copper.

(To be continued.)

NOTICES OF PATENTS.

1238. *Materials to be Used in the Process of Dyeing and Printing.* J. Riley, Hapton, near Accrington. Dated May 15, 1861. (Not proceeded with.)

Instead of employing stannate of soda alone as the basis of the mordant used in fixing certain colours in dyeing and calico printing, Mr. Riley recommends the addition of tungstate of soda for the purpose of forming a

compound salt which appears to possess advantages as a mordant.

The employment of tungstate of soda and other compounds of tungsten for the purpose of producing mordants suitable for calico printing appears to have been anticipated by the patent claim of J. Meyer, (No. 1072,) which bears date April 30, 1861.*

1266. *The Manufacture of Artificial Alizarine.* W. Clark, Chancery Lane, London. A Communication. Dated May 17, 1861. (Not proceeded with.)

This invention relates to the preparation from naphthaline of a red colouring matter, which, on account of its exhibiting many of the characters of that produced from madder, has received the name of "artificial alizarine." The general instructions contained in the specification are so very similar to those described in connection with another claim, (No. 1087,) † that it is sufficient to point out the coincidence of pewter being employed in both cases for effecting the transformation of the nitro-compound, and to state that methods of purification are resorted to in the present instance depending upon the solubility of the alizarine in alkalies and its re-precipitation by an acid. The process of sublimation is mentioned as being also available for the purification of this substance.

1269. *Combining together Siliceous Powder, into Solid Masses of Any Form, by Means of Sulphur.* A. C. Ponton, Arlington Villas, Clifton. Dated May 18, 1861. (Not proceeded with.)

According to this invention it is proposed to manufacture a kind of stoneware from powdered flints, ground quartz or sand, tripoli, or siliceous powder from any other source, mixing with either of these a sufficient quantity of melted sulphur to bind the particles together, and kneading them into a mass which will, by pressure in a mould, take any form that may be desired. A colouring pigment may be incorporated with mixture of sulphur and silica to vary the tint of the ware.

The chief disadvantage attending the use of this novel kind of stoneware would consist in its inability to withstand the heat of boiling water, and its liability to fracture from slight mechanical causes, on account of the brittle nature of the agglutinating material. The employment of sulphur is objectionable also from the circumstance of its rapidly tarnishing the polished surfaces of copper, silver, and other metals, when brought in contact or placed sufficiently near to come within the sphere of its influence.

1316. *Ascertaining and Removing Impurities Contained in Water.* F. H. Danchell, Red Lion Square, London. Dated May 24, 1861. (Not proceeded with.)

This specification describes a volumetric method for determining the nature and amount of the impurities contained in any sample of water. For this purpose a series of standard solutions are employed, each of which is intended to serve for the detection of a particular ingredient, these are prussiate of potash, bichromate of potash, permanganate of potash, caustic baryta, chloride of zinc and acetate of lead. Having discovered by such preliminary testing the chief constituent, or impurity, the second part of the invention is directed to the removal of the same, which is accomplished by filtration through a suitable purifying medium. This material is, by preference, composed either of animal or vegetable charcoal alone, or the same in conjunction with other matters having an affinity for the particular impurity which is required to be removed in the course of filtration.

With regard to the recommendation for the employment of wood and animal charcoal in filters intended for the purification of water, there is no consideration which could possibly be advanced in support of the novelty of this claim. The proposal for the application of other materials in admixture is far from being specific.

Grants of Provisional Protection for Six Months.

2972. Charles Stevens, Charing Cross, London, "An improved indelible anti-corrosive ink."—A communication from Louis Croc, Paris.—Petition recorded 26th November, 1861.

150. John Stenhouse, Upper Brunswick Terrace, Barnsbury Road, London, "Improvements in the protection of metallic surfaces, and in rendering certain substances less pervious to air and moisture."—Petition recorded 21st January, 1862.

228. Rudolph Bodmer and William Wilson, Newport, Monmouthshire, "Improvements in the process of manufacturing artificial stones, parts of which improvements are also applicable to the manufacture of artificial fuel."—Petitions recorded 28th January, 1862.

317. Edward Cooper Willis, Addison Road, Kensington, London, "Improvements in the treatment of wax and other substances of a similar nature."

325. Hugh Adams Silver, Silvertown, Essex, "Improvements in the manufacture of trays, cases, and other similar articles and bottles in ebonite, vulcanite, or other hard india-rubber."

397. Arthur John Dodson, Clapham, Surrey, "An improved composition for coating, covering, or protecting ships' bottoms, applicable also for coating or covering railway sleepers, telegraphic wires, and other surfaces, and likewise as a cement and as a substitute for metal for certain constructive purposes."

414. Robert Bell, Westland Row, Dublin, "Improvements in treating fabrics or articles composed of animal and vegetable substances for the purpose of separating one class from another."

444. William Davis, Spencer Road, Stoke Newington, London, "Improvements in increasing the illuminating effect of coal-gas and other gases."

CORRESPONDENCE.

The Divisibility of Matter.

To the Editor of the Chemical News.

Sir,—We are led to infer, from various properties of matter, that it consists of an immense collection of molecules, and that these are not continuous, but are aggregates of smaller particles called atoms. These minute particles are retained side by side, although without actual contact, by a powerful attractive force. This subject is noticed in most physical and chemical works, and it has been the subject for discussion between many eminent philosophers; yet up to the present no definite conclusion has been arrived at. The more we study the subject, with greater force does this question present itself to our minds, "Has the divisibility of matter any limit?" From a careful study of the subject, I have arrived at the conclusion that it "has no actual limit;" experiment fails to determine any. Matter may be and is continually being divided into infinitesimal particles or globules. A small piece of orris-root will continue for a long lime to spread around its fragrant perfume, yet does not lose any appreciable weight. Matter certainly does continue to be divisible long after it ceases to affect our senses. This subject was noticed in the Chemical News for January 18, 1862, when mechanical illustrations were given to show the minute subdivision attainable by these means. Chemical illustrations may fall short, yet, by careful manipulation, a very minute subdivision may be shown by them. Ganot tells us, "Blood is composed of red, flattened globules, floating in a colourless liquid called serum. In man the diameter of one of these globules is less than $\frac{1}{5000}$ths of an inch,

* See Chemical News, Vol. V. p. 97.

† See Chemical News, Vol. V., p. 97.

and a drop of blood which might be suspended from the point of a needle would contain about a million of these globules." How minute they must be; yet we are able to carry this division still further. One drop of sulphuric acid may be equally diffused through two or more million times its bulk of water, and can be detected in any part by concentration and applying the proper test. When a perfectly soluble salt is put into water, disintegration of its particles takes place, and the salt is equally diffused through the liquid, each atom of water containing an atom of salt. If we take one grain of barium, and dissolve it as chloride in 500 grains of water, disintegration takes place, and each grain of water holds in solution $\frac{1}{500}$th part of a grain of the salt. If we then add more water, further disintegration of particles takes place, until each atom of water contains an atom of salt, when it is stayed. If we again add water, disintegration again takes place, and thus we may go on until one grain of the water contains only $\frac{1}{5000000}$th of a grain of the salt; and yet we are able to detect it by concentrating a portion and applying the proper test. I believe we might go on for an almost indefinite quantity, and yet if we had tests sufficiently delicate, we should find the salt equally diffused through the liquid. I have subdivided iron to the $\frac{1}{2600000}$th of a grain, and yet have been able to detect it in any portion with ferrocyanide of potassium, which strikes a uniform blue colour. We almost start with astonishment at the fineness of the subdivision, and are totally unable to form any adequate idea of the minuteness of these globules. Here a large field of labour presents itself to the student, in which he will find each day something interesting and equally astonishing.

I am, &c. E. J. Rutter.

Uttoxeter.

MISCELLANEOUS.

CURIOSITIES IN CHEMICAL EVIDENCE.

STAFFORD ASSIZES.

We take the following from the reports of the trial Timmins *v.* the Birmingham and Staffordshire Gas Company, which occupied the Court the greater part of three days. Knowing the danger of trusting in such matters to reporters unacquainted with Chemistry, we have collated the reports of the *Staffordshire Advertiser* and the *Birmingham Daily Post*, and, finding that the two substantially agree, we infer that the evidence is correctly given. The action was brought against the Gas Company for fouling the plaintiff's well by leakage from a gas-tank. To prove the case,

Mr. Duncalfe, Surgeon, of West Bromwich, was called, who stated that the water in the well has the smell of gas, and the same taste as water he got from his own gas-meter. The peculiar flavour of water impregnated with gas was due to benzole. The most delicate test used to detect its presence in water, nitric acid, would only detect one part in 2000 of water. *Cross-examined.*—He had himself analysed water impregnated with gas. The way he tested the water in the well was this: Supposing the water in his meter to be stronger than that in the well, he took some of the former and boiled it with nitric acid, —one part of the acid to one of water. Its colour was Magenta, and that was a proof that benzole was in it. He then took more water from his meter, and diluted it with water until it was brought to the same taste and smell as the water from plaintiff's well. *Re-examined.*—The nitric acid test would not discover the presence of benzole in the well-water.

Alfred Bird, Practical Chemist, Birmingham, said he had inspected the water in the plaintiff's well. As to its flavour, it contained the volatile aroma of the hydrocarbons which arose in the distillation of gas; in other words, the smell of gas. He tested it to detect benzole, but it was too much diluted. He next examined the water in the defendant's tank. He tested five ounces of the tank-water with fifteen drops of strong nitric acid, and the aniline colour (Magenta) became developed. One drop of benzole to four ounces of pure water developed the Magenta colour. When this mixture was diluted to the smell of the water from the plaintiff's well, the Magenta disappeared.

William Mattieu Williams, Chemical Professor at the Midland Institute, Birmingham, stated that he had examined the water from the well. He tasted and smelt it. It gave forth the smell of gas, and had a corresponding taste. Examined also the water in the defendant's tank, and found it had the same taste and smell as the well water, but stronger. Two per cent. of tank water with pure water produced the smell and taste of the well water. Adding water to benzole and nitric acid he produced the Magenta colour, but diluting that mixture with water and reducing it to the smell and taste of the water from plaintiff's well, the Magenta colour left it. *Cross-examined.*—He had tested the water from the plaintiff's well for everything relating to gas and sewage. He looked for gas, and finding that the sewage about there was impregnated with sulphuretted hydrogen, he looked for that too. Found the gas, but not sulphuretted hydrogen. He should not think by any means that 5 per cent. of phosphate of lime in water would prove the presence of excrementitious matter. He had tested for phosphates by sulphate of copper and chloride of lime, but neither precipitate nor discoloration was produced.

The defence was, that the well water was contaminated with sewage, but not with gas.

Dr. Alfred Hill said that there were no gas products in it whatever, but it contained phosphates and nitric acid, which the tank water did not. The nitric acid was no test at all (for benzole), it would not produce a Magenta or any other colour.

Dr. Wrightson, Birmingham, Professor of Chemistry, utterly denied the possibility of nitric acid being a test for benzole. A solution of benzole, pure water, and nitric acid with nothing else would never produce a Magenta colour.

Dr. Letheby could not detect coal gas in water from plaintiff's well either by smell or taste. The cellar water contained no coal gas whatever.

The plaintiff obtained a verdict.

[We shall give the evidence in the action Downing *v.* Chance in our next.]

ANSWERS TO CORRESPONDENTS.

*** All *Editorial Communications* are to be addressed to the Editor; and *Advertisements* and *Business Communications* to the Publisher at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

*** In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

Carbolic Acid, W. C.—This compound may be obtained perfectly pure by the destructive distillation of salicylic acid. Mix intimately together equal weights of salicylic acid and powdered glass; introduce the mixture into a good German retort and heat on a sand bath, gradually raising the heat till it becomes red-hot at the bottom. It is not necessary to use an adapter, as the carbolic acid condenses very readily. If the materials are perfectly dry, it solidifies to a mass of crystals as soon as it condenses, but if there be a trace of water present it remains liquid. The slower it distills over, the lighter will be the colour, while if a high temperature be employed, it comes over nearly black. It may be rendered colourless and anhydrous by rectification over quick lime. It is poisonous, has a sweet pungent taste, and peculiar odour, and is a powerful antiseptic.

A Subscriber Abroad.—We regret that we cannot furnish the desired information, you had better write to the authors of the paper.

E. Sherratt.—We will answer our correspondent in an early number.

R. J. Rutter.—The experiments would be very interesting, and we should much like to have an account of them.

Book Received.—"On Dr. Van Hecke's System of Warming and Ventilating." By W. W. Phipson. Reprinted from the *London Medical Review*.

THE CHEMICAL NEWS.

VOL. V. No. 122.—April 5, 1862.

THE EVIDENCE OF "EXPERTS."

THE evidence of "*Experts*" is just now the object of general derision. Smart newspaper writers, wishing to indite a telling sarcastic article, select the discrepancies in scientific evidence for a theme; noble Lords, anxious to enliven the dull debates of our hereditary legislators, find nothing so provocative of laughter as a story about the differences of "mad doctors;" and barristers, ready to advocate any opinion, and anxious, perhaps, for a monopoly of the "any-sidedness," when addressing a jury, dilate with well-simulated indignation on the fact that eminent scientific men are to be found in the witness-box on opposite sides. But there is nothing in these differences to excite astonishment. Scientific men are constantly called upon to express opinions on matters which do not admit of demonstration, and about which men may conscientiously come to different conclusions. Whether a fever that raged in a certain neighbourhood was or was not caused by a stench evolved from some manufactory in the vicinity, must be a matter of opinion; for at present it is incapable of proof or denial either way. Whether a given substance is or is not coal, must remain a matter of opinion until philosophers have arrived at the true definition of a coal. Whether the act of subscribing to the Society for the Conversion of the Jews is a proof of insanity in the subscriber; or whether the doctor who urges the fact as a proof of insanity is not mad, are things open, perhaps, to less doubt, but still may be matters of opinion. On all such points, men do, will, and may reasonably differ. But scientific witnesses—and our readers will see that we here use the term "scientific" in the loosest way—are sometimes called upon, and sometimes go out of their way, to speak to things the truth or falsehood of which can be easily demonstrated. That one drop of benzol, four ounces of pure water, and fifteen drops of nitric acid, will produce a Magenta-coloured liquid, is either a fact or exactly the opposite; and any one who will take the trouble to make the experiment may settle which for himself. And whether the presence or absence of phosphates is likely to be proved by sulphate of copper and chloride of lime, is well known to every one who knows anything of chemical analysis. But how can a judge and jury, all profoundly ignorant of chemistry, estimate the value of such assertions? If any barrister had tried to persuade Mr. Baron Channell and a jury that an individual with red hair could not recover on a policy for fire insurance, both judge and jury would have laughed at the absurdity of the notion; and yet the barrister would have advanced nothing more absurd or untrue than some statements which were listened to gravely at Stafford. The fact is, the machinery of ordinary judge and jury for the trial of cases which depend on chemical evidence is simply "a mockery, a delusion, and a snare." A chemist might just as reasonably sit at Westminster to decide points of law. How can people ignorant of chemical science judge between the contradictions of chemists, or estimate at their proper value the evidence of a druggist who "*makes his own tinctures*," *and does not remember the composition of cyanogen, for the compounds of hydrogen are so complicated*, and the evidence of a Faraday or a Miller? It is only a few days since the leading daily Journal remarked that, "one of the most unsatisfactory parts of our law of evidence is that which relates to the admission of the testimony of experts." The law is indeed in an unsatisfactory state, and it cannot too soon be amended. The interests of science, truth, and justice demand that the attempt be made at once; and the attention of the Legislature and Government should be called to these Stafford trials as excellent illustrations of the necessity for a change. If a chemist of acknowledged reputation were in such cases to sit with the judge as assessor, it would probably have the effect of keeping out of the witness-box men who are wholly incompetent to form a correct opinion, although it might not in all cases ensure unanimity in the witnesses.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Equivalent of Lithium, by M. L. TROOST.

THE discovery of two new metals in lithium ores, and the valuable aid afforded by the spectrum, enable us to investigate, with some chance of success, the cause of the differences observed in determining the equivalent of lithium. With this view, I have re-examined the salts of lithium prepared and described by me in the *Annales de Chimie et de Physique*, Third Series, vol. li. By submitting them to spectrum analysis, with the co operation of M. Grandeau, I find that the composition of these salts vary greatly according to the nature of the substances from which they were prepared. The salts, which I obtained by double decomposition, extracted directly from sulphates, contain, besides traces of potash and soda, cæsium and rubidium in considerable and nearly equal quantities. The salts obtained by the chloride, purified by alcohol and ether, also contain these two metals, with traces of sodium. The carbonate of lithia, and the salts derived from it, contain no trace of foreign metals.

I have obtained this body, as I have stated, by treating the chloride with carbonate of ammonia; the precipitate, washed and dried, was held in suspension in water, and a current of carbonic acid passed through it; it dissolved rapidly, and precipitated itself again, in a crystalline state, when boiled. Again dissolved and precipitated, a salt was obtained which, under spectrum analysis, showed no indication of soda nor of the two other metals. The absence of these bodies is easily accounted for by the solubility of their carbonate. Carbonate of lithia, then, must be employed for preparing the compounds to be used in determining the equivalent. In the paper

above cited, as I attributed to this salt a greater degree of purity than to all the others, I have made use of it in my researches, and with it I prepared the chloride which I forwarded to M. Dumas for the determination of the equivalent.

The purity of the chloride used being established, it becomes more than probable that the number found by M. Dumas is the exact equivalent. I have endeavoured to verify this by the use of two methods entirely different.

Chloride of lithium, heated while exposed to the air, decomposes partially, which at first made me avoid using it. I have since, however, proved in the following manner that a very exact result can be arrived at by the use of this body:—Place the chloride in a platinum vessel in the middle of a Bohemian glass tube traversed by a current of dry hydrochloric acid gas. Heat to dryness. When cold, replace the hydrochloric acid gas by a current of dry air. Close the tube containing the vessel with good india-rubber stoppers, and weigh it. A similar glass tube, also stopped with india-rubber, may be used as a counterpoise. The weight being determined, replace the tube under experiment, and heat the platinum vessel for two hours in a current of dry hydrochloric acid gas; finish the operation, and conduct the weighing under the same conditions as before. It will be found that there has been no loss of weight. It is then evident that the chloride does not decompose when placed under the conditions described.

In precipitating by nitrate of silver certain quantities of chloride which, according to M. Dumas, may be used in the ratio of one to two, I obtained,

	Milligrammes.		Milligrammes.
I.	For 1309 chloride of lithium	4420	chloride of silver,
II.	„ 2750 „	„ 9300	„ „

which gives, as the equivalent of lithium,

I. 7·030
II. 6·99 } mean, 7·01.

To test this result by another method, I again took the carbonate of lithia and estimated the lithia in separate portions, by combining it with sulphuric acid and the carbonic acid, by heating the carbonate with an excess of pure powdered quartz. The number, rather too low, though equal to that of Berzelius, which I had previously obtained, might be due to the ready decomposition of carbonate of lithia, under the influence of even moderate heat. To avoid this cause of error, I took care not to heat it above 100°, and in one experiment I even dried the carbonate at the ordinary temperature, *in vacuo*, in presence of sulphuric acid. I have likewise proved, in the following manner, by employing silica, that—

I.	970 mgrs. carbonate	contain	577 mgrs.	carbonic acid,
II.	1782 „	„	1059 „	„ „

which gives as the equivalent,

I. 7·00
II. 7·02 } mean, 7·01.

The estimation of lithia by sulphuric acid gave, for 1217 milligrammes of carbonate, 1808 milligrammes of sulphate; making the number 7·06. The equivalent of lithium is then 7, as M. Dumas concluded from his experiments; and the discovery of the two new metals in the lithium ores do not invalidate his conclusions.

Another confirmation of this equivalent—obtained also by M. Mallet—is derived from the experiments of M. Karl Diehl, tested with the spectrum analysis in Bunsen's laboratory, and described in the *Annales de Wöhler et Liebig*, January, 1862. In fact, M. Karl Diehl, by determining the carbonic acid expelled from the carbonate by diluted sulphuric acid, by four concurring experiments, arrives at the number 7·026.

I have, in the course of my experiments, repeatedly verified the fact, already stated, that anhydrous or hydrated lithia, as well as pure salts of lithia, do not act in platinum. The alteration of this metal, when it ascends, is attributable to compounds of cæsium and rubidium. MM. Bunsen and Kirchhoff have, in fact, proved that this property is possessed by a sub-oxide of these metals.—*Comptes-Rendus.*

Oil of Cajuput as a Means of Distinguishing Between Amber and Copal, by HARRY NAPIER DRAPER, *F.C.S.*

SEVERAL oxygenated volatile oils, notably those of lavender, rosemary, and peppermint, possess the property of softening copal resin in the cold, and of dissolving it at higher temperatures, to a greater or less extent. Oil of cajuput, however, readily dissolves copal at the ordinary temperature, forming a perfectly transparent solution, from which the oil evaporates, leaving a very brilliant coating of the resin. Amber is totally insolubie in oil of cajuput, even at the boiling point of the latter; and, as some of the specimens of copal often simulate and are mistaken for amber, this different behaviour may serve as a ready means of distinguishing between the two resins. I may add, as a fact also possessing some technical interest, that the solution of copal in cajuput oil is easily miscible with alcohol.

Dublin, March 28.

On the Synthesis of Acetylene by the Direct Combination of Carbon with Hydrogen, by M. BERTHELOT.

THE hydrocarbons and alcohols are the starting-points in the formation of other organic compounds; therefore, after having succeeded in effecting the synthesis of the alcohols and their ethers from hydrocarbons, I directed all my efforts towards the formation of the hydrocarbons themselves from their elements. I have already given different methods by which this end may be attained, and the most simple carburets obtained, starting from carbon and hydrogen. But if these methods leave no doubt as to the final result, they are nevertheless indirect, and only yield circuitous ways of realising the initial combination of carbon with hydrogen. In the present state of our knowledge, there was scarcely any hope of proceeding otherwise. We all know, in fact, what is the chemical indifference of carbon at the ordinary temperature even to the most powerful reagents. This indifference only ceases at a red heat, and then only for oxygen and sulphur. But as to hydrogen, all its combinations with carbon hitherto obtained from organic products are actually destroyed under the influence of a red heat; it would therefore seem chimerical to attempt their direct formation.

My last researches on acetylene appeared, however, to authorise new attempts. This compound is the least rich in hydrogen of all the carburetted gases, for it is the only one which contains its own volume without condensation:—

$$C_4H_2 = 4 \text{ volumes} : H_2 = 4 \text{ volumes.}$$

Acetylene is at the same time the most stable of hydrocarbons. Not only is it formed in large quantity at the expense of olefiant or marsh gas under the influence of heat or the induction spark, but it may be produced by the action of the latter, although in small proportion,

at the expense of even benzol and naphthalin—that is to say, at the expense of carburets which we have hitherto been accustomed to regard as the most stable of all. In the presence of these facts, I thought that there was still room for the attempt to form acetylene by the direct union of its elements.

But before undertaking my experiments, it was necessary to ensure the purity of the materials with which I had to work.

Hydrogen is easy to prepare by means of zinc in a state of purity and dryness, but with carbon it is otherwise. Carbon is usually obtained from organic substances; there are, therefore, different kinds of carbon, containing variable quantities of hydrogen. A sustained calcination removes the greater part; but the best calcined carbon—retort carbon, for instance—in spite of its semi-metallic properties, still retains some trace. This last carbon contains, besides, a small quantity of tarry matter, the unsuspected presence of which might give rise to serious errors. To completely and certainly eliminate the hydrogen and tarry matter contained in the carbon, I know of only one process—the employment of chlorine at a red heat. Chlorine, moreover, presents this advantage in purifying carbon, that it separates sulphur, iron, aluminium, silicium, and most of the metals in the form of volatile chlorides. It has thus been employed by M. Dumas in his researches on the equivalent of carbon. If I insist upon these precautions, it is because their omission would remove all the demonstrative character from the results which I am about to give, by leaving it uncertain whether the formation of acetylene should be attributed to the direct union of carbon with hydrogen, or to the decomposition of some hydrogenised matter contained in the carbon.

I therefore employed retort carbon heated to redness, for some time in contact with air, and then heated to redness for an hour and a half in a current of chlorine.

At first I had recourse to the action of heat alone: I heated the purified carbon to a bright red heat in a current of hydrogen, but without success. Wishing to increase the temperature still higher, I had recourse to the kindness of M. H. St. Claire Deville, who placed at my disposal his apparatus at the Ecole Normale, and his great experience with heat. But I had no more success than I had had before; after having kept the temperature at a white heat for more than an hour, we saw the porcelain tube containing the carbon fuse as if it were glass, without obtaining the least trace of acetylene.

To carry the experiment further, electricity remained with its powerful effects, or the proper influence of this agent, together with that of heat. I employed, first, the induction spark in contact with calcined carbon, or in contact with finely divided carbon, produced in the apparatus itself by the decomposition of marsh gas; but the experiment still failed, which I attribute to the carbon not being heated by the induction spark.

Finally, I had recourse to the battery, and the electric arc produced between two carbon poles, with the excessive elevation of temperature and the transport of carbon from one pole to another. I took care to purify the carbon poles from all tarry and hydrogenous matter, by the employment of chlorine, as explained above.*

* Desiring to control my results from this point of view, I took a fragment of carbon, purified for my experiments, weighing 1·078 grammes, and without pulverising, or even breaking it, I burned it in a current of oxygen. I obtained 0·010 gramme of water; that is to say, 1 milligramme of hydrogen. This probably owed its origin to hygrometric water.

Under these new conditions, the experiment succeeded fully. The combination of hydrogen with carbon took place instantly, as soon as the arc shone. Acetylene was produced, and that is the only product which I recognised in the reaction; its production continues as long as the electric arc passes; it may be produced indefinitely with the same carbons, as long as the transport of matter which takes place between the poles has not entirely disentegrated them.

$$C_4 + H_2 = C_4H_2$$

Carbon. Hydrogen. Acetylene.

I had the honour to perform this experiment before the Academy. The acetylene formed round the poles is carried away by the current of gas, and condensed in a solution of ammoniacal protochloride of copper, producing a red precipitate of cupreous acetylide. The experiment is equally striking, on account of the employment of the electric light and the characteristic formation of the precipitate. It is so easy of performance, that it may be readily reproduced as a lecture experiment.

Nothing is easier than to obtain in this way notable quantities of acetylide of copper. In the conditions under which I operated there were formed about ten cubic centimètres of acetylene per minute; the proportion of carbon entering into combination with the hydrogen may be estimated at about half that which is disengaged and carried from one pole to the other.

Upon subsequently treating the acetylide of copper with hydrochloric acid, pure acetylene is reproduced. After having satisfied myself that the carbide obtained by this means possessed all the characteristic properties of acetylene, I made an analysis of it:—

20 volumes of the carbide obtained from its elements, being burned in the endiometer, yielded
40 volumes of carbonic acid, absorbing
51 volumes of hydrogen.
20 volumes of acetylene ought to produce
40 volumes of carbonic acid, absorbing
50 volumes of oxygen.

Acetylene thus formed by the direct synthesis from its elements, is not an isolated fact, but a point of departure. I have, indeed, shown how it may easily be changed into olefiant gas by the simple addition of hydrogen:

$$C_4H_2 + H_2 = C_4H_4$$

Acetylene. Hydrogen. Olefiant gas.

From olefiant gas may be formed alcohol; and we thus enter into the chain of bodies whose *ensemble* constitutes organic chemistry. To all these syntheses and progressive formations, that of acetylene will henceforth allow the starting-point to be a direct synthesis.—*Comptes-Rendus*, liv. 640.

On the Estimation of Selenium, by M. H. ROSE.

SULPHUROUS acid is the best reagent to precipitate selenium when this element is in solution in the state of selenious acid; it is necessary that the precipitation be performed in the presence of hydrochloric acid. The sulphurous acid may also be replaced by phosphorous acid; likewise in the presence of hydrochloric acid; but the reduction takes place much slower than when sulphurous acid is used.

Mr. Oppenheim has shown that selenium may be estimated by fusing the body containing it with cyanide of potassium, dissolving the fused mass in water, and then super-saturating the solution with hydrochloric acid

which effects the complete precipitation of the selenium at the end of a few hours. The fusion ought to be performed in an atmosphere of hydrogen; it is advisable, when the substance contains free selenious acid, to previously saturate this acid with an alkaline carbonate, so as to avoid volatilising small portions before the cyanide of potassium has had time to react upon it.

The solution obtained by treating the fused mass with water contains seleno-cyanide of potassium, together with a small quantity of selenide; it is necessary, on this account, to boil the liquid for some time before the addition of hydrochloric acid, to convert the selenide into seleno-cyanide. Without this precaution, a portion of the selenium might be disengaged in the form of seleniuretted hydrogen.

When the selenium acids are fused with alkaline carbonates, in an atmosphere of hydrogen, they are reduced to alkaline selinides; from a solution of these latter a slow current of atmospheric air entirely precipitates the selenium. This process may serve for estimating selenium, but it is less accurate than the preceding.

Sulphuretted hydrogen completely precipitates selenious acid from its solutions in the form of sulphide of selenium SeS_2, from the weight of which the selenium may be determined. Selenious acid may be estimated in its aqueous solution, or, in the presence of nitric and hydrochloric acid, by simple evaporation, taking care not to exceed a temperature of 100° C., above which a portion of the acid may volatilise.

The ordinary process for the preparation of selenic acid, which consists, as is known, in precipitating this acid as a baryta salt, is far from deserving the confidence with which it is usually regarded. On the one hand, seleniate of baryta is much more soluble than the sulphate; on the other hand, it possesses, in a much greater degree than this latter salt, the property of carrying down with it considerable quantities of the soluble salts which are contained in the liquid. It is better to reduce the selenic acid into selenious acid with hydrochloric acid, and then to precipitate the selenium with sulphurous acid.

To separate selenium from tellurium, the substance is fused with ten times its weight of cyanide of potassium in an atmosphere of hydrogen, in which the fused mass is allowed to cool. This is dissolved in abundance of water, and a current of atmospheric air passed through the solution. At the expiration of twelve hours, the deposited tellurium is separated by filtration; the selenium is estimated in the liquid in the way described above.

The separation of selenium from sulphur may be effected with cyanide of potassium, either by simple ebullition or by fusion; the first process is only applicable to the non-oxygenised compounds of selenium and sulphur. The sulphur and selenium dissolve in the form of sulpho- and seleno-cyanides; the first sometimes with great difficulty, and the selenium is precipitated from the liquid with hydrochloric acid. In the second case, when the substance is fused with cyanide of potassium, the mass is dissolved in water, and the liquid boiled; selenium is precipitated with hydrochloric acid, and the sulpho-cyanide in the solution is transformed into sulphate, which is precipitated by a baryta salt.

To separate selenium from sulphur and tellurium, the best process consists in fusing the substance with cyanide of potassium in an atmosphere of hydrogen; to dissolve the mass in water, precipitate the tellurium with a current of air, to boil the filtered liquid; then precipitate the selenium with hydrochloric acid; and afterwards estimate the sulphur by changing it into sulphuric acid by the action of chlorine, after super-saturating with potash.

Selenium cannot be separated from the metals with which it is combined when the sulphides of these metals are insoluble in the sulphide of ammonium, by making use of the solubility of selenium in this reagent. The insoluble metallic sulphide is almost always mixed with selenide. Most frequently, selenium may be separated from metals by heating the mixture in a current of chlorine; the chlorides of selenium are sufficiently volatile to render the separation generally easy. In acid solutions of the selinites of metals not precipitable by sulphuretted hydrogen, the selenium may be precipitated by this gas in the state of sulphide of selenium.

To estimate the alkalies and alkaline earths combined with selenium acids, it is sufficient to fuse them with chloride of ammonium. The alkali or alkaline earth remains in the state of chloride. One single fusion, or two at the most, are sufficient to drive off all the selenium.

When it is desired to estimate the selenic acid in an insoluble combination, particularly in seleniate of baryta, this combination is decomposed by an alkaline carbonate; the transformation into alkaline seleniate takes place even in the cold, and it is then easy to reduce the selenic into selenious acid by means of hydrochloric acid.—*Poggendorff's Annalen der Physik und Chemie.*, cxiii. 472 and 624.

TECHNICAL CHEMISTRY.

Historical and Scientific Facts about Petroleum.

Within the last three years there has sprung up an important and extensive branch of industry,—the refining of petroleum, or, as it is sometimes called, mineral oil. This is already a staple article, and its use as an illuminator is becoming every day more extended. When properly manufactured it is not explosive; it affords a brilliant flame; it can be furnished at a moderate price; and, moreover, its sources of supply are abundant. The subject is one of so much general interest that we are induced to publish the following interesting article concerning this substance, which was communicated to the *Scientific American* by a member of the Chemical Society of Schenectady, N. Y.:—

Petroleum is not of constant composition, but is a variable mixture of numerous liquid hydro-carbons, as benzol, naphtha, keresolene, &c., with paraffin, naphthalin, and asphaltum, solid hydro-carbons. It is of a very dark green colour, and in density varies from a thin fluid, lighter than water, to a thick viscous liquid, heavier than water. The lighter qualities yield the larger proportion of burning oil.

The evidence of the most ancient occurrence of petroleum is among the ruins of Nineveh, whose existence dates back more than 2000 years before the Christian era. In the construction of this city, an asphaltic mortar was extensively employed, the asphaltum being obtained by the evaporation of petroleum.

A later mention is found in the accounts of Babylon, whose walls were cemented with asphaltum, which was poured, in a melted state, between the blocks of stone, and an indestructible mortar thus secured. This asphaltum was procured from the fountains of Is, which were about 120 miles above Babylon, on the Euphrates. Together with saline and sulphurous water, it issued

from a rock and was conducted into large pits. The oily matter was then skimmed off and solidified by atmospheric evaporation. These springs, from the abundance of their products, attracted the attention of Alexander, Trajan, and Julian, and even at the present time asphaltum procured from them is sold in the neighbouring village of Hits.

From time immemorial asphaltum has been found on the shores of the Dead Sea, and this is one of the most remarkable localities for it. This sea, as is well known, is of supposed volcanic origin; and is the probable site of the ancient cities of Sodom and Gomorrah. Its surface is 1300 feet below the surface of the ocean, and it has been fathomed to the depth of 2000 feet. In several places no bottom has been reached, and, owing to internal convulsions, the depth changes from time to time. The water is very dense, holding in solution 25 per cent. of solid matter, of which 7 per cent. is salt. The bituminous substance is up-thrown from below, and towards the centre of the sea it is found in a liquid state, like petroleum; but it is probably solidified by evaporation, as it appears upon the shores in hard, compact masses. The explanation of this phenomenon is, that a connection between the sea and some internal volcano exists, whence this substance is ejected.

In the vicinity of the Caspian the Bakoo springs have yielded large quantities of oil, and are widely celebrated. Some of the Persian wells have furnished 1500 barrels a-day, and throughout this region this material, under the name of naphtha, is very generally burnt for its light.

At Rangoon, in Burmah, petroleum has been obtained for many years, and at this time there are over 500 wells, which annually afford 400,000 hogsheads. The oil occurs in a strata of blue clay; wells about sixty feet deep are dug, into which the petroleum oozes. This is sometimes used in its natural state, but more frequently it is first purified by distillation with steam. The raw material is also mixed with earth and used as fuel.

In Europe there are few abundant springs. On one of the Ionian Islands there is an oil fountain, which has flowed for over 2000 years; and the oracular fires of ancient Greece have been attributed to similar sources. Oil springs also occur in Bavaria, in the Grand Duchy of Modena, at Neufchatel, at Clermont and Gabien in France, and near Amiano in Italy. Petroleum procured from the last-named locality is used for lighting the city of Genoa, but elsewhere in Europe it is not employed to any extent as an illuminator.

On this side of the ocean there is an enormous quantity of this substance. Upon the Island of Trinidad, one of the West Indies, at a distance of three-fourths of a mile from the sea, is a lake of asphaltum three miles in circumference. Near the banks the asphaltum is hard and cold, but as you approach the centre the softness and the temperature increases, until finally it is liquid and boiling. From the bubbling mass proceeds a strong, sulphurous odour, which is perceptible at a distance of ten miles. Between the banks of the lake and the shore of the island is an elevated tract of land, covered with hardened asphaltum, upon which vegetation flourishes. The explanation put forward in connection with the Dead Sea is equally applicable in this case.

Upon others of the West Indies petroleum has been obtained, as well as at several places in Central and South America; but it is in the northern portion of this continent that the abundant reservoirs of this substance are located; and it seems truly wonderful that their extent and richness should not have been discovered at an earlier period. For many years the Seneca Indians collected petroleum, and, under the name of Seneca oil, sold it as a remedy for rheumatic complaints. At numerous places in the Middle States it was found in salt borings, and was collected and burnt by the farmers, but it was not till August, 1859, that it was obtained in noticeable quantities. At this time oil was "struck" upon Oil Creek, Venango County, Pennsylvania, by sinking an Artesian well to the depth of seventy feet, and for many weeks a thousand gallons a-day were pumped from it. The news of this discovery spread far and wide, and gave rise to an "oil fever." Thousands flocked to this vicinity in the hope of making their fortunes. Before the close of 1860 there had been over a thousand wells bored, many of which were productive, but a large proportion returned nothing. Some of the adventurers have been very successful, and have made large amounts of money; but, as in all commercial "fevers," a large number of persons have been utterly impoverished by their speculations. The mere sinking a well by no means insures a bountiful flow of oil. The petroleum is stored in fissures formed by the upheaving of the earth's crust by volcanic action; and these fissures are perpendicular rather than horizontal in tendency, as is proved by the fact that at wells but a few yards apart the oil is "struck" at very different depths. The lowest parts of the fissures contain water, above which is the oil, while in the highest portion there is a quantity of gas. If, therefore, the well strikes the fissure at the lowest part, the water will be forced up by the pressure of the supernatant oil and gas. Persons ignorant of the formation sink a well at random, and perhaps strike a fissure; but, obtaining nothing but water, they abandon the spot as worthless, whereas, after removing the water by pumping, a large quantity of oil might be obtained.

In some localities in Ohio, as is the case in Burmah, the ground is saturated with the oil, and wells several feet in diameter are dug, into which the oil oozes. Porous limestone, containing petroleum, is found in some sections of the West, and has been subjected to distillation with profitable results.

In regard to the origin of petroleum, scientific authorities differ; but the theory most generally favoured is, that it is the product of the slow distillation, at low temperatures, of organic matter in the interior of the earth, the vapours being condensed in the previously-mentioned fissures and the surrounding soil. The Lake of Trinidad, and the bituminous matter of the Dead Sea, may also be referred to a similar source. But for how many centuries must this operation have been going on to have effected such enormous results?

Of the many uses to which petroleum and its derivatives are applied, that of illumination is the most important; and the process of refining is exceedingly simple. The crude material is put into a large iron retort, connected with a coil of iron pipes, surrounded by cold water, called the condenser. Heat is applied to the retort, and from the open extremity of the condenser a light-coloured liquid, of a strong odour, soon flows, This is naphtha, and is very volatile and very explosive. Some refiners mix it with the burning oil, and numerous accidents have resulted from such mercenary indiscretion. It is usually run into a separate tank. After the naphtha has passed over, the oil used for illumination distils off. Steam is now forced into the retort and the heavy lubricating oil driven over. There now remains a black, oily, tarry matter, sometimes used to grease heavy machinery, and a black coke, employed as fuel. There are, therefore, five substances separated in

this operation, but only the first three are of any economic importance.

The naphtha is used as a substitute for turpentine in paints, or, by repeated distillations, the benzole is separated from it, and employed to remove spots from fabrics. This, however, is rather a drug in the hands of the refiner.

The burning oil, as it comes from the retort, is of a yellow colour, and, in order to remove this, it is placed in a large, lead-lined cistern, and agitated with about ten per cent. of sulphuric acid. After the acid and impurities have subsided, the oil is drawn off into another tank, and agitated with four per cent. of soda lye. This last operation is to remove any acid remaining with the oil, and also to extract the residue of the colouring matter. In fact, it is sometimes employed alone, and a very good oil obtained. The oil is now agitated with water to remove the soda lye, and is then ready for consumption. The colourless oil is by no means the most economical, but, on the contrary, more light is obtained from the yellow article.

The heavy oil is cooled down to 30° F., when the paraffin crystallises out, and is separated from the oil by pressing. It is further purified by another pressing and by alternate agitation, in a melted state, with sulphuric acid and soda lye. It is then moulded into candles. It is a curious fact, that the composition of paraffin and good coal gas is exactly the same.

In Egypt, a substance derived from petroleum was used in embalming bodies; and in Persia and the neighbouring countries, asphaltum is used to cover the roofs of the houses and to coat the boats. In France, asphaltic pavements have been successful in several cities, and for the protection of stone no material is better adapted. Mixed with grease, the Trinidad asphaltum is applied to the sides of vessels to prevent the boring of the teredo, and with quicklime it affords an excellent disinfectant. Among the products of the distillation of petroleum are naphthalin and keresolene. The former is the substance from which is obtained aniline,—the base of the beautiful colours, Mauve, Magenta, and Solferino. The latter has been proposed as a substitute for chloroform and ether. Many other substances have been separated, but as yet none of them have been applied.

On the Corrosion of Zinc-covered or "Galvanized" Pipes when used for Conducting Water, by A. A. Hayes, *M.D.*

Iron pipes covered with a firmly-adhering surface of zinc more or less pure, have been used as conduit pipes, under the received supposition that the zinc, by its polarising action from contact, will preserve the iron from corrosion, in the act of itself suffering oxidation. As the oxide of zinc, formed under some circumstances, adheres to the metal and encrusts it with a body not soluble in water, it has been assumed that water, passing through such pipes, would not become contaminated by either iron or zinc oxide.

Some months since, I analysed some well-water, which had produced a white deposit in the culinary vessels in which it had been boiled, and was itself turbid. The deposit proved to be oxides of zinc and iron with organic matter, and the water held suspended and dissolved organic salts of both these metals. On learning the fact that the pipes had not been long in use, a request was made that suitable precautions should be taken to avoid using the water in preparing food; and by ensuring a large flow of water through the pipes continued for several weeks, the possible formation of a protecting surface was expected. But after long exposure in this way to much water drawn from the well, analyses of the water in the pipe and that in the well did not indicate any diminished action on both the metals. The zinc exposed to this water not only dissolved in it, but lost its usually observed power of protecting the less oxidisable metal in contact with it, and the quantity of salts formed from both metals was so large as to render it unfit for general domestic use.

Some weeks later, I received a sample of water from a more distant town, the purity of which was suspected, and this was found to contain organic salts of zinc and iron also, although colourless and transparent. In this case the galvanised pipe had been longer exposed, and symptoms of anomalous disease in the family consuming the water, led to the chemical trials. The acid present in both salts appeared to be the crenic, and, in one case, traces of ammonia were found, constituting a compound salt. When the water was boiled, especially in metallic vessels, a white deposit of oxides of zinc and iron, with altered organic matter, appeared, but long-continued ebullition was required to ensure complete decomposition of the salts.

The observed loss of protection in this exposure was deemed a point of much interest, for I had repeatedly examined iron boilers protected from corrosion by zinc surfaces, and have recommended this resort in numerous cases within the last thirty years, under varied circumstances, where the protection seemed to be nearly complete.

Mentioning these facts to my friend, Dr. Samuel L. Dana, of Lowell, he informed me that zinc surfaces failed to protect iron surfaces exposed to the flowing water of the Merrimac River, and showed me the result of such trials; the iron being much corroded both near by, and remote from the protecting metal.

As the kinds of well-water which acted on the zinc and iron in these cases are quite common in every part of New England, it seems doubtful, in a sanitary point of view, if such pipes are proper for conducting water generally; for, even when care is exercised, the metals dissolved in the water will surely be found in the food partaken of by families thus supplied.

I am aware that many persons consider both zinc and iron compounds, when taken into the system, as not actively poisonous, if even harmful; compounds of iron, especially, being found in the system. The chemical fact of the most importance in this connection is, that the compounds of iron naturally found in the system are derived from compounds of iron existing in the food by the simplest transformation, and that other forms of combination will not supply these, and are active extraneous bodies, which leave their marks on the stomach tissues.

In illustration of the activity of an iron salt when the dose is very minute, the effects of chalybeate waters may be instanced, and there are few medical men who have not witnessed the most surprising changes in the system induced by these, even when the ordinary preparations of iron have failed in their action. Now, in most of the ferruginous waters, it is the crenate of the protoxide of iron which occurs—the same salt which the galvanised pipes produce,—while the zinc is not found as the well-known oxide, but in the state of an active salt corresponding to the iron compound, and has no claim to consideration as a body forming healthy secretions.—*Boston Medical and Surgical Journal.*

PHARMACY, TOXICOLOGY, &c.

On the Behaviour of Essential Oils to Iodine and Bromine, by John M. Maisch.

(Continued from page 149.)

Oleum Menthæ piperitæ Germ.—Rectified, nearly colourless, thin.

Iodine, aided by stirring, dissolves, without any brisk reaction, to a reddish brown liquid of the consistence of honey; the residue of heavier oils is thicker, but still fluid; oils distilled from old herbs evolve a few vapours. Z.

It has a quiet and slow reaction, and dissolves to a brownish liquid, with which the black iodine sediment mixes to a syrupy liquid.

Ether sol. iodine is spreading, miscible to an iodine coloured liquid, which changes to a thick yellowish, blackish brown; after twelve hours it has a deep greenish brown colour, and the consistency of butter.

Ether sol. bromine.—The mixture has a beautiful rose colour, which gradually darkens, but retains in thin layers a pinky shade.

Oleum Menthæ viridis.—Yellowish, red, old, rather thick.

Iodine dissolves slowly to an evenly thick iodine coloured liquid.

Ether sol. iodine shows little spreading, and mixes to an iodine coloured solution; in six hours it is a thick, chesnut brown liquid, and after twelve hours more it has a butyraceous consistence.

Ether sol. bromine.—A few white fumes are evolved; the yellowish red solution passes through various shades to a deep yellowish brown; the sediment is of a lighter colour. (Separation of water?)

Oleum Monardæ.—The crude oil, light brown, one year old.

Iodine dissolves quietly; during the first few moments a nearly vermilion red colour is produced, which soon darkens to a brownish red; the nearly black sediment is of a pitchy consistence.

Ether sol. iodine is miscible to a clear iodine-coloured solution.

Ether sol. bromine mixes to a brownish violet colour, which subsequently deepens to a dark purplish brown.

Oleum Myristicæ.—Colourless, thin.

Iodine.—Violent reaction with explosion, and with white and some purple vapours; the residue consists of a light blackish green solution, with a deep greenish brown sediment, which are miscible to a transparent liquid, assuming a more yellowish shade, and is subsequently light greenish yellow.

Ether sol. iodine.—The mixture has instantly a turbid yolk-yellow colour, and is afterwards transparent and colourless, regains a yellowish brown tint, and is rendered colourless again.

Ether sol. bromine.—At first colourless; the heavier part assumes a light brownish milky appearance, the brown shade afterwards disappears; the upper stratum remains transparent, with scarcely a brownish tint.

Oleum Patchouly.

Of this oil, which is extensively used in perfumery, I had three different kinds at my disposal, of which at least two were of lots imported at different times; they were all of a brown colour, and about the consistency of olive-oil; one approached more the consistence of castor-oil; their very characteristic behaviour was alike.

Iodine dissolves with radiating motion, without vapours, to an iodine-coloured solution, which changes to brown black, afterwards to bluish green black; the liquid is not thickened.

Ether sol. iodine dissolves, without spreading, to a homogeneous dark green liquid, which is nearly black in appearance; after twelve hours no change had taken place, except the formation of a black precipitate.

Ether sol. bromine mixes with the evolution of a few white vapours, to a solution of a splendid deep violet colour; it deepens in its shade so as to appear almost black; upon the sides of the vessel, when some spreading had taken place, indigo blue stripes, and vermilion red spots were observed, the colour of which darkened to greenish black and yellowish green black; the oil was not thickened; the sediment had the colour of burned umber.

Oleum Patchouly two parts, oleum Limonis one part.—No difference from the reaction of the pure oil could be observed in the behaviour of this mixture to the ethereal bromine solution, except that upon the sides of the dish a brownish green margin was visible.

Oleum Rhodii ligni.—Yellowish, slightly viscid.

Iodine.—Scarcely any action; but, on stirring, the oil assumed a greenish hue, and gradually dissolves iodine, forming a deep greenish brown liquid.

Ether sol. iodine.—Miscible with very little spreading, and scarcely any undissolved particles; the liquid has a peculiar yellowish brown iodine colour; gradually it separates, the upper stratum being of a greenish yellow, the lower one of a greenish brown. In six hours it is again of a uniform thin consistence, and of a greenish yellow brown colour.

Ether sol. bromine at first collects on one side of the dish, gradually sinks below the oil, and the reaction now shows a pure yolk colour; then brownish stripes appear on the surface of the lower stratum, which increase in size until the colour is entirely changed to umber brown; the supernatant liquid shows scarcely a brownish tint; after stirring the two together, they separate again.

Immediately after the above reactions, the odour approaches nearer the perfume of roses than in the original oil.

Oleum Rosmarini.—Yellowish, almost colourless, thin.

Iodine.—Radiating motion of the iodine solution, with few red vapours, and some heat; the yellow brown residue, of the consistence of a soft extract, has its odour unaltered. Another sample showed a curdy precipitate in a yellowish brown liquid, which could not be mixed to a uniform mass. Z.

With a brisk radiating motion, and the evolution of white vapours, a solution is obtained of a brown colour, tinged with yellowish green; the dark greenish brown sediment is miscible by agitation.

Ether sol. iodine.—Two strata are formed, the lower one of which has a deep iodine colour, while the upper one is much lighter coloured.

Ether sol. bromine yields a colourless mixture, which separates into a colourless, clear upper stratum, while the heavier oil is milky white; the latter then changes to a light brown, and the former is rendered white and cloudy.

Oleum Rutæ.—Yellow, with a bright brownish tint scarcely thickish.

Iodine.—The oil prepared by Zeller dissolved the iodine slowly without heat, vapours, or motion, to a yellow brown liquid; with the commercial oil, little

heat, a few vapours, and some radiating movement were observed. *Z.*

It dissolves with some radiating motion to a yellowish iodine coloured liquid.

Ether sol. iodine.—On dropping it into the oil, some spreading occurs, the iodine sinks to the bottom, the supernatant liquid is yellow; without stirring, the two combine to a homogeneous solution of iodine colour.

Ether sol. bromine.—The mixture is at first slightly cloudy, but has a pale brownish yellow shade afterwards.

Oleum Sassafras.

I have examined two oils, one anhydrous and colourless, the other one being yellowish brown, and containing water, both perfectly transparent; they were the upper and lowest stratum of an oil saturated with water, which had stood undisturbed for about a month. (See Proceedings of Amer. Pharm. Assoc., 1858, p. 355.)

Iodine dissolves, without heat or vapours, to a clear, uniform, yellow-brown liquid, without thickening appreciably. *Z.*

It dissolves entirely without vapours or motion; the solution has an iodine colour; the odour is unaltered.

The yellow oil has the same behaviour, only effects the solution slower.

Ether sol. iodine mixes, with spreading, to an iodine coloured liquid, which after five hours is limpid and of a rose colour.

The yellow oil yields a darker solution, spreads more than the former, and after five hours had changed to a somewhat thicker, blackish brown oil.

Bromine.—Hissing, some heat, and white vapours; the oil is milky, and around the bromine shows a yellow and green colour; odour somewhat resinous. The dark oil was not tested.

Ether sol. bromine mixes easily, with some heat and vapours; the mixture is milky, with yellowish spots at the bottom.

The dark oil shows the same reaction; the liquid is milky, gradually turns green; the sediment yellowish, then green, and getting darker; the margin is now blue, then violet, and subsequently rose colour.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

SOCIETY OF ARTS.

Wednesday, February 5.

Dr. A. W. Miller, *F.R.S., Professor of Chemistry at King's College, in the Chair.*

Continued from page 175.)

"Mr. Charles Lauth also published on December 24, 1860, an ingenious method of obtaining purple aniline, which I shall describe when treating of blue colours obtained from aniline.

"*Red Dyes obtained from Aniline,* called fuchsine, azaléine, roseine, &c.—The production of the fine colour, which bears the popular name of Magenta, was first observed by Mr. Natanson in 1856, and more especially by Dr. Hofmann when preparing cyantriphenyl-diamine, by the action of bichloride of carbon on aniline. But it was M. Verguin who first brought it forward to the trade as a dyeing agent, and his mode of preparation, which was patented in April, 1859, by Messrs. Renard and Franc, of Lyons, is the following:—Into a glazed iron pan are introduced 100 parts of aniline and 60 parts of anhydrous bichloride of tin, and the whole is heated for 15 or 20 minutes, at a temperature of about 392°. The dark red liquor thus produced is left to cool, when it becomes thick and glutinous; it is then mixed with boiling water and filtered; to the filtrate is added chloride of sodium, which determines the précipitation of fuchsine, as it is insoluble in saline solutions. Magenta was afterwards prepared by C. Greville Williams, with permanganate of potash, and by Dr. D. Price, with binoxide of lead; nitric acid, and nitrate of mercury were also successfully employed. These different methods of preparing Magenta were followed by several other patents, purporting to obtain the same results, and amongst them I may cite that taken on December 10, 1859, by Mr. Rudolph Heilman, in which the employment of arsenic acid is mentioned, and one also for the employment of the same agent on the 18th of January, 1860, by Dr. H. Medlock. As it is probable that this agent is the best suited for producing Magenta, commercially, I will give a sketch of the process. Dr. Medlock heats two parts of aniline with one of arsenic acid to about 250°, and when the red colour is produced it is mixed with boiling water and allowed to cool. The red colour is thrown down by saline matter, washed, and dissolved in methylated alcohol, or the mass is digested in hydrochloric acid diluted with water, and the clear fluid solution is saturated with an excess of soda which precipitates the colour, while the arsenious acid is held in solution by the alkali. Magenta is a rather powerful organic base which is sparingly soluble in water, but its solubility is increased by the presence of an acid. It leaves a brittle mass, having a beautiful golden green metallic reflection when its alcoholic solution is left to spontaneous evaporation, and this is not peculiar to Magenta, as the whole of the coal tar colours, when in a high state of purity, present the same appearance. Purple aniline differs from the red, not only in its composition, which is as follows:—

$$\overset{\text{Purple.}}{C_{36}\,H_{17}\,N_3\,O_2} \qquad \overset{\text{Red.}}{C_{36}\,H_{20}\,N_4\,O_4}$$

but also because the fuchsine dissolves in ammonia and in sulphuric acid with a yellow colour, and is discoloured by sulphurous acid, whilst the purple is unaffected by those reagents. Silk or wool is dyed with fuchsine by simply adding some of the colour to a slightly acidulated bath. The dyeing colour of this material is so great that 10 grains will dye 2 square yards of silk.

"Of late years many attempts have been made to fix another colour obtained from coal tar, called rosalic acid ($C_{12}\,H_6\,O_3$), but up to the present time I believe all attempts have failed, with the exception of rosalate of magnesia, which was employed for some time in calico printing.

"*Blue Colouring Matters from Coal Tar.*—I have already drawn your attention to the blue colouring matter patented by Mr. Girard, and carried out practically by Messrs. Renard and Franc, of Lyons. Mr. Lauth also has observed that if an alcoholic solution of red aniline, and especially azaléine, is heated with a reducing agent, such as protochloride of tin, or still better with aldehyde or hydruret of benzoïle, a blue colour is produced even at ordinary temperatures. This blue colour is soluble in water, alcohol, and acetic acid, but does not resist the action of mineral acids, alkalies, or light.

"Mr. Willm has recently published an interesting paper on this aniline blue, which not only shows how aldehyde acts, but exhibits the composition of the blue itself.

$$\underset{\text{Azaléine}}{2\,(C_{36}\,H_{20}\,N_4\,O_4)} + \underset{\text{Aldehyde}}{5\,C_4\,H_4\,O_4} + \underset{\text{Water.}}{8\,HO}$$

$$= \underset{\text{Blue or Oxyphenylanilide}}{3\,(C_{34}\,H_{11}\,NO_3)} + \underset{\text{Acetate of Ammonia.}}{5\,C_4\,H_3\,(N\,H_4)\,O_4}.$$

"Therefore the triamine azaléine has been transformed into a monamine blue, by a new chemical reaction, for aldehyde not only acts as a reducing agent, but converts a part of the nitrogen into ammonia.

"*Bleu de Paris.*—Recently, Messrs. Persoz de Luynes, and

Salvétat called public attention to a new blue which they had produced, and to which they gave the name of Bleu de Paris; this they prepared by heating for thirty hours, in a sealed tube, at a temperature of 356°, one part of anhydrous bichloride of mercury with two parts of aniline. The blue thus produced can resist the action of weak acids and alkalies, but assumes a red hue when acted on by these agents in a concentrated state. Sulphurous acid has no action upon it, and it dyes animal fibres with facility.

"*Bleu de Mulhouse.*— MM. Gros-Renaud and Schaeffer have lately published an interesting process for obtaining from the red aniline, called azaléine, a purple and a blue. It consists in dissolving in a litre of boiling water, 50 grammes of white gum lac in powder, and 18 grammes of carbonate of soda, to which is added 50 grammes of an alcoholic solution of azaléine. After an hour's ebullition, the red colour is transformed into the Bleu de Mulhouse.

"*Azuline.*—This beautiful blue colour, which resists the action of the strongest acids, and which was introduced into this country at the latter end of 1860, by Messrs. Guinon, Marnas, and Bonnet, of Lyons, is prepared by them from phenic acid, and, when pure, presents itself under the form of copper-bronze coloured crystals, soluble in alcohol, to which they communicate a magnificent blue colour, slightly tinged with red. The following is the process for dyeing silk and wool:—To an acidulated lukewarm bath of water an alcoholic solution of azuline is added, and the silk or wool worked in it until it is of the required shade. It is then transferred to another bath of boiling water, strongly acidulated with sulphuric acid, when the purple colour is dissolved, leaving a most brilliant and permanent blue upon the material. The dyed silk or wool is washed repeatedly, passed through a bath containing a little tartaric acid, and dried.

"*Chinoline Blue.*—Mr. C. Greville Williams introduced in the spring of last year a fine blue colour, which he obtained by boiling together a substance derived from quinine or cinchonine, called chinoline, with iodide of amyl. The resulting product is boiled with water and then with potash for a quarter of an hour, filtered to separate the resinous matter, when a gorgeous blue is obtained, with scarcely any shade of red. This colour is so fugitive that its use has ceased.

"*Green Colours from Aniline.*—Although it has been known to chemists that aniline would yield a green colour under certain oxidising agents, up to the present time all efforts to dye silk or wool commercially with it have failed, but to avoid having to refer to this green colour again I may mention that Messrs. Samuel Cliff, Charles Lowe, and myself, patented on June 11, 1860, a most easy and practical method of producing it under the name of *Emeraldine*, on cotton fabrics, specimens of which I have the honour to show you. The process consists in printing an acid chloride of aniline on a cotton fabric prepared with chlorate of potash, and in a few hours a beautiful bright green gradually appears, which only requires to be washed. If the green fabric is passed through a solution of bichromate of potash, this colour is transformed into a dark indigo blue, called by us *azurine*.

"*Naphthaline Colours.*—The beautiful solid hydro-carbon naphthaline, which has yielded such a long category of substances to the chemist, has up to the present time yielded nothing of practical importance to the dyer, with the exception of a case which I shall mention presently. From it the following coloured derivatives have been obtained, namely, chloroxynaphthalic acid, perchloroxynaphthalic acid, carminaphtha, ninaphthalamine, nitrosonaphthalin, naphthamein, and a body of a purple colour. It is to Mr. Perkin that we owe the knowledge of several of these substances and their colour-giving properties. In my laboratory a fine purple colour has been obtained from naphthalin, which dyes with facility silk and wool, and the process is so far perfected as to enable me to show you some silk dyed and a piece of calico printed with it."

(To be continued.)

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, March 18, 1862.

J. C. Dyer, *Esq.*, *Vice-President, in the Chair.*

The following communication from Sir John F. W. Herschel, Bart., M.A., D.C.L., F.R.S., &c., Honorary Member of the Society, was read by Mr. Baxendell:—

In the Report of the Ordinary Meeting of the Literary and Philosophical Society for February 18, 1862, I find an abstract of a paper, "*On the Present State of Meteorology*," by Mr. T. Hopkins, M.B.M.S., in which he is reported to state that in my recent work on Meteorology, I "omit to notice the disturbing influence of the liberated heat of condensing vapour on the gases," and also that in that work I "abandon the old theory of winds" (meaning, I presume from the context, the Hadleian theory), "and attribute them to the action of aqueous vapour in a new form."

With regard to the former of these two statements, I beg to refer to Art. 53 of that work, where, alluding to the condensation of the vapour in the atmosphere, it is remarked that "in every case such condensation is accompanied with a mitigation of cold at the point where it actually takes place." What effect beyond this the condensation of vapour can produce on the gases, I am at a loss to understand. It can in no case give rise to an actual elevation of temperature above that of the mixture of air and vapour which may be introduced into any mass of cold air, and which may thereby effect a partial condensation. It can only act in mitigation of the chilling effect of such admixture on the introduced portion which would arise were the vapour not condensed. Inasmuch, however, as its condensation and precipitation as rain diminishes *pro tanto* the total barometric pressure, and therefore allows both air and vapour to flow in from other quarters, such condensation may, and no doubt does aid in producing atmospheric movements. (See *Meteor.*, Arts. 165, 171.)

But as respects the statement, that I abandon the Hadleian theory of the winds, I can only say that nothing is farther from my thoughts, and that I must protest against any such mode of expressing my views. It is true that in describing the *modus operandi* in which the sun's heat, acting on the equatorial regions of the earth, ocean, and air, produces that ascentional movement and overflow of the atmosphere, which is what Hadley assumes as the *primum mobile* of his theory, I have (*Meteorol.*, Arts. 54, 55, 58) brought expressly and prominently into view the very considerable share which the generation and ascent of vapour has in producing that result, but without ignoring or in any way unduly depreciating the effect of the rarefaction of the air itself. On the contrary, it is expressly said (Art. 53), "The general effect" (of the two causes) "is similar, and as the sun cannot generate vapour without at the same time heating the air, it is impossible to separate their dynamical effects. Whether the air go forth from its place *proprio motu*, or be jostled out of it by the introduction of a lighter medium, the local relief of pressure is equally produced." When Hadley wrote, the distinction between air and vapour was not recognised. He took the atmosphere *en masse*, and attributed to it an ascentional movement due to the heat of the sun, by the process of "rarefaction;" and this, so worded, remains true, although such rarefaction be a more complex process than he understood it to be.

The displacement of air by vapour, the disturbance of statical equilibrium, and the dynamical effects consequent thereon, are no new principles in meteorology. They have

been strongly insisted on, and, if I may venture to say so, rather over insisted on, and with a premature air of reduction to computative precision, by Daniell, in his Meteorological Essays, and perhaps hardly enough insisted on in my work on Meteorology which has given occasion to Mr. Hopkins' remarks. I would take this opportunity, however, to call the attention of meteorologists to the very extraordinary and abnormal features of the last two years, as strongly illustrative of the important part this element of meteorological dynamics has been playing in their production. The great outbreak of the solar spots occurred in the year 1859 with unusual suddenness, and on September 1 in that year, phenomena were exhibited in its photosphere indicative of a most remarkable state of excitement, accompanied with magnetic and auroral disturbances of unprecedented intensity and duration. This occurred as the sun was passing southward across the equator, and from the accounts received from Australia, the southern summer of that year (1859-60) appears to have been one of very unusual heat. The quantity of vapour thrown into the atmosphere during that summer from the Southern Ocean would seem not yet to have been got rid of, and to have given rise to diversions of the aërial currents, both of air and vapour, from their normal courses, which have not even yet subsided into their regular and habitual train. I throw out this, however, rather as a suggestion which I consider worthy of further examination by the light of meteorological records and returns collected on a very large scale, than as having myself arrived at any definite conception of the actual steps of the process that has been going forward.

Mr. Baxendell, in illustration of the remarks in the latter part of Sir. John Herschel's communication, read the following extract of a letter dated at sea, near Ceylon, January 30, 1862, which he had received from Mr. Thomas Heelis, F.R.A.S., who sailed from England for Calcutta on November 18 last:—

"We have had very peculiar and unsatisfactory trade-winds; we ran down inside of Madeira and the Cape de Verde Islands, and had the north-east trade almost the whole way down at E.S.E. Our south-east trade, which we picked up in the Bight of Benin, came out at S.S.W., but we afterwards, on going to the S.W., got it in its proper direction. We were on the skirts of three cyclones between the Cape and Amsterdam Island; and there have been within the last week evident indications from the set of a heavy swell that another cyclone is now running down to the Mauritius. The result is, that although we have had trade-wind clouds, we have had no S.E. trades in the Indian Ocean, and we are now, and have for some days been, in the N.W. monsoon, and have been nearly boiled by its warm damp atmosphere. The captain has told me that for days since leaving the Cape he has not known what to make of the weather, and his experience of these seas (which is great) has only served to puzzle him. This leads me directly to a point which bears strongly upon the theory of hurricanes, and which I want you to put in train. All old seamen agree that the trade-winds are variable in force and direction. I should be glad if Mr. Mosley, or some one who is from time to time in communication with Captain Maury, would try to get him to discuss his collection of above 500,000 trade-wind observations in order of time, as this question bears directly upon that of which I have spoken to you, viz., the perturbations, so to speak, of the hurricane orbits of the West Indies, by which their tracks sometimes pass over St. Domingo, and sometimes do not."

Dr. Joule made a communication "*On the Probable Cause of Electrical Storms.*"

The very close correspondence between the theoretical rate of cooling in ascending, and the actual, indicates a rapid transmission of the atmosphere from above to below, and *vice versâ*, continually going on. We may believe that during thunder-storms this interchange goes on with much greater than ordinary rapidity. At a considerable distance from the thunder-cloud, where the atmosphere is free from cloud, the air descends, acquiring temperature according to the law of convective equilibrium in dry air. The air then traverses the ground towards the region where the storm is raging, acquiring moisture as it proceeds, but probably without much diminution of temperature, on account of the heated ground making up for the cold of evaporation. Arrived under the thunder-cloud, the air rises, losing temperature, but at a diminished rate, owing to the condensation of its vapour to form part of the immense cumulus cloud which overcasts the sky on these occasions. The upward current of air carries the cloud and incipient rain drops upwards, but presently, in consequence of the increased capacity of the mass from the presence of a large quantity of water, the refrigeration of the air, in consequence of its dilatation, will be so far diminished as to prevent the condensation of fresh vapour, and ultimately to re-dissolve the upper portion of the cloud. This phenomenon, which has been noticed by Rankine in the cylinder of the steam-engine, will account for the defined outline of the upper edges of cumulus clouds. The upward current no doubt extends occasionally to regions below the freezing temperature. If cloud be carried with it, snow or hail will be formed, which, if sufficiently abundant, will pass through the cloud and fall to the ground before it is melted. Now, the dry cold air in which the snow and hail are formed is a perfect insulator. Ice has also been proved, by Achard, of Berlin, to be a non-conductor and an electric. Even water, in friction against an insulator, is known, from the experiments of Armstrong, explained by himself and Faraday, to be able to produce powerful electric effects, and this fact has been suggested by Faraday to explain powerful electric effects in the atmosphere. Sturgeon has noted the remarkable development of electricity by hail-showers. Few heavy thunder-storms occur without the fall of hail. Hail, whether in summer or winter, is almost, if not invariably, accompanied with lightning. In the presence of these facts, it seems not unreasonable to consider the formation of hail as essential to great electrical storms; although, as has been pointed out by Professor Thomson, very considerable electrical effects might be expected from the negatively charged air on the surface of the earth being drawn up into columns, and although, as the same philosopher has observed, every shower of rain gives the phenomena of a thunder-storm in miniature, the physical action of insulators and electrics in mutual friction must certainly produce very marked effects on the grand scale of Nature. If we suppose that the falling hail is electrified by the air it meets, the electrification of the cloud into which the hail falls might thus be constantly increased until the balance between it and the inductively electrified earth is restored by a flash of lightning. If the hail is negatively electrified by the dry air with which it comes in contact, the latter will float off charged with positive electricity, which may account for the normal positive condition of the atmosphere in serene weather, as well as the electrification of the upper strata evidenced by the aurora borealis. The friction of wind has been supposed by Herschel to contribute to the intense electrification of the cloud which overhangs volcanoes during eruption.

Dr. Thomas Alcock read a Paper "*On the Tongues of Mollusca.*"

Dr. Crace Calvert stated that one of the difficulties which Dr. Alcock had encountered in his extensive and laborious researches, namely, that of preserving his specimens from putrefaction, would be overcome by the use of a saturated solution of pure carbolic acid, taking care that a small excess of this powerful antiseptic should also remain at the bottom of the vessel in which the anatomical preparations were preserved. His authority was Dr. De Morgan, of the Middlesex Hospital, who stated to him, a few days since, that he had succeeded by that means in

preserving classes of animals which all other means had failed in preserving, namely, Molluscæ, Zoophytes, and Acalephæ.

Dr. Calvert also read a Paper "*On the Employment of Galvanized Iron for Armour-plated Ships.*" The author stated that no doubt many gentlemen present were acquainted with the fact that he had been for some time past engaged in ascertaining the chemical composition of various woods employed, and susceptible of being employed in the Navy. On a recent visit to one of the dock-yards, he found that while the armour-plates were fixed against a layer of teak, the ribs of the ship were of oak, and that the iron bolts which were to fasten the plates were to pass through the oak ribs. It occurred to him that the inconvenience which would probably result from the action of the oak upon the iron might be obviated by substituting galvanised iron bolts for those now in use, and he therefore instituted a series of experiments, the results of which he had great pleasure in laying before the meeting. The first series of experiments consisted in driving through large pieces of oak bolts and screws of unprepared iron and of galvanised iron, prepared by his friends Messrs. Richard Johnson and Brother, of Dale Street, Manchester, which were then immersed in soft and sea water for the last three months. The results clearly showed, firstly, that the friction did not remove the zinc from the galvanised iron; secondly, that the oak and galvanised bolts were unchanged; whilst the unprepared iron bolts were much rusted, and the pieces of oak had become quite black by the formation of tannate and gallate of peroxide of iron. During the experiments, the waters were changed every week, those containing the galvanised iron appearing unaltered, whilst in the case of the unprepared iron they had a dark blue-black appearance, owing to the formation of gallate and tannate of iron. In order to ascertain the comparative action of soft and salt water upon iron and galvanised iron when in contact with oak under identical circumstances, he made the following series of experiments. Plates of galvanised iron having eighteen inches of surface, lost during three months the following weights:—

	Soft Water.	Sea Water.
Plate No. 1.	0·10 grains.	—
„ No. 2.	0·11 „	—
„ No. 3.	—	0·095 grains.
„ No. 4.	—	0·090 „

Similar plates of iron lost during the same time:—

	Soft Water.	Sea Water.
Plate No. 1.	1·23 grains.	—
„ No. 2.	1 52 „	—
„ No. 3.	—	2·40 grains.
„ No. 4.	—	2·38 „

There can, therefore, be no doubt that galvanised iron offers great advantages, the action of water on it being less than a tenth of the same action on unprepared iron, and further, as iron when galvanised is in the most favourable electrical condition to resist the action of oxygen, being in an electro-negative condition, it follows that in all probability the use of galvanised iron would be very advantageous in armour-plated and other iron ships. The author hoped that Government and other large ship-builders would avail themselves of this suggestion, and make experiments on a large scale to verify the results he had obtained.

NOTICES OF BOOKS.

Report on the Past, Present, and Future of the Royal Institution, chiefly in regard to its Encouragement of Scientific Research. By the Honorary Secretary. London: W. Clowes and Sons.

(*Continued from page* 180.)

The Report terminates with the following *conclusions*:—

"Why then do our Professors stay? They stay to follow out original research. They are kept by the opportunity and encouragement that is given to experimental science, by the Board of Managers and the body of Members.

"In sixty years the original work that has been done in the cellars of the Royal Institution has greatly aided in changing the state of science in Europe; and if in this time the Institution has gained so high a reputation for discovery, whilst the task of diffusing instruction has extended so widely into other hands, does not this point out our future course?

"We must leave in great part to other bodies the diffusion of philosophical knowledge. By their numbers at least they will do this work more extensively and in a manner better fitted for the general public, if not more systematically, than the Royal Institution can do; but no body has done what we have done, and no body is therefore as likely to do the work that we have yet to do in experimental research.

"Research is the glory of the Institution, and to promote research should be its chief aim.

"What then is wanted? Chiefly four things.—1st. We must continue to choose our Professors well. 2nd. We must give them the utmost time for original research. 3rd. We must supply them well with means for work; and 4th. We must keep them long.

"1st. Hitherto, as the result has shown, the choice of our Professors has been the success of the Institution. In future elections the great object must be to choose the man who is likely to do the most scientific work.

"2nd. The amount of time now required for the lectures of the Professors is comparatively short; and if they had no other duties excepting those of their position at the Institution, eight months or more, at least, each year might be given to research. But can our Professors now LIVE on what they receive from the Institution? are they not of necessity compelled to give a great part of their time to other lectures elsewhere?

"3rd. Apparatus is sure to be provided when it is wanted; thus, when Davy asked for a large voltaic battery, his want was supplied by voluntary subscriptions, and now by the same means any amount could be raised. In proof of this it may be mentioned that one of our members, Sir H. Holland, without asking, anticipates many of the special wants of the Professors, by giving each year 40*l.* for apparatus that may be required for research.

"4th. There remains only that having good Investigators, we should keep them long. Each year one is asked to leave us. Large bribes are offered. Great promises are made. Our Professors, moreover, might gain probably many thousands a year if they would leave research and give their time to those who would pay for scientific advice. The sums we are able to pay our Professors are comparatively so small that we are obliged to allow them to give part of their time to other Institutions, to earn the means of living. Thus they are enabled to live only by depriving themselves and us of scientific research. They are obliged to say and do the reverse of that which Davy said:—'Having given up lecturing, I shall be able to devote my whole time to the pursuit of discovery.'

"In 1803, Sir H. Davy delivered six Lectures to the Board of Agriculture, and was retained by the Board at a salary of 100*l.* a year as Chemical Professor. He lectured for ten successive seasons.

"In November, between the 8th and 29th, 1810, he delivered a course of Electro-Chemical Lectures to the Dublin Society, for which he received 525*l.*

"In 1811, he delivered to the Dublin Society two distinct courses: one, on the Elements of Chemical Philosophy, and the other on Geology, for which he received 750*l.* in all.

"For twenty-one years, from 1830 to 1851, Mr. Faraday lectured at Woolwich, and gave a small part of the lectures to the Medical School of St. George's Hospital.

"A portion of his time also has, ever since 1836, been given to the Trinity House, as the scientific adviser in questions relating to Lighthouses.

"Dr. Tyndall is Professor at the School of Mines, Examiner, &c., and has given courses of lectures at the London Institution and elsewhere.

"By thus employing their time elsewhere, our Professors have been enabled to stay at the Institution. They cannot work elsewhere without stinting the time they would otherwise give to original research in our laboratory.

"If another Institution should offer more time for research by giving more means for support, can we be surprised if our Professors should be tempted away? Hence, to make sure of keeping them we should pay them as they could be paid elsewhere.

"A professor of Chemistry in the pay of the Government can earn nearly 1000*l.* a-year; and many scientific men by private practice, giving advice, make more than 1000*l.* a-year.

"The demand for scientific Professors for Great Britain and the Colonies is increasing, and if we wish to keep our Professors, and to enable them to give the greater part of their time to the promotion of experimental inquiry, we must pay them better.

"So long since as 1833 one of our members, Mr. Fuller, feeling how inadequately Mr. Faraday was paid, endowed a Professorship of Chemistry, with the yearly interest of 3333*l.*, and he appointed Mr. Faraday Professor, without calling upon him for lecture duty. Mr. Fuller also endowed with the same sum a Physiological Professorship, and he left an equal sum to accumulate, and this may ultimately be used to increase the income of our Professors. Meanwhile the ordinary income of the Institution is unable to give more; and until the Professorships are better endowed by the liberality of the wealthy, the position of the Institution must remain imperfect and insecure.

"The Professors of Chemistry and Natural Philosophy, together, now receive from subscriptions and endowments a total of 750*l.* a-year. If we could obtain donations and bequests to the amount of about 25,000*l.*, this would give 700*l.* a-year to each Professor, and if 50,000*l.* should be raised or be left to the Royal Institution, to enable the Professors to give the chief part of their time to scientific research, who would say that 1000*l.* a-year was a large sum for a Young or Davy, for a Faraday or a Tyndall?

"Large sums have been bequeathed, in the last few years, for the promotion of science. Some part of these bequests might have been given to the Royal Institution, if it had been known what that body had already done, and how much permanent gain might result to science if the Professors could devote more of their time to research.

"For example, Mr. Jas. Smithson, an Englishman, who died in 1829, left a very large sum (500,000*l.*?) to the United States of America for the increase and diffusion of knowledge among men. After much was spent in law, the American Congress, in 1838, received upwards of 100,000*l.* The funds accumulated until 1846, when the Smithsonian Institution was founded at Washington, with an annual income of 6500*l.*

"By the will of Mr. W. Ford Stevenson, who died in 1852, about 28,000*l.* were left to the Royal Society.*

"I ask for no subsidy for the Royal Institution from the Government; but I appeal, by sixty years of facts, to the public for whose good our Institution was founded. It arose out of 'a Society for bettering the condition of the poor.' It has bettered the condition of the world, by the discoveries that have been made during sixty years by its Professors, Young, Davy, Faraday, and Tyndall.

"To ensure its permanence its Professorships must be better endowed.

"The liberality of one man or of many may enable the Laboratory of the Royal Institution to continue always what it has become, the most celebrated in Europe for scientific research.

"In this great work ladies also may greatly help science, either directly by their support, or by their influence over those who can give support. The Royal Institution, almost from its commencement, has admitted ladies to the membership, and encouraged them to attend the lectures of the Professors, and I cannot conclude this Report better than by the words of Sir H. Davy in his lecture in 1810:—

"'Our doors are open to all who wish to profit by knowledge, and I may venture to hope that even the female part of our audience will not diminish, and that they will honour the plan with an attention which is independent of fashion or the taste of the moment, and connected with the use, the permanence, and the pleasure of intellectual acquisitions. It is not our intention to invite them to assist in the laboratories, but to partake of that healthy and refined amusement which results from a perception of the variety, order, and harmony existing in all the kingdoms of nature, and to encourage the study of those more elegant departments of science, which at once tend to exalt the understanding and purify the heart.

"'The leisure of the higher female classes is so great, and their influence in society so strong, that it is almost a duty that they should endeavour to awaken and keep alive a love of improvement and instruction.

"'Let them make it disgraceful for men to be ignorant, and ignorance will vanish, and that part of their empire founded upon mental improvement will be strengthened and exalted by time, will be untouched by age, will be immortal in its youth.

"'Even in the common relations of society, how much must be referred to the conduct of the female mind! The mother gives, or ought to give, most of the early impressions to the child, and his future habits may depend in some measure upon her influence. It may in some measure depend upon her whether he becomes an honour or a disgrace to his country. Her power of enforcing instruction is the most effectual, as flowing from love.—We know that without feeling, the human being is mere clay, the dust of the earth without the living soul. Whatever is to be permanently infixed in the understanding must be associated with hope, or with joy or with passion. How much more efficacious must instruction be when communicated by an object beloved and venerated, and in infancy almost adored, and when, instead of being afforded with an effort of pain and of labour, it is carried into the heart by kindness, and made delightful by caresses and smiles!'

"The Royal Institution is not a child of the nation requiring instruction, but it requires support to ensure the continuance of its glorious career in scientific research."

* Since 1849 1000*l.* per annum has been voted by Parliament, for the promotion of science. The distribution of this grant is intrusted to the President and Council of the Royal Society, from whom a return of its application is made to the House of Commons.

NOTICES OF PATENTS.

Grants of Provisional Protection for Six Months.

2702. John Watt, Lorrimore Street, Walworth, Surrey, and Thomas Snaith Haviside, Cornhill, London, "Improvements in the manufacture of soap."—Petition recorded 29th October, 1861.

126. Barrow Moss, Liverpool, "The application of steatite, either alone or in combination with other substances, to the manufacture of bricks, fire-bricks, the lining of furnaces, and other similar purposes."—Petition recorded 17th January, 1862.

356. William Wood, Monkhill, Pontefract, Yorkshire, "Improvements in the process of manufacturing pomfret or liquorice cakes."—Petition recorded 11th February, 1862.

428. Richard Watkins, Lower Belgrave Place, Pimlico,

London, "Improvements in oil and spirit lamps, and in the means of producing light therein, parts of which improvements are applicable to lamps generally."

484. Marc Antoine François Mennons, Rue de l'Echiquier, Paris, "Improvements in burners for heating by gas."—A communication from Carsten Richard Meyn, Carlshütte, Rendsburg, Holstein.—Petitions recorded 22nd February, 1862.

504. Edwin Bliss, Percival Street, Clerkenwell, and Henry Lamplough, Holborn Hill, London, "Improved means for viewing microscopic photographs and other minute objects."—Petitions recorded 25th February, 1862.

524. John Cliff, Imperial Potteries, Lambeth, Surrey, "Improvements in glazing stoneware, red clayware, porcelain, and other kinds of earthenware."

532. George Torr, Bucks Row, Whitechapel, London, "Improvements in, and an improved apparatus for manufacturing and reburning animal charcoal."

72. Robert Johnson, Liverpool, "An improved composition for coating the bottoms of iron ships to prevent their fouling, and which composition may be used as a protective coating for wood, iron, or other substances exposed to the action of sea-water."—Petition recorded 10th January, 1862.

128. Julius Cæsar Dickey, Saratoga Springs, Saratoga, New York, U.S., "An improved quartz crusher."—Petition recorded 17th January, 1862.

423. Edward Thomas Hughes, Chancery Lane, London, "An improved method of, and apparatus for collecting the gases given off from furnaces."—A communication from Emil Langen, Siegburg, Prussia.

424. Thomas Birdsall and James Birdsall, Leeds, Yorkshire, "Improvements in preparing hides or skins for tanning."—Petitions recorded 17th February, 1862.

429. Charles Denis Segoffin, South Street, Finsbury, London, "An improved apparatus for the purpose of viewing photographs on cards."—Petition recorded 18th February, 1862.

445. James Paterson, Middle Temple, London, "Improvements in means or apparatus for re-burning animal charcoal."—A communication from George Alexander Drummond, Montreal, Canada East.

455. James Paterson, Middle Temple, London, "Improvements in the use of animal charcoal."—A communication from George Alexander Drummond, Montreal, Canada East.

495. Lewis Davis, Gloucester Gardens, Hyde Park, and Frederick Major Parkes, Marylebone Road, London, "An improvement in the production or manufacture of gas for lighting and heating."

512. Courtenay Kingsford, Fenchurch Street, London, "A new composition for the manufacture of bread."

525. William Miller, Upper Stamford Street, Blackfriars, Surrey, "Improvements in the manufacture of sugar."

564. Patrick Robertson, Sun Court, Cornhill, London, "Improvements in treating yeast and in the manufacture of ammoniacal salts, and a substitute for animal charcoal."

574. Thomas Bell, Wishaw, Lanarkshire, N.B., "Improvements in apparatus for distilling shale and other bituminous minerals."

581. Gustav Bischof, jun., Swansea, "Improvements in treating ores and solutions containing copper and iron, or either of them, to obtain products therefrom."

587. Bridge Standen, Salford, near Manchester, "Improvements in the preparation or manufacture of portable manure or fertilising compound, and in the collection or extraction therefrom of a certain liquid applicable to various purposes, and also in machinery or apparatus to be employed therein."

Notices to Proceed.

2677. Thomas Richardson, Newcastle-on-Tyne, and Robert Irvine, Hurlet, Renfrewshire, "Improvements in treating bones and gelatine."

2694. William Smith, Leek, Staffordshire, "Improvements in the preservation of stone, brick, and other such materials used in building, applicable also to the waterproofing of walls."—Petitions recorded 26th October, 1861.

2716. John Henry Johnson, Lincoln's Inn Fields London, "Improvements in the preparation or treatment of skins and hides."—A communication from Dr. Frederich Knapp, Munich, Bavaria.—Petitions recorded 29th October, 1861.

2819. Richard Archibald Brooman, Fleet Street, London, "Improvements in obtaining alkaline phosphates."—A communication from Edouard Aubertin, Paris.—Petition recorded 9th November, 1861.

2702. John Watt, Lorrinfore Street, Walworth, Surrey, and Thomas Snaith, Haviside, Cornhill, London, "Improvements in the manufacture of soap."

2726. Eugene de Bassano and Adolphe Brudenne, Brussels, Belgium, "Improvements in the manufacture of stearine."—Petitions recorded 30th October, 1861.

2790. Frederick George Stuber, St. James's Road, Brixton, Surrey, "An improved hygrometer for measuring the humidity of the atmosphere, dampness of beds, garments, and for other similar purposes."—Petition recorded 6th November, 1861.

2801. John Barrow, Dalton Chemical Works, West Gorten, near Manchester, "Improvements in the manufacture of benzole, naphtha, naphthaline, aniline, and carbolic acid."—Petition recorded 7th November, 1861.

2835. William John Hay, Southsea, Hampshire, "Improvements in protecting iron and wooden ships, caissons, dams, and other wooden or iron structures from decay, and from fouling by vegetable and animal matters, and in preparing the materials to be employed therein."

275. Friedrich Wilhelm Daehne, Swansea, Glamorganshire, "Improvements in furnaces used in the manufacture of zinc."—Petition recorded 1st February, 1862.

2781. John Peter Bourquin, Newman Street, Oxford Street, London, "Improvements in ornamenting the covers of photographic albums, books, writing-cases, and other like articles.

2783. Henry Orth, Wissembourg, France, "An improved soap."—Petitions recorded 5th November, 1861.

2787. Alexander Prince, Trafalgar Square, Charing Cross, London, "Improvements in furnaces for reducing zinc ores."—A communication from Adolphe Charlier, Stolberg, near Aix-la-Chapelle, Prussia.—Petition recorded 6th November, 1861.

2815. François Henry Marie Comé Damiens Chevalier Fenis de Lacombe, Paris, "Improvements in generating hydrogen gas for illuminating or other purposes, and in apparatus used therein."

Chemical Notices from Foreign Sources.

I. ORGANIC CHEMISTRY.

Action of Chloracetyl on Tartaric Acid.—By heating these two bodies together with proper arrangements, Pilz obtained (*Litzungst. d. Akad. d. Wissenschaft zu Wien*, Bd. xliv. s. 47) crystals of an acid having the formula $C_{16}H_8O_{14}$. They are very soluble in alcohol and ether, and dissolve slowly in water, the solution having a strongly acid reaction, and decomposing the alkaline carbonates. The crystals are obtained by subliming the mass which results from heating together chloracetyl and tartaric acid for some hours in an atmosphere of tartaric acid. The aqueous solution, evaporated to dryness on a water bath, evolves acetic acid, and leaves a residue of tartaric acid.

Desulphuration of Leucine.—Gorup Besanez (*Annal. der Chem. und Pharm.*, Bd. cxviii. s. 230) removes the sulphur by dissolving the leucine in a solution of

potash or soda, adding a solution of lead oxide in the alkali and then boiling. After some minutes the sulphide of lead separates, and then the excess of lead is removed by sulphuric acid, and the sulphate removed by filtration. The filtrate is now evaporated to dryness, the residue boiled in alcohol and filtered hot. On cooling, the leucine deposits in pearly crystals. Cystine may be desulphurated in the same way.

Transformation of Benzoic Anhydride by Hydrochloric and Hydrosulphuric Acid Gases. — By passing hydrochloric acid gas over anhydrous benzoic acid gently warmed, Mosling (*Ann. der Chem. und Pharm.*, Bd. cxviii. s. 303) procured chloride of benzoyle and benzoic acid. By means of hydrosulphuric acid he obtained crystals of what appears to be bisulphide of benzoyle.

II. ANALYTICAL CHEMISTRY.

Separation of Tartaric and Citric Acids.— We find the following method in the *Archiv. der Pharm.*, Bd. clviii. s 206. Add to the solution to be tested an excess of hydrated oxide of iron, and heat almost to boiling. Allow the excess of iron to deposit, decant the reddish-yellow clear liquid, and evaporate to a syrupy consistence. If the citric acid be pure, the residue remains red and clear, but the presence of a very minute quantity of tartaric acid (one centigramme) gives it a cloudiness, and tartrate of iron is deposited.

III. TECHNICAL CHEMISTRY.

Dyeing with Gold.—The present is a luxurious age, but we doubt if the following process will come into extensive use. Immerse the silk stuff in a bath of chloride of gold (strength not given) for ten minutes, then wring and dry. At first the silk has a clear straw-coloured shade. When exposed to direct sunlight the colour changes; but the change disappears in the shade. If the free acid is removed from the silk by rinsing in pure water, and the tissue is then spread out in the sun, it soon changes to a beautiful lilac. In summer an hour's exposure suffices, but in winter it often requires some weeks to effect the change. The colour, it is said, has a reddish *nuance* in direct sunlight and artificial light, but in the shade it looks blue.—*Chem. Centralblatt*, 1862, s. 32.

MISCELLANEOUS.

CURIOSITIES IN CHEMICAL EVIDENCE.

STAFFORD ASSIZES.—DOWNING *v.* CHANCE.

This was an action brought by a farmer to recover damages for injury done to his crops by the escape of hydrochloric acid from the alkali works of the defendants. The following chemical evidence was given for the plaintiff:—

Mr. Alfred Bird said he was a practical chemist, living at Birmingham. He had examined, on September 6, some wheat which had been pointed out to him; the ears appeared to have suffered an unusual injury, the grains were not properly formed, and the whole of the crop appeared more or less out of health. This he believed to have been caused by some burning or corrosive agent, and muriatic acid gas would have the effect. In the manufacture of soda from salt, the chlorine was liberated by the addition of sulphuric acid, and the chlorine, mixed with the hydrogen of the water, took the form of muriatic acid gas—a gas most poisonous to vegetation. There was no affinity between the smell of muriatic acid gas and the smell from iron or brick works. On September 6 the wind was from the direction of defendants' works, and the smell of muriatic acid gas was unmistakable. When this gas came in contact with ammonia, it formed sal-ammoniac, and when it approached ammonia the vapour assumed a denser and bluish appearance, consequent on the formation of chloride of ammonia. Witness took a sheet of filter-paper, which he soaked in ammonia, and hung on a line where the vapour came in contact with it. When the excess of ammonia was dissipated, he took the paper down, wrapped it up carefully, tested it with nitrate of silver, and produced chloride of silver, which showed that he had caught hydrochloric acid gas in the paper. *Cross-examined.*—Was sure that nothing but hydrochloric acid gas would have produced the same result as he found on his filter-paper. If cyanogen had been on the paper, the appearance and the result would not have been the same; it would throw down a white precipitate; but he could not mistake cyanide of silver for chloride of silver. He could not just then refresh his memory as to which two gases cyanogen was composed of at the moment; some of the compounds of hydrogen were so difficult, that he did not care to speak of them at a moment's notice. Had he met with chlorine on his test-paper, the same precipitate would be deposited, for in the water the chlorine would be formed into hydrochloric acid; but witness was sure the vapour was not chlorine, but hydrochloric acid.

Mr. W. M. Williams said he was a chemist at Birmingham, a Fellow of the Chemical Society, and Lecturer on Theoretical and Practical Chemistry at the Birmingham and Midland Institute. On the 27th of February he was near the defendants' works to discover if there was any escape of hydrochloric gas allowed. He was at a distance of some 200 yards from the works, and saw distinctly a vapour that came in a direct line from both of the chimneys of the works. He discovered the presence of hydrochloric acid gas by placing on two hillocks pieces of paper, on which he poured liquor ammonia. When the vapour touched the pieces of paper, dense fumes were formed of a peculiar bluish colour, which clearly showed the presence of hydrochloric acid gas. Witness also repeated the test used by Mr. Bird, of catching the vapour in filter-paper which had been soaked in ammonia, and then testing it with nitrate of silver, by which he obtained the characteristic white precipitate nitrate of silver. He further tested the solution with chloride of barium, and that gave no precipitate, which indicated the absence of phosphoric acid, of sulphites or sulphates, and the absence also of arsenic. [The cross-examination of this witness is, unfortunately, not reported, and we can, therefore, take no notice of it.]

The defence was, that no hydrochloric acid had been allowed to escape the defendants' works since April, 1861, and that the works were surrounded with brick, iron, and phosphate works, from all of which noxious vapours—chlorine, sulphurous, hydrochloric, and arsenious acids—were evolved. Drs. Wrightson, Hill, and Voelcker, were called on the defendants' side, but we need not quote their evidence. The verdict was entered for the plaintiff, with leave to the defendants to move that the verdict be entered for them.

ANSWERS TO CORRESPONDENTS.

*** All *Editorial Communications* are to be addressed to the Editor; and *Advertisements* and *Business Communications* to the Publisher at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

*** In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

Carbolic Acid.—We are informed that this valuable disinfectant may be obtained at the Tower Chemical Company's Works, Droylsden, near Manchester.

J. G. Tatters —1. We do not know the details of the process. 2. A letter addressed to Mr. Brady, M.P., House of Commons, will find him. 3. The date of any patent can be obtained at the Patent Office, Southampton Buildings.

F. C.—In testing solutions containing magnesia, you must have chloride of ammonium present, or the magnesia will pe partially precipitated by several reagents in the wrong place, and may be mistaken for other earths.

P. S. M.—Apply to Professor Redwood, 17, Bloomsbury Square.

THE CHEMICAL NEWS.

VOL. V. No. 123.—April 12, 1862.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On Soil-Analysis, by Professor S. W. JOHNSON, *of Yale College.*

(*Continued from page* 170.)

HAVING shown how small an error in sampling may affect the chemist's estimate of a soil, it is not out of place to insist for a moment that a similar error in the analysis itself must have the same result. In running over 200 pages of Dr. Peter's Fourth Kentucky Report, we find five analyses of soil in which there is a gain of from five to eight-tenths per cent.; we find twenty-three in which there is a loss exceeding five-tenths per cent. In thirteen of the latter the loss is eight or more tenths, in eight instances the loss is one per cent. or more, and in one case is one and eight-tenths per cent. We should scorn to notice little matters like these, errors which are inseparable from the best manipulation and the best processes, were it not that in soil analysis it is precisely the small quantities which alone have any importance.

We find in Dr. Peter's work, as in the work of all who have preceded him in the analysis of soils, from Davy and Sprengel downwards, evidence that the best endeavours in this line of research are entirely in commensurate with the desired results.

It may be objected to this criticism of the analyses, that the loss or gain must be distributed among the twelve ingredients determined. It is true that there is a probability that such distribution would be just; but this is by no means certain, and it is equally true that, this being done, there is still force in the criticism, for the four-tenths per cent. of the soil which a century of wheat crops would remove likewise consists of twelve ingredients.

The second result of these analyses, according to Drs. Owen and Peter, is what the former (Fourth Kentucky Report, p. 33) declares to be "a general law" "now established," viz. "that soil analysis is capable of showing the exhaustion in land of the mineral food of plants by continual cropping."

To show the removal of soil ingredients by cropping, the plan was followed of collecting soils from contiguous fields, one of which had been "cultivated," while the other was in its virgin state. On comparing the analyses it was found that in seventy-one* cases out of seventy-nine a loss had occurred in the soil which had been in use without manure from ten to fifty years. In eight instances, however, the analysis failed to show such a result, owing to local causes, the soil of the old field being based on a sub-soil richer than was the virgin field, or the old field having received washings of more elevated lands, &c.

The admitted richness of the old over the new soil in these eight exceptional cases is expressed by hundredths of per cent., *e.g.*, soil Nos. 982, virgin, and 983, cultivated, differ by 0·066 per cent. of potash. Soils 1144 and 1146 by 0·032 per cent. of phosphoric acid. Soils 1204 and 1205 by 0·092 per cent. phosphoric acid. Soils 1207 and 1208 by 0·033 per cent. potash. Similar fractions likewise show the amount of deterioration in the other seventy-one cases. We adduce two instances pointed out by Dr. Peter in the Third Kentucky Report, p. 207, and one given on p. 176 of the Second Arkansas Report:—

	Carb. of lime.	Magnesia.	Phos. acid.	Potash.
Virgin soil, No. 557 .	0·345	0·335	0·181†	0·156
Old soil, No. 558. .	0·215	0·465	0·103†	0·101
Difference	0·130	0·130 gain	0·078†	0·055
Virgin soil, No. 738 .	0·180	0·444	0·179	0·256
Old field, No. 739 .	0·145	0 388	0·163	0·179
Difference	0·035	0·056	0·016	0·077
Virgin soil, No. 288 .	0·121	0·371	0·127	0·116
Old field, No. 289 .	0·021	0·371	0·053	0·097
Difference	0·100	0·000	0·407	0·019

We were prepared to find these differences much larger. It is seen at a glance that they fall within the errors of Dr. Peter's own manipulation, and when we assert, that of ten analyses of the most homogeneous material made by the same analyst under the most favourable circumstances, five would differ among each other by an amount equal to the quantities upon which this "natural law" is supported, we assert what every competent analyst knows to be true, and what moreover pronounces most emphatically upon the value of such investigations.

It is therefore our conclusion, that while, as has long been known, the soil loses in mineral matter what the crop gains, it is doubtful if in any given case chemical analysis can indicate this difference with certainty, for the reasons that the accidents which affect analysis make the limits of inaccuracy to cover more than the loss by years of cropping. When we take into account the changes that are constantly progressing in the soil when under cultivation—changes by which the disintegration is hastened, changes by which it is made, in many instances, more retentive of soluble matters—when we remember that most cultivated crops, although they carry off in seed, stem, and foliage, a quantity of mineral matters, yet derive these in part from a depth below the range of analysis, and in their roots or stubble leave upon the surface salts brought up from a considerable depth—we perceive that the problem is so complicated with compensations and variable quantities as to put it beyond the reach of quantitative chemical analysis.

If, in any case, soil analysis does show, or appear to show, the exhaustion of the soil, it is, however, the appeal to experience which proves it; and as this is the first, most obvious, and an entirely sufficient proof, we

* Misprinted twenty-one, on p 31, Fourth Kentucky Report.

† Misprinted on p. 207, Third Kentucky Report, where the difference is made 0·045 instead of 0·078, as given above from the tabulated analyses.

do not see the value of the "law" that has 10 per cent. (eight-seventy-ninths) of exceptions, the existence of which, like that of the rule itself, is only to be established by comparison with the plain agricultural fact.

In short, if we admit the result as Drs. Owen and Peter would have it, of what use or interest is it?

The third point is, that analysis shows "the peculiarities of the soils derived from different geological formations." Says Dr. Owen, "These analyses most distinctly show that certain geological formations impart to the soil more of the important mineral fertilisers than others." The reader will be able "to see that it is those formations which are composed of easily disintegrating materials which, all other things being equal, yield the soils richest in phosphoric acid, lime, and potash, and at the same time contain the quantity of alumina and oxide of iron necessary to render them sufficiently retentive and attractive of atmospheric water and ammonia; therefore these soils are the best adapted for those grains and crops which require the largest proportion of these ingredients." "He will, moreover, be able to trace the gradual diminution in the proportion of the more important mineral ingredients, down from these extraordinarily fertile soils derived from the highly fossiliferous, argillo-calcareous beds of the lower silurian, the cretaceous, and the tertiary systems of the West; through the silico-calcareous soils of the upper silurian, devonian, and sub-carboniferous limestone strata, in which fossils are either more sparingly distributed, or in some cases almost wanting, and which are far less easy of decomposition; thence through the argillo-silicious soils of the coal measures with only locally organic remains, and these chiefly of plants, down to the more purely silicious soils prevalent where the non-fossiliferous sandstones of the coal measures and of the millstone grit, prevail to the exclusion of either shales or limestones, and which afford the most unproductive soils as yet analysed." While it is to be expected that rocks of complex origin rich in organic remains—which are evidences that the rocks themselves originally resulted from the deposition of the washings of fertile lands—should yield richer soils than sandstones or limestones, we do not see that analysis of the soil makes the fact more evident. Knowledge of the composition of a rock enables us to judge in a general way of the value of the soil, so far as this depends upon chemical characters. We do not see what is gained by further analyses of the soil. It would appear that the cheap mental processes of deduction or inference may accomplish here in a moment all that an expensive analysis can show.

We fail, moreover, to perceive that analysis shows "the peculiarities of the soils derived from the different geological formations." In a cretaceous or limestone soil we of course expect to find much carbonate of lime, and in a sandstone or millstone grit soil much insoluble silica or silicates, but the quantities of phosphoric acid, potash, and sulphuric acid do not appear to bear any definite relation to their geological origin. It is impossible to represent the composition of the soil of any geological formation by a typical statement of percentages, or to point out its peculiarities further than by an undefinable more or less. Although Kentucky and Arkansas lie mostly or altogether beyond the influence of drift, yet the action of running water in its constant passage from hill-top to valley has to a great degree obliterated from the soils those peculiar differences to be found among the rocks from which they have been derived.

A careful examination of the analyses recorded in the Arkansas survey shows that the average composition of the eight soils analysed from the lower silurian and of the fourteen from the millstone grit compare as follows, in regard to the more important ingredients:—

	Carb. lime	Magnesia.	Phos. acid.	Sulph. acid.	Potash.
Lower silurian, average of 8 soils	0·533	0·485	0·184	0·052	0·355
Millstone grit, „ 14 „	0·215	0·531	0·180	0·057	0·148

Here we see that the soils of the poorest formation are inferior to those of the richest only in carbonate of lime and potash. Of the soils of the millstone grit, nine are richer in carbonate of lime than the poorest of the silurian, and five of the former contain more potash than the poorest of the latter. On the other hand, but two of the silurian soils have higher per-centages of either carbonate of lime or potash than the richest soil of the millstone grit. If these figures demonstrate anything, it is the fact that no geological formation has the absolute monopoly of either barren or fertile soils. If the analyses of Dr. Peter show the "peculiarities of the soils of any geological age, then certainly these peculiarities are not remarkably peculiar!"

On page 50 of the Second Arkansas Report, Dr. Owen remarks as follows:—

"With the table of the composition of the ashes of plants to refer to, appended to this report, and after becoming acquainted with the usual proportions of mineral constituents in an average soil,—information which is easily acquired by looking over the table of soil analyses in this report,—it is easy for any individual to see, when he is provided with a reliable analysis of his soil, not only to what crop it is best adapted, but what kind of mineral fertilisers, if any, it requires as a manure, and how it compares in fertility to the various grades of soils from other farms and other States. Is not this knowledge of some value to the farmer?"

The above, we are of opinion, proceeded rather from the generous heart than from the critical brain of its lamented author. Had he attempted to do the things which he believed to be so easy, we are sure his statements would have lost somewhat of their directness, and would have appeared in a form highly modified from the above. "The usual proportion of ingredients in an average soil." What is an average soil? Our only way of deciding what is such a soil consists in noting the average yield of soils. But the yield depends not alone on the soil, but upon climate, weather, tillage, and various incidents and accidents. It depends not on the composition of the soil, not on the "proportion of ingredients" alone, but likewise on the condition of those ingredients, their state of combination, their solubility. It depends also on the physical characters of the soil, which determine the relations of the crop to the essential conditions of regulated heat and moisture. The soil is not less important to the plant in its function of home than in its function of food, the lodgings are of equal influence with the board. It is a nice work to balance these varying circumstances, many of which have as yet in our science no shadow of a numerical expression, and then to say how many thousandths of a per cent. of potash, lime, phosphoric acid, &c., belong to the "average soil."

Dr. Peter has, indeed, attempted to show the degree of availability of the elements of the soil by the following process:—"A quantity, generally thirty grammes, of the air-dried soil is placed in an eight-ounce strong vial, with a close fitting stopper, and the bottle is filled up with distilled water which has been charged with pure carbonic acid gas, under a pressure of about two

atmospheres. The bottle is allowed to remain for about a month at a temperature about that of summer heat." The matters thus dissolved were then analysed as usual. These results have this value, they show that the water of the soil is capable of dissolving all the elements of the food of plants. They furnish, moreover, a rough comparative view of the available matters in different soils. Beyond this we cannot attach any value to them.

(To be continued.)

On the Commercial Analysis of Chrome Ores, by Charles O'Neill, *F.C.S., Author of "Chemistry of Calico Printing and Dyeing."*

During the last two years, I have analysed nearly 200 samples of chrome ore, principally from America, and chiefly in the interest of consumers here. I have been frequently brought into conflict, in an indirect manner, with foreign analysts, on account of the discrepancy between my results and theirs. These differences have ranged from 5 to 25 per cent. of green oxide of chromium, my determinations being invariably less than those returned by the American chemists. Chrome ore is sold by its per-centage of chromic acid, calculated from the green oxide obtained, and very much dissatisfaction has, of course, arisen from the importers having to pay for a considerably higher per-centage than actually existed. The large consumers of chrome ores generally accept bills for the cargoes long before they arrive, and, as there appears to be no legal remedy for them when they discover the inferior quality of their ore, the complaints and correspondence have been numerous and unpleasant. There is nothing really new in the system of analysis which I follow, excepting the combination of treatments which the ore receives; it has the merit of being expeditious, and is sufficiently exact for all commercial requirements. I shall give it in detail, and if it has any defects I have no doubt they will be soon pointed out.

1. Grinding of the Ore.—The ore is sent for analysis in three states: either, first, massive, in lumps of two or three ounces each, called rock ore; secondly, in a granular state, called sand ore; or, thirdly, rather finely-ground, as it is used in the bichrome manufacture. A fair sample is taken, and reduced to a tolerably fine powder in an iron mortar. I weigh 6 grains of this upon a common balance, and grind in an agate mortar, taking 2 grains only at once; the grinding requires twenty minutes. I consider it sufficiently ground when all gritting-sound or feeling has disappeared, and the ore forms flat cakes rising under the pestle. The success of the analysis depends altogether upon the grinding, which must be scrupulously attended to; the caking mass of ore must be several times swept together with a trimmed quill or a stiff hair pencil, and spread out again with the pestle, so as to thoroughly crush every particle. The ground ore is then placed in a balanced watch-glass, and 5·1 grains of it taken. The usual qualities of ore are very little hygrometric, and present no difficulties in weighing.

2. Fusion with Bisulphate of Potash.—I prepare bisulphate of potash by acting upon pure nitrate of potash with sulphuric acid, fusing the whole at a red heat in a platinum capsule, and pouring to cool on a stone slab. Sixty grains of this are coarsely pulverised and placed in a platinum crucible, so large that the bisulphate when fused only rises one-fourth its height. The ore is then swept on the bisulphate, lightly mixed with it by means of a wire, and the whole exposed to heat for twenty minutes. The mass must be maintained in perfect and quiet fusion for ten minutes; for the first few minutes care must be taken that the fused mass does not rise over the edge of the crucible. When cooled, the mass must be detached from the crucible, by gently pressing the sides of the crucible, and received into a porcelain dish, where it is treated with water until entirely dissolved or disintegrated. If the mass comes out of the crucible in a solid lump, this dissolution will take a considerable time. I avoid this inconvenience by taking the red-hot crucible with a forceps, and cause the molten mass to flow round the sides, where it cools in a thin crust, easily detached from the crucible, and readily acted upon by water.

3. Precipitation of the Mixed Oxides.—Carbonate of soda is added to the acid mixture until it is strongly alkaline; the precipitate thrown upon a filter and slightly washed; then drained and dried as quickly as possible. Up to this treatment, the process is exactly that given by Hunt, fifteen years ago. If there be any ore unacted upon, it is visible here as a heavy, black, crystalline, or granular powder, easily discernible in the white dish. I never have a trace of this black powder visible; but young pupils, making an analysis, never fail to see it, either because they have not had patience in grinding or they have not had sufficient strength of wrist and thumb to effectually crush the ore.

4. Fusion with Carbonate of Soda and Chlorate of Potash.—A mixture is made of two parts by weight of powdered chlorate of potash and three parts of pure dry carbonate of soda; seventy grains of such a mixture is taken, and the greater portion thrown into a small glass mortar. The dried precipitate is detached as much as possible from the filter, allowing it to fall on the mixed carbonate and chlorate. The filter is then carefully burned, and the ashes added to the contents of the mortar; the precipitate is carefully mixed up with the alkaline and oxidising mixture, by means of a glass pestle, and the whole transferred to a platinum crucible; the remaining chlorate and carbonate serve to rinse out the mortar. The whole is mixed up with a wire, and exposed to a gradually-increasing heat until in perfect fusion, and kept so for ten minutes; the whole time of heating is from twenty to thirty minutes. Some ores, which contain a good deal of magnesia and silica, are difficult to fuse, and require the heat of a muffle, but for ordinary ones a Bunsen's burner suffices very well, and twenty minutes is long enough for the fusion. The fused mass is cooled, dissolved in hot water, and filtered from the oxide of iron, &c., the filter being well washed. Any undecomposed ore remaining will be found on the filter, and may be rendered visible by boiling its contents with hydrochloric acid. The substitution of chlorate of potash for nitrate was rendered necessary by the subsequent method of determining the chromic acid in solution. With nitrate of potash there is nearly always production of alkaline nitrite, or some other compound of the lower oxides of nitrogen, which causes the evolution of nitric oxide upon addition of acid, and the consequent partial or total reduction of the chromic acid. Chlorate of potash is easily and wholly decomposed by heat. I have never found any chlorate or perchlorate left after an ordinary fusion.

5. Estimation of the Chromic Acid.—I use a volumetrical method, depending upon the capability of sulphurous acid to deoxidise chromic acid at the ordinary temperature in presence of free sulphuric acid. I prepare a strong solution of bisulphite of soda, by passing sulphurous acid through caustic soda to satura-

tion, and then make it alkaline with caustic soda, so as to have a neutral sulphite, which is less readily oxidised by keeping than the bisulphite. I use a dilute solution made from this concentrated sulphite, of such a strength that one grain of pure bichromate of potash requires about, and not less than, 200 grains measure of the sulphite to deoxidise it. The value of the sulphite must be determined for every operation, since it is continually absorbing oxygen. This is done twice by weighing out three grains and four grains of pure bichromate, dissolving each of them in ten ounces of water and acidulating freely with sulphuric acid, then adding the sulphite from the burette, with continual stirring, until the chromic acid is destroyed. The stopping-point may be ascertained by the colour when one is accustomed to the reaction, but even an experienced eye will often be glad of additional evidence. A mixture of iodide of potassium and boiled starch, slightly acidulated, forms a delicate test; it has usually a faint colour, which is even preferable to a colourless mixture. An exceedingly small quantity of chromic acid developes the blue colour in spots of this mixture, and a very slight excess of sulphite makes it colourless. One division of the sulphite test-liquor, or 0·05 grains of bichromate of potash in twelve ounces of water, easily and quickly influences the test mixture, The chromate from the chrome ore is tested in the same manner, and the quantity of oxide of chromium or chromic acid calculated from the equivalents of bichromate of potash. The tenth of a grain more than five taken is to allow for all losses, and the results are multiplied by twenty for the per-centage. 151 of bichromate of potash is reckoned equivalent to 80 of green oxide of chromium, and 104 of chromic acid. A determination can be made by this process in three or four hours, and a double determination in a little longer time. I usually make two analyses, and reject them if they present a difference of more than two per cent. of oxide of chromium; usually the results are within one per cent.

I have been favoured with an outline of the method of analysis followed by one of the American chemists whose results differ from mine. The ore is ground, half a gramme being taken, fused with bisulphate for a much longer time than I think necessary, and then, without removing the fused mass, the necessary quantity of carbonate of soda and nitrate of potash added, and the whole fused again; the resulting mass digested a long time with water and filtered; the filtrate mixed with carbonate of ammonia, and kept in a warm place for some hours, to precipitate alumina; the liquor again filtered, acidulated, and the chromic acid reduced by sulphurous acid; excess of ammonia added to precipitate the oxide of chromium, which is collected upon a filter and weighed in the usual manner. I think this process may be called a bad modification of Hunt's. There would be a saving of time and trouble if the alkaline and oxidising mixture could be added to the bisulphate as directed; but from two or three attempts I made, I found it impracticable without having platinum crucibles of an excessive size, or else previously heating the bisulphate so intensely as to expel entirely the extra atom of sulphuric acid. The fluid containing the chromic acid will contain likewise alumina, silica, magnesia usually, and titanic acid sometimes. The carbonate of ammonia may remove the alumina, but it will not remove the silica and magnesia. These, I believe, will be precipitated, wholly or in part, with the oxide of chromium, whence, I presume, the discrepancies complained of.

I find that the bulk of American chrome ore contains from thirty to thirty-five per cent of oxide of chromium. The richest ore I ever examined was massive rock ore, which contained fifty-eight per cent. of sesquioxide of chromium. I have examined specimens of titaniferous iron sand, sold to manufacturers and applied by them as chrome ore, which contained less than one per cent. of oxide of chromium. Other samples of ore contained three to seven per cent., but thirty per cent. is a good average. It is in the argillaceous chrome ores that the greatest difference of results have been obtained by myself and the American analysts. One sample, sold as an ore of thirty-four per cent., only yielded to me nine per cent. This error was admitted and explained by saying that the method of analysis was found not applicable to ores containing much clay. I believe if the Transatlantic analysts were to analyse their precipitates of the sesquioxide of chromium, they would find them to contain magnesia, silica, and sometimes alumina. I consider their determinations can only be correct in ores free from silica, magnesia, and alumina, and these are very rare.

92, Grosvenor-street, Manchester.

TECHNICAL CHEMISTRY.

On the Construction of Basins and Reservoirs Unattackable by most Acid or Alkaline Liquids, by M. H. Kalisch.

Unless by making use of wrought or cast iron (which have the inconvenience of being easily attacked by all acid liquids), it has been found very difficult to construct reservoirs capable of resisting the action of boiling solutions of caustic alkalies.

Most of the materials or luting proposed for this purpose are either much too easily acted on, or are too expensive for application on a certain scale.

The author proposes to line the sides of such stone reservoirs with plates or slabs of *heavy spar* (native sulphate of baryta), and to cement all the joints with a luting prepared in the following manner:—

Digest one part of india-rubber, in small pieces, with two parts of freshly rectified spirit of turpentine until the mixture becomes perfectly homogeneous, then incorporate with it four parts of powdered sulphate of baryta.

Reservoirs constructed in this way ought to resist not only the corrosive action of boiling caustic alkalies, but most organic or inorganic salts,—for instance, sulphates, chlorides and nitrates of zinc, iron, copper, soluble glass, cream of tartar, &c., and boiling hydrochloric, phosphoric, boracic, oxalic, tartaric, and citric acids, and slightly diluted cold sulphuric acid. — *Repertoire de Chimie*, iii. 474.

Preparation of Pure Nitrate of Silver, by M. Lienau.

Attack cuprous silver containing copper by nitric acid; to the solution, sufficiently concentrated, add chlorine water, freshly prepared, which precipitates the silver only. Then wash the precipitate in chlorine water, which prevents the chloride of silver from decomposing under the influence of light, and renders it more speedily soluble in ammonia; when well washed, dissolve it in the liquid, and plunge into the solution a well cleaned copper-wire. As the copper dissolves, the silver is precipitated, and is deposited as a brown powder; the operation is at an end when the wire preserves its brightness after being washed in water.

To render the precipitated silver perfectly pure, it is only necessary to wash it in ammoniacal water.—*Archiv. der Pharm.*, cvi. 27.

PHYSICAL SCIENCE.

On Spectrum Analysis, by* W. A. Miller, *M.D., LL.D., V.P.R.S., Professor of Chemistry, King's College, London.*

The subject which I propose to bring before you on the present occasion is one which, from the striking character of its results, the precision of the observations, and the extension of views to which it has given rise, has attracted a large share of the attention, not only of the scientific community, but also of that of educated persons in general.

It must not, however, be supposed that the subject is a new one, or that it has reached its present development by the exertions of any single individual. It may not, therefore, be uninteresting to trace the principal steps of discovery, from the time of Newton, who first examined the solar spectrum, to the present day.

Newton, by admitting a beam of solar light through a small circular aperture into a darkened room, and allowing it to fall upon a triangular prism of glass, obtained the magnificent coloured image known as the solar spectrum, which shades off, by insensible gradations, from the least refracted red into the most refracted or violet portion of the light. But it does not appear that any one, from Wollaston's time, a century later, examined the effect of admitting the light through a narrow slit, with sides parallel to those of the prism (*Phil. Trans.*, 1802, 378). Wollaston found that the spectrum so obtained was not, as it appeared to be by Newton's mode of examination, a continuous stripe of light, but that it was crossed at right angles to its length by dark bands.

It was not, however, till 1815 that these dark bands were carefully examined, when the celebrated German optician, Fraunhofer, published a minute description of them, accompanied by a careful map, in which he figured more than six hundred of these lines, which have ever since borne the name of *Fraunhofer's lines*. The more important of these lines he distinguished by the letters of the alphabet, and in the uppermost spectrum shown in the figure, a few of them are given as points of comparison with other spectra.

As Fraunhofer's mode of observing the spectrum is essentially that adopted by all subsequent inquirers, it will be necessary to point out the leading features of the method which he employed. The sun's light having been admitted through a narrow vertical slit into a darkened room, was allowed to fall upon a prism with its axis parallel to the slit, and at a distance of about twenty-four feet from it. The prism was fixed before the object-glass of a telescope, of low power, in such a manner that the angle formed by the incident light with the first face of the prism, was equal to that formed by the refracted beam with the second face. Under these circumstances, he observed numberless vertical lines varying in breadth and in strength in different parts of the spectrum. These bands were always visible, whatever was the solid or liquid medium used in the construction of the prism, and whether its refracting angle were great or small; and, under all circumstances, they preserved the same relative position in the respective coloured spaces in which they occur.

When, however, the source of the light was varied, as if the flame of a candle, the light of the fixed stars,† or the spark from the electrical machine was made use of, a different set of lines was in each case observed to occur.

Beyond this fact, viz., the dependence of the position of the lines upon the source of the light employed, Fraunhofer was unable to ascertain anything connected with their cause.

The inquiry thus launched by Fraunhofer has been followed in four principal branches of research, which may be described as relating to,—

1. *Cosmical lines*, or the black lines produced in the light of the sun, the planetary bodies, and the fixed stars.

2. *Black lines produced by absorption*, a class of phenomena discovered by Sir D. Brewster, in his observations upon the red vapours of nitrous acid.

4. *Bright lines produced by the electric spark* when taken between different conductors.

4. *Bright lines produced by coloured flames*, or by the introduction of different substances into flame.

The following chronological table contains the names of those who have made the principal steps in these different subjects:—

Newton . .	1701
Wollaston . .	1802
Fraunhofer . .	1815

Cosmical.	Absorption Bands.
Brewster, 1832.	Brewster, 1832.
E Becquerel, 1842.	W. H. Miller and Daniell, 1833.
Draper, 1842.	
Stokes, 1852	W. A. Miller, 1845.
Brewster and Gladstone, 1860	

Electric Light.	Coloured Flames.
Wheatstone, 1835.	Brewster, 1822.
Foucault, 1849.	Herschel, 1822.
Masson, 1851-55.	Fox Talbot, 1826, 1834.
Angstrom, 1853.	
Alter, 1854-55.	W. A. Miller, 1845.
Secchi, 1855.	Swan, 1857.
Plücker, 1858-59.	
V. Willigen, 1859.	

Kirchhoff . .	1859
Kirchhoff & Bunsen	1860

1. The Cosmical Lines admit only of partial illustration in the lecture-room by means of photographs, by which they may be projected upon the screen. Most of these lines shown by the photograph are, however, invisible to the eye, as they occur in that part of the spectrum which is more refrangible than even the violet rays. Edmund Becquerel‡ was the first who received the impression of the spectrum with suitable precautions upon a daguerreotype plate, and he made the important and interesting discovery that the inactive spaces in the portion of the chemical spectrum produced by visible rays, correspond accurately with the dark lines of Fraunhofer,—a discovery immediately afterwards corroborated by the independent observations of Dr. Draper, of New York, and subsequently confirmed in a remarkable manner for the invisible rays, by Stokes, who (*Phil. Trans.*, 1852) succeeded in rendering the lines in the

* A Lecture delivered before the Members of the Pharmaceutical Society of Great Britain; from the *Pharmaceutical Journal*.

† The Moon and Venus exhibit lines corresponding with those of the Sun. Sirius shows different lines, and Castor others somewhat similar. Amongst the lines of Procyon, Fraunhofer recognised the solar line D, and in those of Capella and Betalgeus, he described both D and *b*.—*Brewster's Edinburgh Journal of Science*, 1828, vol viii. p. 7.

‡ *Bibliothèque Universelle de Genève*, August, 1842, No. 80; or Taylor's "Scientific Memoirs," ii. 537.

most refrangible and extra-violet portions apparent to the eye, by his discovery that the fluorescent power of the spectrum was interrupted by inactive spaces, the position of which corresponded accurately with the lines observed by Becquerel.

2. The Absorption Bands, produced by coloured gases, were first indicated in 1832 by Sir David Brewster (*Phil. Mag.*, May, 1836, viii. 384), who found that the brownish-red vapours of nitrous acid have the remarkable power of absorbing the sun's rays in such a manner as to produce a series of dark bands in the light when transmitted through it. Professors W. H. Miller and Daniell subsequently showed that the same effect is produced, whatever be the source of light employed. In the course of this investigation, an important observation was made by Brewster, who noticed distinct lines and bands in the red and green spaces, which at other times wholly disappeared. This he found to be due to an absorptive action of the earth's atmosphere; for these bands were only visible when the sun approached the horizon. A few years later, I had an opportunity myself, whilst examining the spectrum of diffused daylight in the afternoon, during a violent thunder-shower, to observe the sudden appearance of a group of lines in the brightest part of the spectrum, between D and E, increasing in distinctness with the violence of the shower, and fading and disappearing as the rain passed away. These observations, therefore, prove that certain of the fixed lines in the solar spectrum are dependent, in part at least, upon the absorptive action exerted by the earth's atmosphere. But the larger portion, it is supposed, are due to another cause, first suggested by Kirchhoff. Professor Miller, of Cambridge, in conjunction with Professor Daniell, followed up these experiments, and showed that other coloured vapours, viz., those of bromine, iodine, and euchlorine possess this property (*Phil. Mag.*, 1833, ii. 381).

Twelve years afterwards, I myself made a numerous series of experiments upon the same subject (*Phil. Mag.*, xxvii. 81). The result of these experiments showed that mere existence of colour in a vapour does not indicate of necessity the existence of bands in its spectrum. The red vapours of chloride of tungsten give no lines, while bromine, which has to the eye the same colour, gives a remarkable series.

"The probable position of the lines cannot be inferred from the colour of the gas; with the green perchloride of manganese, the lines are most abundant in the green, whilst with the red vapours of nitrous acid they increase in number and density as they advance towards the blue end of the spectrum. Simple bodies, as well as compounds, may produce lines; and two simple bodies, which singly do not produce them, may in their compounds occasion them abundantly; *e.g.*, neither oxygen, nitrogen, nor chlorine, when uncombined, occasions lines, but some of the oxides, both of nitrogen and of chlorine, exhibit the phenomena in the most striking manner. There are, however, oxides, both of nitrogen and chlorine (some of them coloured), which do not occasion the appearance of lines. We find also that lines may exist in the vapour of simple substances, as in iodine, which disappear in their compounds. This is exemplified in the case of hydriodic acid. . . . Sometimes the same lines are produced by different degrees of oxidation of the same substances, a remarkable instance of which is furnished in the oxides of chlorine." . . . Coloured lithographs of several spectra accompany the paper, including those of bromine, iodine, peroxide of nitrogen, perchloride of manganese, and peroxide of chlorine.

3. On the Spectra of the Electric Spark.—Wollaston (*Phil. Trans.*, 1862) observed that the spectrum of the electric spark is not continuous, and that it differs from that of ordinary sunlight, as well as from that furnished by the light of a candle. Fraunhofer also has a similar observation, but the first person who called attention to the important fact that the nature of the metals employed modifies the resulting spectrum, was Professor Wheatstone, who, at the Dublin meeting of the British Association for 1835, read a paper "On the Prismatic Decomposition of the Electric, Voltaic, and Electro-Magnetic Sparks." In the abstract published in the Report of the Proceedings of the Association for that year, the author states that,—1. "The spectrum of the electro-magnetic spark taken from mercury consists of seven definite rays only, separated by dark intervals from each other. These visible rays are, two orange lines close together, a bright green line, two bluish-green lines near each other, a very bright purple line, and, lastly, a violet line." 2. "The spark taken in the same manner, from zinc, cadmium, tin, bismuth, and lead in the melted state, gives similar results; but the number, position, and colours of the lines vary in each case. The appearances are so different that by this mode of examination the metals may be readily distinguished from each other." A table accompanied the paper, showing the position and colour of the lines in the various metals used. . . . 3. "Where the spark of a voltaic pile was taken from the same metals, still in the melted state, precisely the same appearances are presented. 4. The voltaic spark from mercury was taken successively in the ordinary vacuum of the air-pump, and the Torricellian vacuum, in carbonic acid gas, &c, the same results were obtained as when the experiment was performed in air, or in oxygen gas. The light, therefore, does not arise from the combustion of the metal. Professor Wheatstone also examined by the prism the light which accompanies the combustion of the metals, in oxygen gas, and by other means, and found the appearances totally dissimilar to the above. . . . 5. When the (electric) spark is taken between balls of dissimilar metals, the lines appertaining to both are simultaneously seen."

The next point of importance relating to the subject of the spectrum produced by ignition in the voltaic arc, is due to M. Foucault, who in 1849 published in the *Journal de l'Institut*, February 7, a note on the light of the electric arc, reprinted in the *Ann. de Chimie et de Physique*, III. lviii. 476. In this paper he says:—

"I caused a solar image, formed by a converging lens, to fall upon the arc itself, an arrangement by which I was able to observe simultaneously the superposed solar and electric spectra, and I observed myself in this manner that the double brilliant line of the arc coincided exactly with the double black line of the solar light.

"This method of investigation enabled me to make some unexpected observations. In the first place, it proved to me the extreme transparency of the electric arc, which only casts a slight shadow upon the solar light. It has shown me that this arc, placed in the track of a beam of solar light, absorbs the rays D, so that the said line D of the solar light is considerably strengthened when the two spectra are exactly superposed. When, on the contrary, they only partially overlap each other, the line D appears darker than usual in the solar light, and stands out brightly in the electric spectrum, so that it is easy to judge of their perfect coincidence. Thus, the arc offers us a medium which itself emits the rays D, and which, at the same time, absorbs them when they come from another luminous source. In order to make

the experiment in a still more decisive manner, I have projected upon the arc the reflected image of one of the incandescent points of carbon, which, like all ignited solids, emits no lines; and in these circumstances the ray D appeared to me as in solar light."

The observations of Foucault contain the germ of Kirchhoff's important generalisation, but their ingenious author was far from perceiving their full importance.

The next observer whom we have to notice is M. Masson, who, in 1851 and 1855, in the course of his investigations on electric photometry (*Ann. de Chimie*, III. xxxi. 295, and xlv. 387), examined the spectra produced by various metals which were employed as dischargers to the Leyden jar, and also when heated by the voltaic arc, and gave drawings of the different spectra, made by means of the camera lucida. The spectra which he has given of the same metals which were examined by Wheatstone, are much more complicated than those described by the English philosopher. These discrepancies were subsequently explained by Angstrom (*Phil. Mag.*, 1855, p. 329), and by Alter (*Silliman's Journal*, xviii. 55, and xix. 213), who showed that, owing to the intense heat of the electric discharges employed by Masson, he obtained two spectra simultaneously, one due to the metal, the other to the atmosphere itself, which became ignited. Certain lines remarked by Masson common to the spectra of all the metals were really these atmospheric lines. By causing the spark to pass between the same metals when immersed in various gases, the particular lines due to the metal remained unaltered, whilst the others, due to the gaseous medium, disappeared, and were replaced by new lines. Angstrom, in the course of his paper, makes the following remarkable observations, which suggest, though they do not distinctly state, the explanation of Fraunhofer's dark lines, subsequently brought forward by Kirchhoff:—

"When the solar spectrum is compared with the electric one, it is found that some of the lines, such as C, D, E, and we may also say H, have their corresponding lines in the solar spectrum, but for the strongest, γ and δ, this is not the case. . . .

"Regarded as a whole, they produce the impression that one [spectrum] is a reversion of the other. I am, therefore, convinced that the explanation of the dark lines in the solar spectrum embraces that of the luminous lines in the electric spectrum."

In 1858 and 1859, an important series of investigations was published by M. Plücker (*Poggendorff's Annal.*, ciii. 88, 151, civ. 113, 622, cv. 87, cvii. 77, 498), relating to the character of the electric light produced by transmitting the secondary discharge from Ruhmkorff's coil through narrow tubes, filled with different gases, and subsequently exhausted as completely as possible. Vacuous tubes were thus obtained with only imponderable traces of various gases and vapours, including oxygen, hydrogen, nitrogen, chlorine, bromine, and iodine. Plücker found that each exhausted tube gave its own characteristic spectrum, and he measured with great care the principle lines visible in each. These results are very important in relation to Kirchhoff's theory of the cause of the dark lines, which requires that the position of bright lines thus obtained should coincide with the black lines produced by absorption when light is transmitted through these different gases. Plücker's experiments show distinctly that this is not the case in these gases.

Van der Willigen (*Poggendorff's Annal.*, cvi. 617), corroborated the observation of Angstrom, on the effect of gaseous media on the lines furnished by the spark between different metals, and he further made the interesting remark that, by placing in succession upon a pair of wires consisting of a metal which, like platinum, possessed no special bands of its own, small quantities of weak solutions of chloride of calcium, chloride of barium, chloride of strontium, nitrate of lime, &c., new metallic bands are produced, and these bands are characteristic of the particular metal contained in each of these several compounds.

(To be continued.)

On the Molecular Dissymmetry of Natural Organic Products, by L. PASTEUR, *Member of the Chemical Society of Paris.*

(Continued from p. 160.)

[SECOND LECTURE.]

I. If we consider material objects, whatever they may be, in the relation of their forms and of the repetition of their identical parts, we shall not be slow to perceive that they are distributed in two great classes, thus characterised:—Some placed before a mirror give an image which to them is superposable; the image of others would not cover them, although it faithfully reproduces all their details. A straight stair, a stem of distich leaves, a cube, the human body, are bodies in the first category. A winding stair, a stem of leaves spirally inserted, a screw, a hand, an irregular tetrahedron, are so many forms of the second group. These latter have no plane of symmetry.

We know, on the other hand, that compound bodies are aggregate of identical molecules, themselves formed of assemblages of elementary atoms distributed according to laws which regulate the nature, the proportion, the arrangement of them. The individual, for every compound body, is its chemical molecule, and this is a group of atoms, not a group pell-mell; there is, on the contrary, a very determinate arrangement. Such is the manner in which all physicists represent the constitution of bodies.

This stated, it would have been very astonishing had not Nature, so varied in her effects, and whose laws permit the existence of so many species of bodies, offered in the atomic groups of compound molecules both of these two categories, in which all material objects are distributed. In other words, it would have been very extraordinary if, among all chemical substances, natural or artificial, there had not been individuals with a superposable image, and others with an image non-superposable.

Things happen, in fact, as might be naturally anticipated; all chemical combinations, without exception, are equally distributed in two classes: those having an image superposable, and those having an image not superposable.

II. It is easy to show that this is a legitimate consequence, necessary from our first comparison. To place it in clear light, I will briefly recal the principal conditions of the decisive experiment which closed the preceding Lecture. I prepare, by the aid of natural paratartaric acid, the paratartrate of soda and ammonia. It is deposited in beautiful crystals.

Observed in a polarising apparatus, a solution of any portion of this double salt offers no indication of optical deviation; and in separating from the crystals the acid which they contain, paratartaric acid, identical with that which served to form them, is reproduced. So far all is simple and natural, and it seems as if we had to deal

with the crystallisation of an ordinary salt. There is nothing of the kind, however.

Take another portion of the same crystals, and examine them one by one. You will find that one-half has the form, a model of which I here present, characterised by a non-superposable hemihedrity; that the other half has the inverse form identical with the first in all its respective parts, and yet cannot be superposed on it. Then let the two kinds of crystals be isolated to be dissolved separately, and we observe that one of the two solutions deviates the polarised light to the left, and the other to the right, and both equally.

If we extract, by the ordinary chemical processes, the acids from these two kinds of crystals, we perceive that one of them is identical with common tartaric acid, and that the other is in all points similar, except that it cannot be superposed on it. They bear to each other the relations of the two salts from which they were separated. They resemble each other as much as the right hand resembles the left, or as two irregular symmetric tetrahedrons resemble each other, and these analogies and these differences are found in all these derivatives. What can be done with one may be repeated with the other under the same conditions, and the resulting products constantly manifest the same properties, with this single difference, that in some the deviation of the plane of polarisation is to the right, and in others to the left, and that the forms of corresponding species, although identical in all their details, cannot be superposed.

All these facts, so clear, so demonstrative, oblige us to refer the general exterior characters of these acids and their combinations to their individual chemical molecules. Not to do it would be to ignore the rules of the most common logic. It is thus we arrive at the following conclusions:—

1st. The molecule of tartaric acid, whatever it may be otherwise, is dissymmetric, and of a dissymmetry with an image non-superposable. 2nd. The molecule of the left tartaric acid is formed precisely by the group of inverse atoms. And by what characters shall we recognise the existence of molecular dissymmetry? In the one case by non-superposable hemihedrity; in the other, and especially, by the rotary optical property when the body is in solution. These principles being stated, let us examine all bodies, whether from Nature or the laboratory, and we shall find easily that among them a great number possess both this kind of hemihedrity and the molecular rotary property, and that all others possess neither the one nor the other of these characters.

I was right, then, in saying,—The legitimate and forced consequence of our first Lecture may be thus expressed:—

All bodies (I use this expression chemically) are divided into two great classes: bodies with a superposable image, and bodies with a non-superposable image; bodies constructed of dissymmetric atoms, and those formed of homohedric atoms.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

PHARMACEUTICAL SOCIETY.

Wednesday, April 2, 1862.

Mr. P. Squire, *President, in the Chair.*

Professor Bentley, in continuation of his papers on the new American remedies, made some remarks on the hydrastis canadensis. This plant, he said, had long been in use among the Indians as a dye and as a medicine, and had recently been introduced by the eclectic practitioners of America as a tonic. It was also supposed to have an especial action on the mucous membranes. However that might be, the substance was now in common demand, and it was well that pharmaceutists should be made acquainted with its characters and properties. The effects concerned physicians alone, and he should express no positive opinions about them. The root or rhizome of the hydrastis canadensis was variously known as yellow root and turmeric root; and the plant was sometimes called ground-raspberry, in consequence of the fruit somewhat resembling a raspberry. The plant belonged to the order *Ranunculaceæ*, and was found only in North America. It had no interest except as a medicine. As the flower was not beautiful, it had never attracted the attention of floriculturists. The part of the plant used in medicine was the rhizome. This had an irregular, twisted appearance, and was surrounded with numerous rootlets. It was rough, and had a yellowish appearance when fresh, but became brown by keeping. It had a marked odour like opium, and a strong, opiate, and bitter taste. An old specimen smelt like liquorice. It contained tannic and gallic acids, starch, resin, a yellow colouring matter, and, according to Durant, a peculiar, nitrogenised crystallisable substance, which was called hydrastine. This principle was said to be separated by making an aqueous extract, adding magnesia, then extracting by boiling alcohol, filtering hot, and evaporating the filtrate. The hydrastine was thus obtained in four-sided crystals. Two other hydrastines are found among the eclectic remedies, one a resinoid, the other a neutral principle. The yellow translucent crystals of the first body when treated with strong nitric acid evolve nitrous vapours, and yield a deep red solution. Strong sulphuric acid produces an olive brown solution, which on the addition of potash becomes turbid and deposits a resin. Dr. Malha has recently discovered that the substance sold in America as hydrastine is really impure berberine, a remarkable fact, as the first discovery of that body in a ranunculaceous plant. Berberine is now known to exist in four natural orders of plants. Hydrastine, the Professor said, was prescribed as a tonic in affections of the mucous membranes and in other cases. There are substances in the market,—hydrastin, hydrastine, and hydrastina. Perhaps the best preparation would be an alcoholic extract, which would contain all the active principles of the plant. More experience was necessary before any conclusions respecting the remedial value of the plant could be drawn. The Professor concluded by apologising for the somewhat discursive nature of his remarks.

The next paper was "*On Hydrastine*," by Mr. J. D. Perrin. The author commenced by remarking that the hydrastis canadensis was an excellent source of berberine, and that the hydrastine of commerce had been found to be an impure berberine. Berberine, however, is difficultly soluble in dilute nitric acid, while true hydrastine is insoluble in alkalies. To a solution of the two in nitric acid, ammonia is added until a precipitate just begins to appear; the liquid is then filtered, and more ammonia is added. The precipitate so produced is collected and washed with water. Hydrastine so obtained is seen under the microscope to consist of spherical granules, which, when pressed between tiles, seem to assume a crystalline appearance. To purify the substance, it is dissolved in hot proof spirit, the solution filtered while hot, and set aside to crystallise. By dissolving these crystals a second time and treating with animal charcoal, the substance may be obtained quite pure in four-sided prisms, which lose their transparency after drying, not, however, in consequence of the loss of water. One and a-half per cent. of hydrastine may be obtained from the dry root of the hydrastis. The author has not yet determined the exact composition of hydrastine, but had found

that it contained nitrogen. It is a powerful base, and forms double salts with mercury, gold, and platinum. It is soluble in alcohol, ether, and benzole, in which berberine is not soluble. The salts of hydrastine are soluble in water, except the carbazotate. The solutions of the salts have a bitter taste, followed by a sense of numbness. Hydrastine is not a poison; five grains administered to a rabbit only made it uncomfortable for a short time. The crystals touched with sulphuric acid and peroxide of lead, give a colour very much resembling that produced by mere traces of strychnia when treated in the same way. Chlorine water with hydrastine gives a blue fluorescence very like a solution of quinine. In conclusion, the author said he had found that the *xanthoriza apinfolia*, another ranunculaceous plant, also contained berberine. It was, besides, a perfect bitter and a good tonic.

"*On the Preparation of Iodide of Potassium and Iodides in general.*" By W. S. SQUIRE, Ph. D. After alluding to the difficulties in the way of the easy manufacture of iodide of potassium, and the numerous processes employed for the purpose (a manufacturer was said to have tried 700 processes, and then gave it up in disgust), Dr. Squire mentioned the well known methods of obtaining the salt by the double decomposition of the iodide of iron, or zinc and carbonate of potash, and also by decomposing the iodide of barium with sulphate of potash. In the two first processes pearlashes are generally used, and the impurities of these usually find their way into the iodide; the third method also has its objections. The author then proceeded to describe the process proposed by Liebig, namely, by acting on iodine with phosphorus, and so forming phosphoric and hydriodic acids, the solution of which is treated with milk of lime. Phosphate of lime is precipitated, and iodide of calcium remains in solution. The filtered solution of iodide of calcium is then decomposed with sulphate of potash, and any sulphate of lime is carefully removed by carbonate of potash. The solution of iodide of potassium may then be crystallised. A satisfactory result, however, the author said, was not to be obtained unless the salt was subjected to fusion. The iodide would not crystallise in the usual way, and the crystals would have a pinkish colour. Both of these objections were removed by the fusion. When making iodide of ammonium, the author recommended that the iodide of barium should be fused before the carbonate of ammonia was added. In this way the pink colour the crystals would otherwise have was obviated. A caution was then given against fusing iodide of lithium in a platinum crucible, lithium attacking platinum very strongly. The above process, although perhaps not applicable on a large scale, was not expensive. The atomic proportion of phosphorus being low and of iodine high, the quantity of phosphorus it was necessary to use in producing one pound of iodide of potassium would only cost a penny. Dr. Squire then alluded to the so-called periodide of iron, formed when the iodide of iron was kept a long time, and showed that the red solution formed when iodine was dissolved in the iodide of iron, readily yielded up the excess of iodine together, leaving the solution of the iodide. Iodide of mercury, he then said, was directed by the Pharmacopœia to be prepared by rubbing together equivalent weights of mercury and iodine; in this way an olive brown compound was obtained. But people objected to the appearance of this salt, and preferred a yellow iodide, which was made by precipitating the protonitrate of mercury with iodide of potassium. By this means some biniodide of mercury was formed, which gave the lighter colour to the compound. As the biniodide of mercury is a much more active medicine than the iodide, this process was highly objectionable. To prove that the biniodide was present in a specimen of the yellow salt, Dr. Squire boiled some with aniline and produced a red liquid; the olive brown iodide did not produce a red solution.

Mr. PERRIN asked if there was any evidence of alkalinity in the iodide of potassium prepared by Liebig's process?

Mr. SQUIRE said not necessarily; if more carbonate of potash was added than was sufficient to precipitate the sulphate of lime, the salt would, of course, be alkaline.

A MEMBER described a process he had employed for making iodide of potassium from iodide of lead and sulphate of potash.

Dr. SQUIRE said that process was a very expensive one, sulphate of lead always carried down with it a large proportion of sulphate of potash, which was lost.

The PRESIDENT then called attention to some bottles intended for external applications, which had been exhibited at a previous meeting. They are shaped something like a shoe, and must lie flat on the sole.

A MEMBER remarked that at a distance they looked very much like feeding-bottles.

The meeting (the last of the season) then adjourned.

SOCIETY OF ARTS.

Wednesday, February 5.

Dr. A. W. MILLER, *F.R.S., Professor of Chemistry at King's College, in the Chair.*

(*Concluded from page* 191.)

Dr. CALVERT concluded his account of the coal-tar colours with a brief notice of M. Roussin's so-called artificial alizarine, with which our readers are already acquainted, and then passed on to the consideration of the more mechanical improvements in the process of calico-printing, commencing with the engraving of cylinders, which point we will quote in full.

"This branch of calico-printing has made great progress. Not only have the engravings acquired sharper outlines and finer details, but the methods of engraving have greatly multiplied. I may cite as instances the application of the principle of the pentagraph, by Messrs. Smith, so as to trace patterns on the surface of rollers. Also, calico printers have extensively availed themselves of Mr. Locket's improvements for producing the groundwork of prints, or as they are termed 'covers,' by applying 'eccentric engraving,' or etching, which produces with facility most complicated patterns on a varnished roller, by means of a diamond point guarded by machinery. Another improvement, highly interesting in a scientific point of view, is the application of galvanism to the diamond tracer. By combining the galvanic action with an eccentric motion, most beautiful and delicate engravings may be produced. This is done by tracing the pattern with varnish on a zinc cylinder, which is so placed in the engraving machine that as a needle passes over its surface and comes into contact with the zinc, the galvanic current is established, and by simple machinery causes the diamond to trace the corresponding pattern on the copper roller. The communication is so rapid and precise that a great saving of time is effected. But if mechanical art has greatly assisted the engraver, chemistry has rendered him equally important services, by enabling him to abandon costly and cumbrous modes of impressing by force the designs on the cylinder, substituting for them a great number of etching processes. By some of these processes, as by every other addition to the resources of the engraver, an entirely new and beautiful class of engraving is produced, unattainable by any other known means. For instance, owing to various improvements, rollers of 43 inches in circumference and 44 inches long have been introduced, enabling the calico printer to produce cheaply large furniture patterns."

Mercer's process for increasing the strength and beauty of cotton fabrics by passing the cloths into a strong solution of caustic alkali, and then through dilute sulphuric acid, with subsequent washing, was next commented on; while Thom's improvements in "singeing" and "sulphur-

ing" followed in order. On the subject of "thickeners," Dr. Calvert referred his audience for details to the paper "*On Starches*" (Chemical News, Vol I. p. 41), read by him before the Society on December 21, 1852. This is to be regretted, inasmuch as there have been a good many experiments made, and some improvements effected since the above date. The starch and gluten from maize had not, we believe, then been tried to any extent, while as a matter of course the paper in question gave no particulars of glue (as now used for thickening in Germany and elsewhere), albumen, caseine, laetarine, &c.

The lecturer's general observations on the "madder styles" were scarcely of sufficient importance to claim a place in these pages, and to his remarks on "dyeing" also we must take exception, as being imperfect and somewhat obscure; no mention whatever was made of the influence of atmospheric electricity and ozone upon the dyeing process; nor was the well known retarding and injurious effect of easterly winds in any way referred to; while the recent suggestions and experiments for artificial dyeing by chemical means likewise escaped all notice.

With regard to "drug substitutes," the various double phosphates and arsenites of soda and lime, the silicates, borates, and arsenites of potassa and soda, have all been employed of late years with various results. In the way of the processes of "washing," "cleaning," and "finishing," the improvements effected during the last decade being chiefly confined to the minor details, call for no special notice here, and the same observation applies to the various processes of the indigo style.

In "spirit colours," the chief advances have been made (in 1855) by Mr. Robert King and Mr. G. Burch. Pigment printing has also received an increased impetus since 1851, chiefly from the introduction of the aniline colours, and the consequent employment of animal and coagulated mordants. Mr. Lightfoot's tannate of gelatine process has many merits in this way, for the coal tar colours.

Dr. Calvert concluded his paper by remarking upon the wonderful commercial progress made by the art of calico printing. In 1830 about 2,000,000 pieces were printed. The number of pieces exported, according to Mr. Potter, amounted in 1851 to 6,465,000, and in 1857 to no less than 27,000,000.

The Chairman having invited discussion upon the very important and striking paper which Dr. Crace Calvert had just read,

Mr. Wentworth Scott said, in relation to aniline-red, it was, he believed, the general impression that it could only be produced by the action of oxydising agents upon aniline, its formation by other means having scarcely been ever hinted at. He begged, however, to exhibit a specimen of the colour produced by heating aniline (without the intervention of any oxydising agent at all), mixed with pure sand and pieces of pumice-stone, in a sealed tube, for several days, and under a slight pressure. The temperature employed was about 220° F., but the process, although curious in a scientific point of view, was of no practical value, being very uncertain. On the subject of naphthaline colours he felt himself more at home, having devoted his attention to them for a lengthened period. About six years ago—viz., in 1856—he attempted to form alizarine artificially from binitro-naphthaline, and obtained a brilliant red-colouring matter, which, early in 1857, he showed to his friends, Dr. B. W. Richardson and Dr. J. Forbes Watson. This red colour (which he provisionally named "dianthine," from the carnation tint of its alcoholic solution) was obtained by a process differing in many respects from the more recent one of M. Roussin. He considered that some hydrocarbon, like sulphonaphthalic acid, or naphthaline itself, should be added to the acid solution of binitro-naphthaline, as well as a reddening agent, and that M. Roussin employed too high a temperature. Mr. Scott further showed several specimens of wool dyed at Huddersfield with madder, and his own imitations of the same produced with "dianthine." He considered that more than one substance resulted from the process just mentioned, and thought he had detected in the dianthine, by means of the microscope, crystals of true alizarine. Dianthine, when treated by certain oxydising agents, and afterwards with alkalies, such as ammonia, in the presence of alcohol, afforded another colour of a scarlet tint, a weak solution of which he begged to hand round for inspection. These colours, he remarked in conclusion, would be practically tested in the course of the present year.

Mr. Thomas Winkworth expressed a hope that Dr. Calvert would, at a later period, favour the Society with another paper on the subject he had only been able to treat of partly that night for want of a sufficiency of time.

Mr. George Wallis would mention an interesting fact in connection with the question. Mr. Rummey, of Manchester, had, at his suggestion, undertaken to illustrate the chemistry of calico-printing at the forthcoming Exhibition by a complete series of specimens of the substances used. He remarked that there were many ingenious labour-saving machines for calico-printing he had seen in operation in America, a description of which might well be laid before the people of this country. He had not seen any in use in England, but might at some future time bring the subject under the notice of the Society.

Mr. Jones gave the old story about *Lo kao* and the Roman Catholic Missionary, and mentioned the value of the article as being about 5s. per ounce.

Mr. Quin announced himself superintendent of the chemical department of the International Exhibition, and could bear his testimony as to what might be expected from Mr. Rummey's series of specimens.

The Chairman said he was sure they were all indebted to Dr. Calvert for having brought the subject before them, and also to Mr. Wentworth Scott for his practical remarks. He hoped that gentleman would be led to continue his researches. He (Mr. Scott) had gone to the extent of obtaining what he proposed to call "Dianthine," and had got results which led him to think he had also formed alizarine. If this substance could be obtained at all, it could be had in any quantity when once the right method of preparing it was known. He hoped Mr. Wallis would be induced to give them, from his large experiences, some insight into the American machinery he had referred to, on a future occasion; and that Mr. Winkworth's suggestion would not be lost sight of. He, in conclusion, begged to propose a vote of thanks to Dr. Calvert for his able and interesting paper.

The vote of thanks having been cordially given, Dr. Crace Calvert made a suitable reply, and the meeting then adjourned.

CHEMICAL SOCIETY.

Thursday, April 3, 1862

Dr. A. W. Hofmann, *F.R.S., President, in the Chair.*

Dr. H Debus gave a discourse "*On the Influence of the Quantitative Method in the Development of Scientific Chemistry.*" He stated that Lavoisier was the first to introduce this method of investigation, and that the great effect produced by it would be apparent on examining the state of the science before his time. Very different notions then prevailed as to the chemical constitution of substances; metals were considered to be compound bodies, composed of their oxides and a substance called phlogiston, the oxide being considered as an elementary body; sulphuric acid was considered an element, and sulphur a compound of sulphuric acid and phlogiston. The word element at that time had nearly the same meaning as it has at present, namely, a body obtained by chemical

decomposition, the real elements of Nature being a matter of speculation only, and not to be discovered by chemistry; so that sulphuric acid was not considered a real element or substance that could not be further decomposed. Combustion was considered as an analysis; and it was worthy of remark that the history of the views entertained as to the nature of combustion might be considered as the history of the science. A combustible substance such as wood was supposed to consist of volatile matter and ash, the volatile matter and ash being together equal in weight to the wood; the fire observed during the combustion being some escaping matter. The oxides of metals were called ashes, from their supposed analogy to the ashes of wood. It was noticed that some substances burnt better than others, which was attributed to their containing some substance which escaped during combustion, and to this substance various names were given, such as sulphur, oil, fat, &c. It was afterwards suggested that in all combustible bodies it was one and the same principle that escaped, and to this the name of phlogiston was given. Charcoal was supposed to be very rich in phlogiston, so that when heated with an oxide it gave up some of its phlogiston, which combined with the oxide and produced a metal. There were two great facts which did not agree with this theory, one of which was that air was necessary for combustion; to explain this it was supposed that air acted merely as a receiver of the matter set free, or as a solvent. The other objection was that metals become heavier when burnt, which was believed to be due to the loss of the heavenly fire; very little attention was paid to the increase of weight, for weight was considered not an essential property of matter. The discovery of the decomposition of oxide of mercury into mercury and oxygen by heat was another blow to the theory, for there was no substance present to impart phlogiston to the oxide. At the same time it was discovered that phosphorus and sulphur increased in weight when burnt, and that air was absorbed. The quantitative method of research being now adopted, the progress of the science was rapid. Lavoisier and others determined the composition of air, of various acids, and of water. Other questions then arose as to the quantitative composition of bodies; some supposing that substances might combine in any proportion unless physical causes interfered, while others, on the contrary, imagined that they combined indefinite proportions. Dalton's atomic theory was supposed to bear out this latter view. As soon as it was discovered that substances combined in definite proportions, the energies of chemists were directed towards determining these proportions. The next question was as to the manner in which the elements were arranged in compound bodies, and this was still a matter of dispute, various views being entertained on the subject according to the various ways in which substances could be decomposed, the composition of the body being expressed by a rational formula; but it was doubtful what these rational formulæ were considered to express, as they merely showed the manner in which the body behaved under the action of certain reagents, these rational formulæ being almost indefinite in number, and it was impossible to unite them into one. On the whole, it would appear from a comparison of the present mode of research with the ancient, that the difference consisted in the quantitative nature of the present method; the ancients merely noticing the qualitative appearance of substances under different circumstances. It had been remarked that in every branch of science there is only as much true science as there is mathematics; and this had proved to be the case with chemistry, for since the introduction of quantitative research the advance had been much more rapid than before.

The PRESIDENT remarked that the ancient phlogistic theory conveyed both a lesson and a caution; a lesson because it showed the value of a theory, and a caution not to rely too much on any theory, however it may appear to suit the wants of the time.

Dr. FRANKLAND considered that we might learn from it the great value of a theory even if false, for it served as a spur to investigation; with regard to the types to which the constitution of bodies should be referred, he would urge the necessity of chemists coming to some definite understanding.

NOTICES OF BOOKS.

Metallurgy. By JOHN PERCY, M.D., F.R.S. John Murray, Albemarle Street.

[THIRD NOTICE.]

IN the chapter entitled "Historical Notices on Copper-smelting in Great Britain," the author remarks, that "it would be difficult to select in this country a more eligible site for copper-smelting works than Swansea, and this for two reasons. The first is, that it is a good seaport, which is only at a short distance from Cornwall and Devonshire, the two counties in which the greatest amount of copper-ore is raised, and it is also easily accessible to vessels conveying ore, or products containing copper, from South America, Australia, and other parts of the world. The second is, that extensive collieries exist in the immediate vicinity, from which an abundant supply of coal can be obtained at a low price."

Dr. Percy gives a very faithful pictorial representation of Swansea, with its dense cloud of copper-smoke overhanging the town and stretching far out to sea,—a feature in the landscape which enables the site to be recognized from the opposite coast of Devonshire at a distance of nearly thirty miles. In reference to this practical difficulty, the author quaintly remarks, that "the inhabitants of Swansea generally seem to be habituated to the inhalation of the smoke, and to submit to the evil, if evil it be regarded, with unmurmuring resignation." Renewed efforts have of late been directed to the suppression of these noxious vapours, and to the recovery of the sulphur, arsenic, and even copper itself, which escape from the chimney-stacks in the ordinary method of roasting the ore. We are not informed whether any of these suggestions have as yet been practically carried out; if not, the subject is one well worthy of earnest investigation.

It will be impossible in this place to follow the author through all the intricate details of copper-smelting as practised in Liverpool, South Wales, Sweden, Saxony, Chili, and even in India and Japan. These processes are very fully described, and illustrated by numerous sketches of furnaces, implements, &c., drawn to scale, and accompanied by particulars of actual dimensions. The chemical changes which occur in this series of operations are explained by frequent reference to tables of analyses of the crude and refined copper, and the several varieties of regulus and slag. From these statements it appears that the amount of copper retained by the slag when thrown upon the refuse heap should never exceed ·5 per cent.; that the "coarse metal" loses gradually both sulphur and iron in all the stages of purification, giving rise to products distinguished according to their colour and other physical characters by the names of "red-metal" "blue-metal," "sparkle-metal," "white-metal," &c., and in which the proportion of copper is therefore progressively increasing. From the richer kinds of regulus the pure metal is eventually separated in the form of "pimple" or "blister copper," with simultaneous production of a dark-red slag containing often as much as thirty per cent. of copper, chiefly in the state of suboxide.

Great interest must be felt in the efforts made from time to time with the object of curtailing the old and somewhat tedious method of copper-smelting. Some few pages are occupied with the description of the "wet processes" of

Messrs. Bankhart and others, which are especially worthy of consideration from the circumstance that they aimed at employing an entirely new principle in the reduction of copper. According to the terms of Mr. Frederick Bankhart's patent (dated August 7, 1845), the finely-ground copper pyrites, or other sulphuretted ore, was spread out upon the bed of a roasting-furnace so constructed that the smoke and products of combustion from the fuel were not allowed access to the charge of ore, which was, on the contrary, subjected throughout the process of roasting to the full oxidising influence of a current of air. Under these circumstances the sulphide of copper was converted into sulphate, which could afterwards be extracted by washing with water. The solution obtained in this manner was reduced to metallic copper by the action of scrap iron, and the precipitated metal collected, fused, and cast into ingots. This process was, however, ultimately abandoned by the inventors, who are understood to be working on the old plan at the present time.

In the case of a high-priced metal like copper, it is of the greatest importance that an accurate method should be adopted in the analytical examination of its ores and products. Great care is bestowed upon the grinding and mixing of the ore, so as to produce a uniform heap, from which samples are taken and assayed before the lot is offered for sale. The interests of the miner and smelter are here somewhat opposed, and it has become the practice not only to allow a margin to cover loss during the transportation of the ore, but for the smelters to require that the assay shall be performed and the valuation based upon the result obtained by following a conventional process of analysis, viz., the Cornish method of dry assay. Dr. Percy communicates some very interesting particulars relative to the mode of conducting the sales "by ticketings," a system of purchase by auction, according to which the smelters hand in a sealed packet, or tender-sheet, naming the terms of their offer, when the lot is passed to the highest bidder. The associated copper-smelters employ assayers who act in concert, compare their analytical results, and agree upon a uniform list of products, which in most cases is kept secret, and is supposed to guide their employers in bidding for the ores. The author manages to give the kind of information contained in the copper-ore circulars of Swansea and Redruth; and (at page 490) inserts a table of comparative results by the Cornish and wet modes of assay, revealing the error from loss to which the old system in common use is liable.

A few instances of these discrepancies, extracted from the table, are appended:—

Nature of the Ore, &c.	Dry assay. Cornish method. Sellers' produces.	Associated Smelters' products.	Wet assay. Per-centage of copper.
Copper pyrites, quartz, &c.	25½	25¼	26·30
Ditto ditto . .	12⅜	13	14·15
Ditto ditto . .	12¼	12¼	13·97
Ditto ditto . .	9½	9⅜	10·97
Ditto ditto . .	36½	35½	38·59
Spanish grey copper ore .	2½	2⅞	3·46
British regulus . .	16⅜	15¾	18·42
Ditto, ditto . . .	34	34	37·27
Australian regulus . .	64⅜	62	67·41

The loss of copper experienced in following the Cornish method of assay is sufficiently apparent from these results to warrant the statement, that however much skill and judgment are brought to bear in its performance, it is far less expeditious and satisfactory than either of the modes of liquid-testing afterwards to be described.

(To be continued.)

Lithium in Meteoric Stones.—Spectrum analysis has shown Bunsen (*Ann. der Chem. und Pharm.*, bd. cxx. p. 253) that lithium must be added to the list of elements found in stones of meteoric origin.

NOTICES OF PATENTS.

1328. *Candles.* M. D'Albytre, Paris. Dated May 27, 1861.

In carrying out this invention the patentee melts a quantity of tallow by a steam heat over a water bath, or, by preference, over an open fire. When melted he adds essence of spikenard, and at the same time a small proportion of precipitated alumina, which latter ingredient is intended to aid in the separation of colouring and suspended impurities. He next adds a concentrated solution of chloride of zinc, and continues the application of heat with frequent stirring. A small quantity of chloride of lead and dry chloride of zinc are finally added, and the mass kept fused for at least an hour. When the materials have become very dark coloured, and the action is judged to be complete, the melted fat is poured into water and the product employed in the manufacture of candles, which are said to be very superior in quality, having the consistence and transparency of white wax.

By the action of chloride of zinc upon tallow, products are obtained which have a much higher fusing point than the original fat, and are for this reason better adapted for candle-making. In employing the volatile chloride of lead, there is, however, a possibility of noxious metallic fumes making their escape during the combustion of the candle,

1273. *Obtaining Electric Currents for Telegraphic Purposes.* D. G. Fitzgerald, Cambridge Street, London. Dated May 18, 1861. (Not proceeded with.)

In arranging a voltaic series for the purpose of producing an electric current for telegraphic signals, the inventor uses the earth as one of the battery cells in the combination. He, therefore, imbeds in the earth, at suitable distances, the positive and negative plates, forming a voltaic couple, which may consist of the same metals, and be excited in a similar manner with each battery cell of the series. Porous diaphragms may be used in the construction of the earth battery; and, with regard to the mode of making connection, one of the poles is placed in contact with the line wire, the other pole being united with the appropriate terminal of an ordinary galvanic battery, and the circuit is otherwise completed in the usual manner. The patentee occasionally employs an earth couple in combination with galvanic batteries at both stations, instead of one terminus only.

1274. *Batteries for Producing Voltaic Electricity, together with certain Metallic Products.* D. G. Fitzgerald, Cambridge Street, London. Dated May 18, 1861. (Not proceeded with.)

The inventor constructs a voltaic arrangement of the plate of iron, and a metallic salt, the acid of which has a greater power of affinity for the iron than for the metal of the salt. The metallic salt is therefore reduced, the metal being precipitated and forming a product of commercial value. In order to confine the deposit to one of the elements only in each separate couple, the remaining plate or element is surrounded with a porous diaphragm, and this plate excited, if preferred, with a mineral acid. The negative plate in any arrangement is that on which the deposit of metal is formed; and a series of pairs may be placed in communication precisely as in the case of the ordinary forms of voltaic batteries.

The guiding principle in the construction and action of this battery is very similar to that employed in the arrangements of Professor Daniell and later experimenters. The use of iron as the positive element appears, at first sight, to have an advantage over zinc on the score of economy; but it must be remembered that this metal cannot be amalgamated, and that, whether the circuit is open or closed, the iron is constantly undergoing solution and evolving hydrogen, thus giving rise to local action on that

plate, which goes far to counterbalance the consideration of a lower cost.

1278. *Electric Telegraph Apparatus.* W. Clark, Chancery Lane, London. Dated May 18, 1861.

By means of clock-work mechanism, governed and regulated by electricity, the inventor has succeeded in making the ordinary writing telegraph apparatus answer the double purpose of manipulator, or sending instrument, and that upon which the message is received.

1329. *Electric Telegraph Cables or Ropes.* C. S. Duncan, Kildare Terrace, Bayswater. Dated May 27, 1861.

The patentee claims the application of cane in strips, sections, or fibres, or the natural cane bored out in the manner described, to electric telegraph wires or conductors of every description. Secondly, the application of cane twisted together with hemp, coir, or other suitable fibre. Thirdly, the use of the same materials in conjunction with iron or steel wire. Further claims have reference to the manner of making joints, loading the cable, and other details of mechanical construction.

Cane is distinguished among the several vegetable structures of its class, by possessing many valuable qualities which render it particularly suitable for the application in question. Its comparatively low cost, rigidity and indestructibility, combined with the known insulating properties of its exterior silicious coating, may be enumerated as constituting some of its most prominent features and advantages. It might even be improved, we think, by treatment with tar or fatty matters, which filling the interstices, would render it impervious to moisture, and most effectually protect it from decay.

1336. *Coke.* P. A. Millward. Wednesbury. Dated May 28, 1861. (Not proceeded with.)

This invention refers to the production of coke from coal or slack of any description, but particularly from the non-bituminous kinds, such as the "Thick Coal" of South Staffordshire, the "New Mine," and similar varieties. The coal to be converted requires in some cases to be reduced to powder, it is then charged into retorts, which are closed as nearly air-tight as possible, and in this state exposed to heat in ovens of suitable construction, or they may be placed in the tunnel-heads or flues of any furnaces, so that they may be roasted for a period of time varying with the quality of the coal and the weight of the charge.

Such an expedient must undoubtedly be attended with the dangerous consequences of generating gas and steam in closed vessels; there is no question, however, that in the event of the retort being strong enough to withstand the internal pressure, the conditions for securing the production of an adherent coke from these non-caking coals will have been in every way fulfilled.

1338. *Matches.* R. M. Letchford, Old Montague Street, London. Dated May 29, 1861.

This invention has for its object the production of matches at a low cost, free from sulphur and from objectionable odour. It consists in dipping the ends of the wooden splints into melted paraffin, or in saturating them with paraffin oil, prior to their being tipped with either of the ordinary lighting compositions. The patentee prefers to use solid paraffin oil alone for this purpose, but he claims also the employment of stearin or Japan wax, in combination with paraffin.

It would not appear to be an easy task to prepare matches free from objectionable odour by the use of any ordinary sample of paraffin oil. Stearin has been long and extensively employed in match-making as a substitute for sulphur, and although solid paraffin may be equally efficient, it is more costly, and burns with a smoky flame.

1345 *Refining and Purifying Iron, and Converting the same into Steel.* W. E. Newton, Chancery Lane, London. A Communication. Dated May 29, 1861. (Not proceeded with.)

The iron at the moment of production in the blast furnace, or by a subsequent treatment in a smelting furnace, is purified in the following manner: A charge of fuel is thrown into the furnace, and, when sufficiently ignited, a quantity of zinc ore, or the metal itself, is placed upon it; then the iron ore is charged upon the zinc, these materials being alternately supplied to the furnace as the process of reduction is accomplished. In order to increase the temperature of the blast, it is recommended to admit jets of steam along with the air through the tuyères; and to the combined action of zinc and steam is attributed the efficacy of the process in removing the impurities from the iron. Cast iron may be refined by subsequently melting in a reverberatory furnace and treating with zinc and steam. With slight modification a kind of puddled steel may be produced as the result of this treatment.

Incrustations due to the volatilisation of oxide of zinc have been noticed as occurring accidentally during the reduction of certain kinds of iron ore in the Dowlais District. Zinc has usually been considered to exert a pernicious influence upon the quality of the pig; and its ore, blende, contains so large a proportion of sulphur that it cannot be deemed a desirable substance to introduce freely into the blast furnace. By itself, steam is known to have a beneficial action upon the melted iron, and to assist in removing both sulphur and silicium.

Notices to Proceed.

445. James Paterson, Middle Temple, London, "Improvements in means or apparatus for re-burning animal charcoal."—A communication from George Alexander Drummond, Montreal, Canada East.

455. James Paterson, Middle Temple, London, "Improvements in the use of animal charcoal."—A communication from George Alexander Drummond, Montreal, Canada East.—Petition recorded 20th Febrmary, 1862.

587. Bridge Standen, Salford, near Manchester, "Improvements in the preparation or manufacture of portable manure or fertilising compound, and in the collection or extraction therefrom of a certain liquid applicable to various purposes, and also in machinery or apparatus to be employed therein."

CORRESPONDENCE.

Detection of Carbonic Oxide in Air.

To the Editor of the Chemical News.

Sir,—Is there any method (not eudiometrical) of proving the existence of CO in the air? A gas stove here, used to warm the dressing-room of a racquet-court, produces a sudden giddiness in many; yet I believe it to be due not to CO_2, but to CO. Does it discolour Cu_2Cl?

I am, &c. W.

Rugby.

Utilisation of Sewage.

To the Editor of the Chemical News.

Sir,—In one of your late issues you refer in a leading article to the Committee about to sit on the "Sewage of Towns Question," and that now is the time for the best plan. We believe that it is the good fortune of this Company to possess it. Certain we are that we can keep our drains clear, and our streams and rivers pure, better than by any other method yet discovered, and perhaps I might add to be yet discovered by our great men, for our plan allows no human excreta, rubbish, or ashes to enter the drains, but converts them at once into a manure powder,

which is proving itself every day a better and safer fertiliser than guano.

We shall be happy to show you the system if you are ever in this neighbourhood. Most probably we shall give evidence before Dr. Brady's Committee.—I am, &c.

Thomas A. Hanson, Chairman.

Sanitary Manure Company, Manchester, March 26, 1862.

Crystallised Gold.

To the Editor of the Chemical News.

Sir,—In reference to Dr. Phipson's note on Crystallised Platinum, Professor Daubrée has just presented to the Academy of Sciences at Paris, a specimen of crystallised gold from California. The crystals have concave surfaces, striped parallel to the sides. Similar indications of crystallisation have been observed on gold from the Ural Mountains and New Grenada.—I am, &c.

C. J. Vondel.

Amsterdam.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Presence of Phosphoric Acid in Primitive Rocks.—Schiel has confirmed (*Silliman's Journal*, vol. xxxi. p. 353) the discovery of the lamented Professor Fownes, in proving the presence of phosphoric acid in the earliest formations. Schiel found the acid in some trachytic rock on the banks of the Sacramento.

Manganese in the Scoria from Old Copper Workings.—M. Terreil has analysed several specimens of scoria from old copper workings in the Island of Cyprus, and has found in them a large proportion (30 per cent.) of sesquioxide of manganese. (*Comptes-Rendus*, t. liii. p. 1275.) He calls the attention of metallurgists to this fact, thinking that perhaps the ancient smelters may have found manganese useful in smelting copper pyrites.

Action of Iodine on Tin.—M Personne has proved (*Comptes-Rendus*, t. liv. p. 216,) that when equivalent weights of tin react on each other, only biniodide of tin is formed, half the metal remaining unacted on. The proto-iodide, he says, is never formed when the two bodies act directly on each other. Proto-iodide of tin is only formed by dissolving the metal in a concentrated solution of hydriodic acid, or by double decomposition. The proto-iodide combines with oxide of tin in various proportions to form oxido-iodides. The action of iodine on tin is, therefore, exactly similar to the action of bromine and chlorine on the same metal.

II. ORGANIC CHEMISTRY.

Action of Sodium Amalgam on Oxide of Ethylene.—M. Wurz (*Comptes-Rendus*, t. liv. p. 280) placed an aqueous solution of oxide of ethylene with sodium amalgam in a freezing mixture. The next day he distilled the alkaline liquid formed, and discovered the presence of alcohol produced by the direct addition of hydrogen to the oxide of ethylene. $C_2H_4O + H_2 = C_2H_6O$. The liquid from which the alcohol was distilled contained glycol and poly-ethylenic alcohols. The paper from which we extract this fact also contains an account of the action of bromine on oxide of ethylene. M. Wurz mentions another fact which illustrates the basic properties of the oxide. When equal volumes of hydrochloric acid gas and the oxide are mixed, the gases disappear, and a hydrochlorate of the oxide or chlorhydric glycol is formed. M. Wurz has tried to re-convert aldehyde into alcohol by the direct addition of hydrogen, but has not succeeded.

Acetate of Cyanogen.—Schutzenberger thinks he has produced (*Comptes-Rendus*, t. liv. p. 154) acetate of cyanogen by acting on cyanate of silver by chloride of acetyle. In the reaction, which was very energetic, several bodies appear to have been formed, and although the author did not succeed in getting the acetate pure, he described its properties to be the following:—Colourless liquid, with a strong irritating odour, boiling about 80°C., decomposed by water, alcohol, and heat, and burning with a purple flame.

III. TECHNICAL CHEMISTRY.

Revivification of Animal Charcoal.—MM. Leblay and Cuisinier give (*Comptes-Rendus*, t. liv. p. 270) a new process for reviving exhausted animal charcoal. They find that the power of absorbing colouring matter is restored on treating the charcoal with weak boiling solution of caustic alkalies. They also say that the original absorbing power of the charcoal may be very much increased by pouring over it a weak solution of biphosphate of lime.

IV. ANALYTICAL CHEMISTRY.

Estimation of Carbon in Iron.—E. Mulder has published a large work on the estimation of carbon in iron, in which, after reviewing all the other processes for estimating carbon, and showing them to be more or less defective, he gives the following as the best for the purpose. A long combustion tube of the hardest possible glass is drawn out at the end and plugged with asbestos. The tube is then filled two-thirds full of sand which has been ignited in oxygen. The iron filings, previously washed with sulphuric acid and then mixed with pumice which has been ignited in oxygen, are now placed in the tube, then a layer of oxide of copper, and lastly a plug of asbestos. A chloride of calcium tube is then connected, also a tube with peroxide of lead to retain sulphurous acid, then a drying tube with sulphuric acid and pumice, and lastly the potash apparatus. A stream of oxygen is then slowly passed, and the tube is heated first gently and then as strongly as possible. The success of the experiment seems to depend a good deal on having enough sand. When his tube was only half full, the author only obtained 4·42 per cent. of carbon. When two-thirds full, he obtained 5·02 per cent.—*Chem. Centralblatt*, 1862, s. 46.

ANSWERS TO CORRESPONDENTS.

*** All *Editorial Communications* are to be addressed to the Editor; and *Advertisements* and *Business Communications* to the Publisher at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

*** In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

A Reader.—We do not know the gentleman's address, but should think a letter addressed to him, care of the Secretary of the Chemical Society, Paris, would find him.

J. D.—The paper is prepared by mixing lard with lamp black into a stiff paste; rubbing it over writing paper with flannel, and wiping off the superfluous quantity with a soft rag.

Druggists' Show Bottles.—A Constant Subscriber will find all the information he requires on this subject in our Second Volume, page 275.

Bleaching Beeswax.—Enquirer.—Pour the melted wax in a divided state on a revolving cylinder, partly immersed in water, so as to form it into fine ribbons; these are exposed to air and moisture till bleached, and subsequently refined by melting in water containing sulphuric acid.

Soluble Glass.—C. Eades—Mix 10 parts of carbonate of potash, 15 parts of powdered quartz, and 1 part of charcoal. Fuse well together. The mass is soluble in 4 or 5 parts of boiling water, and the filtered solution, evaporated to dryness, yields a transparent glass permanent in the air.

P. T.—We regret we cannot furnish the desired information. You had better apply to Mr. Condy, the patentee.

Strychnine.—Can any correspondent inform H. C. W. of the best method of extracting strychnine from nux vomica?

J. J.—Declined with thanks.

Mulder on the Assay of Silver.—The entire work, translated from the original Dutch, by Dr. A. Adriani, has been in our hands for a considerable time. It is of considerable value, and we regret that circumstances will oblige us to discontinue the publication for the present.

Errata.—Page 177, 2nd col., line 9, instead of "334," *read* "634"; also, page 195, line 6 from bottom, for "*tartaric*" read "*carbonic.*"

THE CHEMICAL NEWS.

VOL. V. No. 124.—April 19, 1862.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Further Remarks on the Preparation of the Ethyl Bases by means of Nitrate of Ethyl, and their Separation, by M. CAREY LEA, *Philadelphia.*

FREQUENT repetition of the process which I have previously described, confirms me in my opinion of its advantages. Nitrate of ethyl is obtained easily and abundantly. From 480 grammes of alcohol of 40° Baumé, I have obtained 231 of nitrate of ethyl, and even a still larger proportion. It is essential in operating upon quantities of half a litre or over, of the mixture of alcohol and nitric acid, to first raise the alcohol to a boiling heat and dissolve the urea in it, and then add the nitric acid, otherwise it may attack the alcohol while the urea remains undissolved at the bottom of the vessel, and thus cause the whole process to fail. I have prepared six or seven pounds of nitrate of ethyl in this way with very little trouble.

In acting upon the nitrate of ethyl with ammonia, it is necessary that the pressure tubes should be extremely strong, and should never be more than one-half full. Saline baths are not to be used.

Triethylamine appears to be only an occasional product of this reaction.

In employing the process which I have recommended for removing the ammonia, viz., converting the mixed bases into sulphates and exhausting with absolute alcohol, it does not answer well to add sulphuric acid to the crude products obtained from the pressure tubes, because it is impossible to know the exact quantity of sulphuric acid required to expel the nitric acid. If too little be employed, some nitrate of ammonia might remain and dissolve in the alcohol. If too much, bisulphate of ammonia may be formed, and this likewise would dissolve to some extent in the alcohol and contaminate the product. It is therefore necessary to distil the crude products with caustic alkali, neutralise exactly with SO_3, and then evaporate to dryness and exhaust with alcohol, as described in the paper here referred to.

In employing picric acid to separate the ethyl bases, each picrate, after having been purified by recrystallisations, is to be treated with chlorhydric acid. The greater part of the picric acid separates, and a further portion by evaporating the liquid. Some, however, remains; and it is important to get rid of it, because picric acid, when distilled with aqueous caustic alkali, evolves ammonia, which would thus contaminate the ethyl base. This is effected as follows: After the chlorhydrate of the ethyl base has been evaporated as far as possible without crystallisation, a very little carbonate of potash is to be added, and the solution stirred at intervals. Almost every trace of picric acid is thus precipitated.

In the final distillation of the chlorhydrate with caustic alkali, if there remains any trace of picric acid, an infinitesimal quantity is apt to be carried over, imparting a yellow tinge to the distillate. This may be prevented by adding before distillation a very small quantity of animal charcoal.—*American Journal of Science.*

PHARMACY, TOXICOLOGY, &c.

On the Behaviour of Essential Oils to Iodine and Bromine, by JOHN M. MAISCH.

(Concluded from page 190.)

Oleum Serpylli.—Pale yellow, nearly colourless, thin.

Iodine produces radiating motion with the oil from the fresh herb, which evolves few yellowish red vapours; with oil from the dried herb, the reaction was stronger, more heat and more vapours were given out; the residue has an extractive consistence. Z.

A brisk radiating reaction takes place, the solution has an iodine colour, and is miscible with a dark sediment.

Ether sol. bromine.—Some spreading, the solution is yellowish red; after six hours, brownish yellow.

Ether sol. iodine.—White vapours; each drop is immediately decolorised; the oil gradually turns to a greenish colour.

Oleum Spicæ.—Old greenish yellow, oily consistence.

Iodine explodes with less force with this oil than with oil of lavender; it evolves less fumes, but more heat; the yellowish brown residue is of soft extractive consistence, and has a modified balsamic odour. Oil of second quality evolved few red fumes only by the influence of a larger proportion of iodine; less heat, no vapours, more fluid residue. Z.

It dissolves slowly by stirring, and has a tar-like sediment.

Ether sol. bromine mixes to a transparent, reddish yellow liquid.

Oleum Tanaceti.—Pale yellow, thin.

Iodine dissolves quietly without heat and with little radiating motion; the residue is liquid, of the consistency of a thin syrup and unaltered odour. Z.

On the introduction of the iodine, a brisk reaction takes place, with many grey and a few violet vapours; the solution is deep yellowish green, afterwards purely green, and is miscible with the remaining black sediment.

Ether sol. iodine.—The upper portion is of a yellow iodine colour; the lower stratum is thick, brown; miscible to a yellowish red iodine-coloured transparent liquid.

Ether sol. bromine.—The mixture is at first colourless, separates into a thin milky liquid and a heavier light brown oil.

I was not aware of the considerable variation of the result of the above experiment with iodine, from the results of Zeller, Liebig, and Tuchen, until I was collecting my notes for this essay. I knew that the oil employed by me was several years old, but as it had preserved its physical qualities, I was satisfied about its purity; particularly the odour was purely that of tansy, without any admixture. Zeller states, alcohol of ·85 dissolves the oil in all proportions; that which I experimented with, required three parts of alcohol for complete solution at 70° F. The reactions, as stated by me, seem to coincide with an adulteration with turpentine; but, judging from all physical properties, I am still of the opinion that the oil is pure, somewhat modified by age. Could age produce such a difference, or is this the effect of our climate? I shall have to inquire into this matter.

Oleum Valerianæ.—Light brownish yellow, rather thickish.

Iodine dissolves with little heat to a dark yellow brown mass of extractive consistence; some genuine oils evolve a few greyish yellow vapours. Z.

The solution is effected with very slight radiating movement; the liquid part is yellowish brown, the sediment nearly black; without stirring, the whole assumes gradually a pitchy consistence.

Ether sol. iodine is miscible with an iodine colour to two not well defined strata, which, by agitation, are easily miscible.

Ether sol. bromine.—Each drop produces a deep purplish black colouration; a separation into two strata takes place, the lighter one, being in the smallest quantity, assumes such a deep violet colour as to appear black; the heavier one is of a greenish black colour; around the margin of the whole liquid are reddish and bluish spots and streaks.

III.—The Sulphuretted and Nitrogenated Oils.

Oleum Amygdalæ amaræ.—One colourless, another one brownish, pale.

Iodine.—No reaction takes place in the cold or by moderate heating; a little iodine dissolves with brown yellowish red colouration and without thickening. Z.

My experience with both oils coincides with this observation.

Ether sol. iodine mixes quietly and readily to a deep iodine-coloured liquid; the whole of the liquid is spreading out, working up the sides of the vessel a thick black margin; in six hours the whole has run together again, and forms a dark brown coloured mass of the consistence of honey.

Ether sol. bromine dissolves without reaction; after favouring the evaporation of the ether by a current of air, a separation takes place into a deeper and a lighter red-coloured liquid, with slight cloudiness on the dividing line of both.

Oleum Sinapis.—Light yellow.

Iodine dissolves quietly without external reaction to a brownish yellow liquid, which is hardly thickened. Z.

It forms easily a perfect solution, with scarcely any radiating motion; the colour is a red iodine.

Ether sol. iodine.—Miscible without any peculiar reaction to a reddish yellow iodine-coloured liquid.

Ether sol. bromine.—Miscible, colourless, subsequently slightly milk white.

Remarks on the Course of Observation.—Above I have described the manner in which the experiments were performed, by giving directions for preparing my test-liquids, &c. I have also given the mode of Zeller's procedure with iodine, which, as far as quantity is concerned, I have carefully followed. But one great difference in the mode of our observation is, that Zeller applies a moderate heat to the thicker oil, so as to render the fluidity of all about alike, while I have taken the opposite course, by making the experiments at as nearly as possible alike temperatures. Necessarily, a difference in the degree of the reaction of iodine must arise, and by comparing the energy of the reaction of the more viscid oils, such a difference, slight and unimportant though it may be, will be noticed, the most vigorous reaction being obtained by Zeller's method.

In the subsequent course of observing the changes, I have also followed another suggestion. Zeller, after the first reaction is over, stirs, by means of a glass rod, the undissolved iodine together with the liquid. It appeared to me more important, to notice not only the reaction of the iodine on the oil, but likewise that of the oil upon the iodine. This is the reason that, in my observations, we often meet with expressions like "dark extract," "deep brown pitchy sediment," &c.; this sediment is generally occasioned by the undissolved iodine, which, on the point of its contact with the oil, is often changed to a resinous mass which envelopes the unused iodine, preventing its farther action on other particles of the oil. This is another cause of the difference between our observations; but my course appeared to me to be the better one, as, though the energy of the reaction may be partly lost, more time is gained for observing the intermediate changes, while the final results will, in most cases, be nearly alike, although from the greater amount of heat, necessarily developed by agitating the two reacting substances, a farther going change might reasonably be expected.

The Poisonous Effects of Carbonic Oxide, by H. Letheby, *M.B., M.A., Ph.D., &c., Professor of Chemistry and Toxicology in the Medical College of the London Hospital.*

A good deal of misapprehension seems to prevail respecting the poisonous action of carbonic oxide. On the one hand, accidents have somewhat recently occurred where in all probability the effects of the gas were entirely overlooked; and on the other, fatal consequences have been unjustly attributed to it. Some of this confusion is due to the circumstance that our standard works on poisons and medical jurisprudence have either omitted the subject entirely, or have discussed it in very meagre language. The recent catastrophe at the Hartley colliery, and the remarks which have been published respecting the supposed influence of the gas in causing the death of the men, have created an opportunity for a re-examination of this question.

Carbonic oxide was discovered by Priestley long before the close of the last century; and in 1802, Clement and Desormes, at the instance of Guyton Morveau, undertook a careful examination of its properties. They not only proved its chemical nature, but they also ascertained that it was a poisonous gas. Birds put into it dropped dead before they could be taken out; and when the experimenters themselves attempted to breathe it, they were attacked with giddiness and faintness. This experiment was repeated by Sir Humphrey Davy in 1810, who says that when he took three inspirations of it, mixed with about one-fourth of common air, the effect was a temporary loss of sensation, which was succeeded by giddiness, sickness, acute pains in different parts of

the body, and extreme debility; some days elapsed before he entirely recovered. It is, he says, fatal to animal life.

About the same time, the researches of Nysten demonstrated that the gas was capable of producing great disturbance of the system when injected into the veins; and although he concluded that the effects were of a mechanical nature, yet the accounts which he has given prove that the gas is a dangerous poison.

Later still, in 1814, the two assistants of Mr. Higgins, of Dublin, made experiments with it upon themselves, and in one case, that of Mr. Wilter, with almost a fatal result. Having exhausted the lungs of air, he inhaled the pure gas three or four times, and was suddenly deprived of sense and volition; he fell upon the floor, and continued in a state of perfect insensibility, resembling apoplexy, and with a pulse nearly extinct. Various restorative means were employed, but without success, until they resorted to the use of oxygen, which was forced into his lungs, and then his life was restored; but he was affected with convulsive agitation of the body for the rest of the day. He suffered also from violent headache, stupor, and a quick, irregular pulse. Even after mental recovery he suffered from giddiness, blindness, nausea, alternate heats and chills, and irresistible sleep. The other gentleman, after inhaling the gas two or three times, was seized with giddiness, tremor, and incipient insensibility. These effects were followed by languor, weakness, and headache of some hours' duration.

Since those experiments were made, others of a more extended character were instituted by Tourdes and by Leblanc. Tourdes found that rabbits were killed in seven minutes when they were put into a mixture of one part of the gas with seven of atmospheric air. A fifteenth part of the gas in common air killed them in twenty-three minutes; and a thirtieth part in thirty-seven minutes. Leblanc's experiments were made in conjunction with Dumas, and he ascertained that one per cent. of the gas in atmospheric air would kill a small dog in a minute and a-half, and that birds were killed immediately in a mixture containing five per cent. of it.

Very recently I have myself ascertained that air containing only 0·5 per cent. of the gas will kill small birds in about three minutes; and that a mixture containing one per cent. of the gas will kill in about half this time. An atmosphere having two per cent. of the gas will render a guinea-pig insensible in two minutes; and in all these cases the effects are the same. The animals show no sign of pain; they fall insensible, and either die at once with a slight flutter,—hardly amounting to convulsion,—or they gradually sleep away as if in profound coma. The post-mortem appearances are not very striking: the blood is a little redder than usual, the auricles are somewhat gorged with blood, and the brain is a little congested. In birds there is nearly always effusion of blood in the brain, and it may be seen through the transparent calvaria by merely stripping off the scalp after death.

Accident has also demonstrated how injurious the gas is even to the human subject. For many years past attempts have been made to promote the use of water-gas as an agent of illumination. The gas sometimes contains as much as thirty-four per cent. of carbonic oxide. It is obtained by passing steam over red-hot charcoal; and as the steam is decomposed by the ignited carbon, the hydrogen is set free, and carbonic oxide, with carbonic acid, is produced. Patents for this process of manufacturing gas date as far back as the year 1810, and they have at various times been put into operation in this country and on the Continent. Sellique, in 1840, obtained permission to use the gas in the towns of Dijon, Strasburg, Antwerp, and two of the faubourgs of Paris and Lyons. At Strasburg an accident occurred which put a stop to its use. The gas escaped from the pipes into a baker's shop, and was fatal to several persons; and not long after an aëronaut, named Delcourt, incautiously used the gas for inflating his balloon. He was made insensible in the car, and those who approached the balloon to give him assistance fainted and fell likewise. The use of the gas has, therefore, been interdicted on the Continent.

Another source of danger from it is in the combustion of carbon. It is found in the neighbourhood of brick-kilns and furnaces. The gases discharged from the latter contain it in large proportion. Iron furnaces produce it to the extent of from twenty-five to thirty-two per cent., and copper furnaces from thirteen to nineteen per cent. In the year 1846, M. Adrien Chenol was anxious to ascertain the properties of the gases yielded by his process of smelting zinc ores with carbon; and, not having a suitable instrument for collecting the gases, he attempted to draw them out of the furnace by means of a pipette. Some of the gas was thus inhaled, and he fell immediately, as if he had been stunned; the eyes were turned back in the orbits, the skin was discoloured, the veins were swollen, and presented a black tint under the skin; there were violent pains in the chest, and the brain felt powerfully oppressed. After removal to the open air, and the application of restoratives, sensibility gradually returned, but the internal pains were still severe, and there was a feeling of suffocation. For several days he felt depressed and languid; the digestion was bad; sleep was obstinate and heavy, and it was frequently disturbed by cramps in the knees and toes. Even for months afterwards there was a morbidly excited state of the nervous system.

In a more diluted condition the gas is still able to exert an injurious action, and it is very probable that the singular catastrophe which happened at Clayton Moor, near Whitehaven, in the summer of 1857, was caused by the diffusion into the air of carbonic oxide from the neighbouring iron furnaces. There is a row of cottages near to these furnaces where, in the month of June, 1857, a number of persons were suddenly seized with insensibility, which soon passed, in some cases, into coma and death. About thirty persons were thus attacked, and six of them died. The effects were attributed at the time to the escape of sulphuretted hydrogen from the slag on which the cottages were built; but it is more probable they were caused by the oxide of carbon from the furnaces.

Lastly, it is worthy of remark that very recently Boussingault has noticed that the leaves of aquatic plants give off carbonic oxide and marsh gas when under the influence of solar light; and he asks whether this gas so produced may not be concerned in the unhealthiness of marsh districts.

A more complete acquaintance with the effects of this poison is a great desideratum, although enough is known to indicate its general mode of action, and to furnish evidence for its discovery.—*Lancet.*

Detection of Glucose Mixed with Cane Sugar.—Schmidt gives the following process (*Annal. der Chem. und Pharm.*, bd. cxiv. s. 102): Add to the suspected solution subacetate of lead and liquor ammonia. The white precipitate which is formed changes to red when heated if a very small amount of grape sugar is present.

PHYSICAL SCIENCE.

On Spectrum Analysis, by W. A. MILLER, *M.D., LL.D., V.P.R.S., Professor of Chemistry, King's College, London.*

(*Continued from page* 203.)

4. Spectra of Coloured Flames.—The first person who seems to have examined a coloured flame by means of the prism was Sir David Brewster, who proposed, as a source of achromatic light, the flame of diluted alcohol, which he found gave a fine homogeneous yellow, with faint traces of green and blue. In the same volume of the *Edinburgh Phil. Trans.*, 1822, at p, 455, Sir J. Herschel describes briefly the spectra of muriate of strontia, muriate of lime, chloride and nitrate of copper, and boracic acid. The same observer, in the article, "Light," *Encycl. Metrop.*, 1827, p. 438, says :—"Salts of soda give a copious and purely homogeneous yellow, of potash a beautiful pale violet." And he there gives a general statement of the results with the salts of lime, strontia, lithia, baryta, copper, and iron. He further continues:—"Of all salts the muriates succeed best, from their volatility. The same colours are exhibited also when any of the salts in questiou are put in powder into the wick of a spirit-lamp. If, however, salt be used, Mr. Talbot has shown that the light is an absolutely homogeneous yellow. . . . The colours thus communicated by the different bases to flame afford, in many cases, a ready and neat way of detecting extremely minute quantities of them. . . . The pure earths, when violently heated, as has recently been practised by Lieutenant Drummond, by directing on small spheres of them the flames of several spirit-lamps, urged by oxygen gas, yield from their surfaces lights of extraordinary splendour, which, when examined by prismatic analysis, are found to possess the peculiar definite rays in excess which characterise the tints of flames coloured by them, so that there can be no doubt that these tints arise from the molecules of the colouring matter reduced to vapour, and held in a state of violent ignition."

The analysis of the spectra of artificial lights was resumed by Mr. Fox Talbot, who published a paper in 1826, in vol. v. of "Brewster's Journal of Science." He there describes a method of obtaining a yellow monochromatic light by the use of an ordinary spirit-lamp, with a cotton wick fed with dilute alcohol holding common salt in solution. He found the same effect, whether muriate, sulphate, or carbonate of soda were employed.

Nitrate, sulphate, chlorate, and carbonate of potash, agreed in giving a bluish-white tinge to the flame. By burning a mixture of nitre and sulphur, he observed a red ray, of low but definite refrangibility, which he regarded as characteristic of the salts of potash, as the yellow ray is of the salts of soda. He concludes his paper with the following observation, which follows some remarks upon some experiments of Herschel's :—"If this opinion should be correct and applicable to the other definite rays, a glance at the prismatic spectrum of a flame may show it to contain substances which it would otherwise require a laborious chemical analysis to effect."

In the *Phil. Mag.* for 1834, vol. iv. p. 114, Mr. Talbot further showed how, notwithstanding the similarity in colour of the light of lithia and strontia, they can at once be distinguished by means of the prism. He says the strontia flame exhibits a great number of red rays, well separated from each other by dark intervals, not to mention an orange and a very definite bright blue ray. The lithia exhibits one single red ray. Hence I hesitate not to say that optical analysis can distinguish the minutest portions of these two substances from each other with as much certainty, if not more, than any other known method. He then describes the spectra produced by the flame of cyanogen.

In a paper in vol ix. of the same journal, p. 3, 1836, Mr. Talbot again adverts to the importance of the study of coloured flames, and describes the spectra of the salts of copper, of boracic acid, and nitrate of baryta, and corroborates, by independent experiment, the observations of Wheatstone on the electric spark.

The spectra of coloured flames were further examined, in 1845, by myself, and an account of these experiments was given in a paper read that year before the Chemical Section of the British Association at Cambridge. (*Phil. Mag.*, xxvii. 81.

An alcohol lamp, fed with a solution of the compound, the flame of which was to be examined, and a common wick supported in a small glass tube, furnished the flame. The lamp was placed opposite the vertical slit, through which diffused daylight could be transmitted at pleasure. Fraunhofer's lines thus served as points of comparison of the different flames. The paper was illustrated by coloured lithographs of various spectra, the first that were published, including those of chloride of copper, boracic acid, nitrate of strontia, chloride of calcium, and chloride of barium, in minute detail.

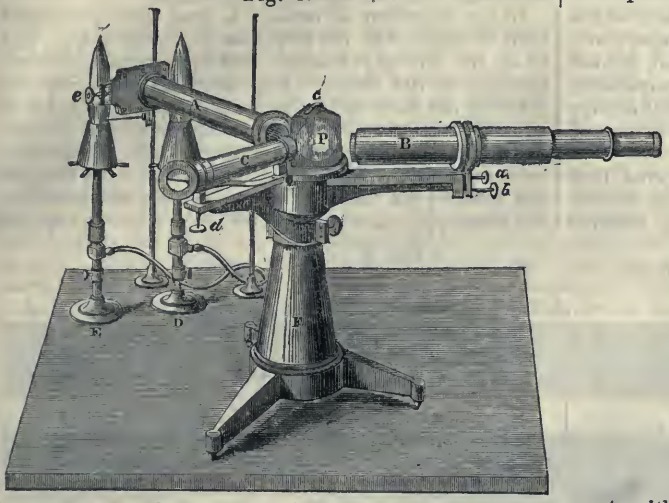
Numerous other spectra were also described, including those of the chloride of sodium, manganese, and mercury, and of a large number of other metals. The paper concludes with the observation :—"It may be interesting to remark, in connection with the speculations on the absorptive action of the sun's atmosphere, that if solar light be transmitted through a flame exhibiting well-marked black lines, these lines re-appear in the compound spectrum, provided the light of day be not too intense compared with that of the coloured flame. This may be seen in the red light of the nitrate of strontia, and less perfectly in the green of the chloride of copper. It would, therefore, appear that luminous atmospheres exist in which not only certain rays are wanting, but which exercise a positive absorptive influence on other lights."

The next paper of importance upon the prismatic analysis of artificial lights was published by Mr. Swan, in 1857. He, as early as the year 1847 (*Edinburgh Phil. Trans.*), in examining the double refraction of Iceland spar, pointed out the convenience of employing the principle of the collimator, which enables the slit through which the light is allowed to fall upon the prism to be brought up to within a very short distance of the prism itself. And in 1857 (*Edin. Phil. Trans.*, vol xxi. p. 411), in the course of an elaborate investigation of the prismatic spectra of the flames of compounds of carbon and hydrogen, he indicated the extreme delicacy of the reaction for sodium. The bright yellow line characteristic of this metal he found could be produced by a quantity of a solution of common salt, which did not contain more than $\frac{1}{2500000}$th of a grain of sodium.

But it is to Kirchhoff and Bunsen (*Poggendorff's Annal.*, cx. p. 161) that we are indebted for reducing the prismatic observation of flame tinged by the salts of different metals to a simple and systematic method of analysis for the alkalies and alkaline earths; and they have contrived an apparatus, of comparatively simple construction, by which the different spectra may be conveniently examined, and compared with one another. Fig. 1 exhibits the instrument in its most complete form (*Poggendorff's Annal.*, cxiii. 374). It is an improvement

on the instrument used by Swan and by Masson, including a scale for ascertaining the position of the lines in different cases, as well as a reflecting prism, by which

Fig. 1.



two spectra can be compared side by side. P represents a flint-glass prism, supported on the cast-iron tripod F, and retained in its place by the spring *c*. At the end of the tube A, nearest the prism, is a lens, placed at the distance of its focus for parallel rays from a vertical slit at the other end of the tube. The width of the slit can be regulated by means of the screw *e*. One-half of this slit is covered by a small rectangular prism, designed to reflect the rays proceeding from the source of light D down the axis of the tube, whilst the rays from the source of light E pass directly down the tube. By this arrangement, the observer stationed at the end of the telescope B is able to compare the spectra of both lights, which are seen one above the other, and he can at once decide whether their lines coincide or differ. *a* and *b* are screws for adjusting the axis of the telescope, so as to bring any part of the slit at *e* into the centre of the field of vision. The telescope, as well as the tube C, is movable in a horizontal plane, around the axis of the tripod. The tube C contains a lens at the end next to the prism, and at the other end is a scale divided into millimètres. When the telescope has been properly adjusted to the examination of the spectrum, the tube C is moved until it is placed at such an angle with the telescope and the face of the prism, that when a light is transmitted through the scale the image of this scale is reflected into the telescope from the face of the prism nearest the observer. This image is rendered perfectly distinct by pushing in the tube which holds the scale nearer to the lens in C, or withdrawing it to a greater distance, as may be required. The lines of the scale can then be employed for reading off the position of the bright or dark lines of the spectrum, as both will appear simultaneously, overlapping each other, in the field of the telescope. By turning the tube C round upon the axis of the tripod, any particular line of the scale can be brought to coincidence with any desired line of the spectrum. Stray light is excluded by covering the stand, the prism, and the ends of the tubes adjoining it with a loose black cloth.

The extraordinary delicacy of certain of these spectrum reactions was indicated by Swan, who measured it for sodium by the only accurate method,—namely, by dissolving a weighed quantity of the salt in a known quantity of water, and he thus determined with precision the limit of the reaction. Bunsen and Kirchhoff attempted to estimate the sensitiveness of the reaction by deflagrating a given weight of the various salts in the room in which they were experimenting, and diffusing the vapour mechanically through the air, increasing the quantity of the salt until a gas flame showed the reaction of the peculiar metal, due to particles in suspension. But it is obvious that this method does not admit of precision, and is liable to lead to an exaggerated estimate of the delicacy of the reaction, from the impossibility of ensuring uniformity in the diffusion of the salt.

The sodium reaction is the most sensitive of all; and so extensively is common salt diffused, that scarcely any flame can be obtained in which the indication of soda is absent.

Having observed the position of the bright lines produced by introducing into the flame of a Bunsen gas-burner the salts of the various alkalies and alkaline earths, each of which had been purified for these experiments with great care, they constructed a chart in which the different lines were laid down for each, and were able, by observing the position of the lines obtained when a mixture of various salts was introduced into the flame, to ascertain the presence of these different bases with sufficient readiness to use the method for the purposes of qualitative analysis. The rapidity with which the result is obtained by a practised observer, and the minuteness of the quantity required for the examination, give this method a superiority over any other now in use for the qualitative analysis for the alkalies and alkaline earths; moreover, the circumstance that the mere inspection of a source of light furnishes information respecting the composition of the bodies undergoing combustion or volatilisation, extends the mode of inquiry over distances limited only by the distance through which the object is visible; we are thus furnished with a method of analysis which is applicable to the luminous atmosphere of the sun, the stars, as well as to the light of the planetary bodies. This circumstance invests the subject with an interest like that which attends the employment of the telescope; at the same time the minuteness of its search enables it to reveal, like the microscope, quantities of matter indefinitely small.

This minuteness in its scrutiny has already, in the hands of Bunsen and Kirchhoff, led to the discovery that many bodies, such as lithia and strontia, formerly supposed to be rare, are really widely distributed in minute quantities. It also led them to discover the two new alkalies, *cæsia* and *rubidia*,—the first named from *cæsius*, "sky-coloured," in allusion to two characteristic blue lines in its spectrum; the second from *rubidus*, "dark red," owing to the existence in its spectrum of two red lines of remarkably low refrangibility.

These bases were found in the course of an examination by the prism of the residue of the mother-liquor from the Durkheim spring, when the occurrence of these hitherto unobserved lines induced Bunsen to make a minute chemical examination of the water which furnished them. The inquiry showed that in every ton of the original water about three grains of chloride of cæsium, and rather less than four grains of chloride of rubidium, were present. These two salts so closely

resemble chloride of potassium in properties, that it would have been impossible to have ascertained their existence in the minute proportion in which they occur but for the method of spectrum analysis.*

Kirchhoff and Bunsen ignited many of the salts of the different metals in flames of very varying temperature, including those of sulphur, bisulphide of carbon, diluted alcohol, carbonic oxide, hydrogen, and the oxyhydrogen mixture, and they found that the same metal always produced the same lines whatever flame they employed to heat it.

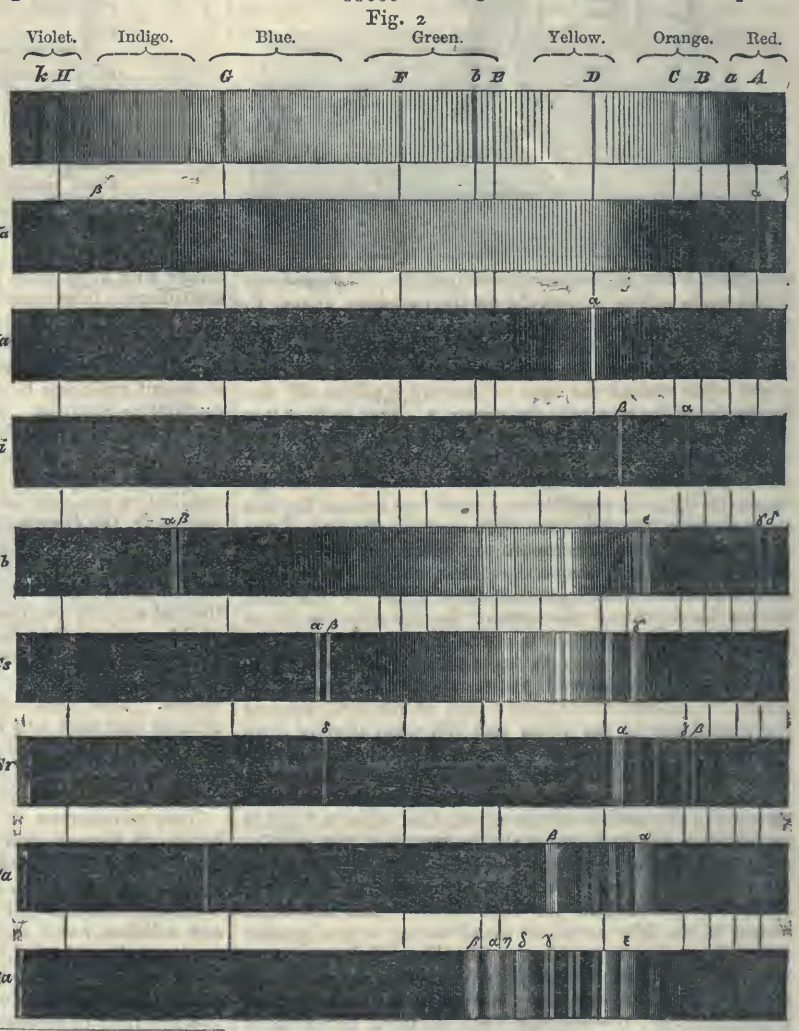
They do not, however, appear to have observed that as the temperature rises, a new series of bands become visible in certain cases. The spectrum of chloride of lithium, in the flame of a Bunsen burner, gives but a single intense crimson line; in a hotter flame, as that of hydrogen, it gives an additional orange ray; and in the oxyhydrogen jet, or the voltaic arc, a broad, brilliant blue band comes out in addition. A similar effect is perceived in the case of metallic iron and other metals when heated by the voltaic arc, which at elevated temperatures furnish much more complicated spectra than when less intensely heated.

The first specimen, shown in Fig. 2, exhibits some of the fixed dark lines of the solar spectrum contrasted with the position of some of the most important bright lines furnished by the spectra of the alkalies and alkaline earths, when their salts are heaped upon a loop of platinum wire introduced into the flame of a Bunsen gas-burner. The characteristic lines in each case are distinguished by the letters of the Greek alphabet, the most strongly marked lines being those indicated by the earliest letter.

Amongst the various spectra those of the alkalies are the simplest. In the potassium spectrum the most characteristic bands are the red band, Ka α, and violet band, Ka β. A copious diffused light fills up the central portion of the spectrum. In the sodium spectrum nearly the whole of the light is concentrated on the intense yellow band Na α. In the lithium spectrum a crimson band, Li α, is the prominent line; Li β is seldom visible; but at the elevated temperature of the voltaic arc an additional blue line becomes very intense.

In the spectrum of cæsium a good deal of diffused light is visible, but the two lines in the blue, Cs α and Cs β, are strongly marked, and may be seen when a quantity of the chloride not exceeding $\frac{1}{170000}$th of a grain of the pure salt is used, or $\frac{1}{7000}$th of a grain if diluted with fifteen hundred times its weight of chloride of lithium. Rubidium is not distinguishable in quantities quite so minute. The lines Rb α and Rb β, in the red, and Rb γ in the blue, are almost equally characteristic, but about $\frac{1}{33000}$th of a grain of the chloride is required

Fig. 2



to render them visible. The spectra of the alkaline earths are equally definite though more complicated.

The salts which are most readily volatilised, such as the chlorides, bromides, and iodides of the different metals, give the most brilliant spectra. But it is only in the case of the alkalies and the alkaline earths that the spectra thus obtained are characteristic. Where the spectra of the other metals are required, recourse must be had to Wheatstone's method of taking electric sparks, between wires consisting of the metal of which the

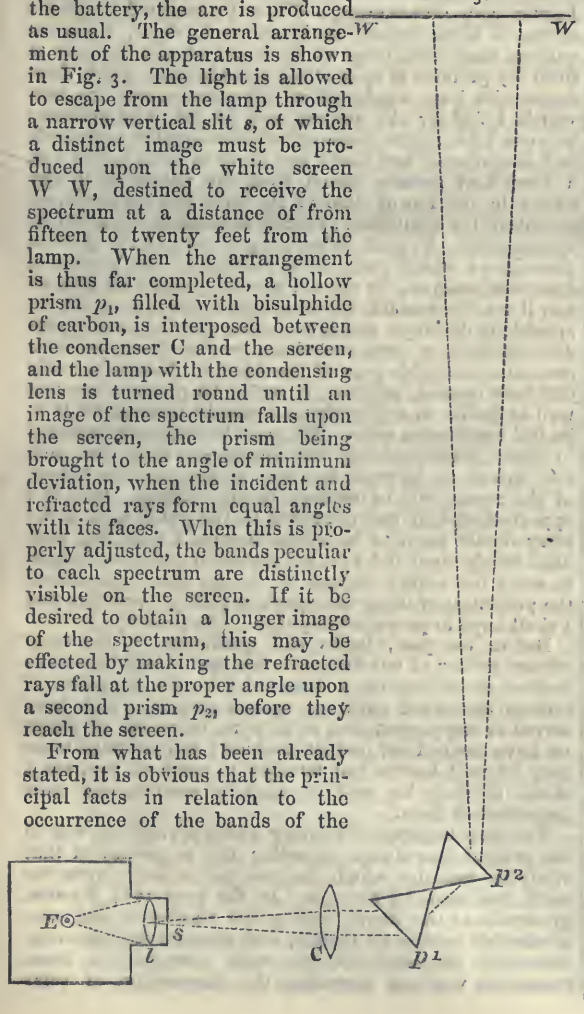
* Bunsen and Kirchhoff found their method of separating potassium from rubidium and cæsium upon the difference in solubility of the double salts which the chlorides of potassium, rubidium, and cæsium form with bichloride of platinum. Boiling water takes up 5·18 per cent. of the potassium salt ($KCl,PtCl_2$), 0·634 of the rubidium salt ($RbCl,PtCl_2$), and only 0·377 of the cæsium salt ($CsCl,PtCl_2$). The cæsia and rubidia may be separated by taking advantage of the solubility of carbonate of cæsia in absolute alcohol, carbonate of rubidia not being soluble in this menstruum. The hydrates of potash, rubidia, and cæsia are all deliquescent, and absorb carbonic acid from the air; they give crystalline precipitates with tartaric acid, and an opalescent precipitate with fluosilicic acid. The chemical equivalents of the three metals are as follows:—Potassium, 39·1; rubidium, 85.36; cæsium, 123·35.

spectrum is required, and the electric sparks may conveniently be procured by the employment of Ruhmkorff's coil. The temperature obtained in this way is very intense, and developes lines not produced by the heat of ordinary flames.

If photographs of these spectra be taken, I have found that the impression obtained in atmospheric air or in nitrogen is nearly the same whatever metal be employed; but if the spark be passed in hydrogen, or in carbonic acid, all the extra violet lines disappear, proving that these lines of the photograph were due not to the metal, but to the nitrogen of the air.

When it is desired to render the lines produced by the spectra from different metals visible to a large audience, they may be shown by the employment of the voltaic battery. About forty pairs of Grove's construction will answer well. The wires of the battery must be connected with the carbon electrodes of a Duboscq's electric lamp. The metals to be burned are supported upon the lower or positive electrode made of the hard carbon deposited in the gas retorts; and when the salts of the metals are to be employed, two or three vertical holes are to be drilled into the upper end of the charcoal point, and into these the salt for experiment is introduced. On completing the connection with the battery, the arc is produced as usual. The general arrangement of the apparatus is shown in Fig. 3. The light is allowed to escape from the lamp through a narrow vertical slit *s*, of which a distinct image must be produced upon the white screen W W, destined to receive the spectrum at a distance of from fifteen to twenty feet from the lamp. When the arrangement is thus far completed, a hollow prism p_1, filled with bisulphide of carbon, is interposed between the condenser C and the screen, and the lamp with the condensing lens is turned round until an image of the spectrum falls upon the screen, the prism being brought to the angle of minimum deviation, when the incident and refracted rays form equal angles with its faces. When this is properly adjusted, the bands peculiar to each spectrum are distinctly visible on the screen. If it be desired to obtain a longer image of the spectrum, this may be effected by making the refracted rays fall at the proper angle upon a second prism p_2, before they reach the screen.

Fig. 3.



From what has been already stated, it is obvious that the principal facts in relation to the occurrence of the bands of the spectrum were known before Kirchhoff and Bunsen directed their attention to the subject. But it has been invested with a new interest by the discovery of the new bases cæsia and rubidia, and particularly by a theory of Kirchhoff's which embraces and generalises the greater number of the phenomena, though it does not account for all of them. This theory we shall now consider.

It is to be remembered that the spectrum produced by the ignition of a solid or of a liquid always yields a continuous band of light, containing rays of all degrees of refrangibility within the range of its two extremes; but the same body, when converted into vapour, may produce a luminous atmosphere which may emit light of certain definite refrangibilities only, so as to produce a spectrum consisting of a series of bright bands of particular colours, separated from each other by intervals more or less completely dark. Bearing these facts in mind, the theory proposed to account for Fraunhofer's lines will be readily understood.

In 1858, Mr. Balfour Stewart published in the *Edinburgh Phil. Trans.*, vol. xxii., a paper on the law of exchanges in radiant heat, and in the following year the subject was taken up by Kirchhoff, who arrived at the same conclusions as Mr. Stewart, independently; and the German philosopher extended his theory to the phenomena of light as well as those of heat. The conclusion at which he arrived may be thus stated,—That when any substance is heated or is rendered luminous, rays of a certain and definite degree of refrangibility are given out by it; whilst the same substance has also the power of absorbing rays of this identical refrangibility.

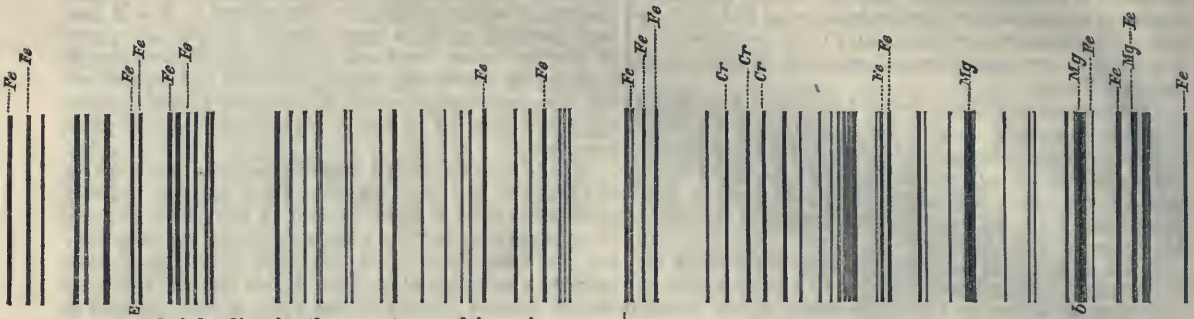
Sodium, for example, when ignited, emits an intensely brilliant yellow light, which is concentrated into two closely contiguous bands or bright lines coincident in position with Fraunhofer's double black line D in the solar spectrum. But if through the flame coloured by sodium, the more powerful light of the charcoal points or ignited lime be transmitted, the continuous spectrum due to this stronger source of light is interrupted by a *black* line coincident with the solar black line D. Kirchhoff has also ascertained that certain of the bright bands in the spectra of potassium, lithium, barium, and strontium, may in like manner be reversed, and I have found that some of the strongest lines in the blue in the spectrum of copper may be similarly reversed.

Now, Kirchhoff has applied these facts to the explanation of Fraunhofer's dark lines. He supposes that in the luminous atmosphere of the sun the vapours of various metals are present, each of which would give its characteristic system of bright lines; but behind this incandescent atmosphere containing metallic vapour is the still more intensely heated solid nucleus of the sun, which emits a brilliant continuous spectrum containing rays of all degrees of refrangibility. When the light of this intensely heated nucleus is transmitted through the incandescent photosphere of the sun, the bright lines which would be produced by the photosphere *are reversed*, and Fraunhofer's black lines are only the reversed bright lines which would be visible if the intensely heated nucleus were no longer there.

Kirchhoff has proceeded to test this theory by submitting the solar spectrum to a still more minute investigation than Fraunhofer, or even than Brewster and Gladstone, who have recently mapped more than two thousand of the dark lines. (*Phil. Trans.* 1860.)

The annexed diagram shows a small portion of Kirchhoff's detailed drawing, including the part of the spectrum extending from E to *b* (Fig. 4), and he states that

Fig. 4.



for every bright line in the spectrum of iron is a corresponding black line in the solar spectrum. Seventy such lines occur between D and F, and in the small portion contained in the figure there are seventeen such lines indicated by the mark *Fe*. The strong lines near *b*, marked *Mg*, coincide with the brilliant green lines in the spectrum of magnesium. Chromium and nickel also give less distinctly marked lines, indicated in the case of chromium by the letters *Cr*.

Kirchhoff, from these and other more extended observations, draws the conclusion that in the atmosphere of the sun the vapours of sodium, potassium, magnesium, iron, chromium, and nickel are present; but that lithium, aluminum, zinc, cobalt, copper, and silver are not present.

Fascinating as this theory is, it must be remembered that it is yet upon its trial, and that it does not explain the facts at present known respecting the vapours of hydrogen, mercury, chlorine, bromine, iodine, and nitrogen. M. Morren even questions the accuracy of some of Kirchhoff's observations. Thus, he states that in a measurement which he made of the red band of potassium, conjointly with Plücker, they found that it did not correspond with the solar line A, but that it is considerably more refrangible. (Chemical News, Dec. 7, p. 302.)

The present occasion, however, is not the most appropriate for discussing these and other points which still require careful experimental elucidation; yet it seems to be not unfitting at the present moment to put some ardent minds upon their guard; for it appears by some to have been hastily assumed, in a spirit of self-confidence, that we already have the key to everything upon this subject. A possible way to new truth has indeed been opened: here, doubtless, as in all other cases where we are permitted to obtain a further glimpse into the machinery of the universe, we shall but see fresh proofs of exhaustless wisdom on the part of the great Author of nature; and instead of boasting of our triumphs, we shall, if we pursue our researches in a right spirit, be taught a lesson of reverent humility as we are allowed to raise a fresh corner of the veil which separates that which is known to man from the infinitely greater portion which still lies beyond.

Royal Institution of Great Britain. — General Monthly Meeting.—Monday, April 7, 1861. — William Pole, Esq., M.A., F.R.S., Treasurer and Vice-President, in the Chair. Alfred Denison, Esq.; Alexander Staveley Hill, Esq., D.C.L.; William Martin, Esq.; Daniel George Rees, Esq.; Augustus Thorne, Esq., F.R.G.S.; John Tyndall, Esq., F.R.S., were elected Members of the Royal Institution. Jonathan S. Crowley, Esq.; Major-General Charles J. Green; Rev. G. Musgrave Musgrave; A. C. B. Neill, M.D.; Sir Joshua Rowe; E. H. Sieveking, Esq., were admitted Members.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Three Lectures on Spectrum Analysis, by Dr. H. E. Roscoe, *Professor of Chemistry in Owens College, Manchester.*

Lecture I. (*Saturday, March* 29, 1862.)

Ladies and Gentlemen,—When I consented to give a course of three lectures on Spectrum Analysis, it was not without some misgivings as to how I should be able to fulfil my promise in spite of the great interest which these discoveries have awakened in this country, and the love which I feel for the subject and for its chief authors; because I felt that a course of lectures should be systematic and educational, and that I was about to bring before you a branch of science which from the nature of things, owing to the recent date of its birth and the enormous extent of its application, cannot at present be perfectly systematised. If, then, in bringing before you the glorious phenomena which present themselves in the science of spectrum analysis, we are not able to explain all that we see, if we find remarkable exceptions to the laws which we are able to develope, and if we notice that the facts appear disconnected, or even sometimes contradictory, we must remember that this page of the book of Nature has only just been opened, and that it would be presumptuous as well as foolish to expect that we should understand it in its full bearings at once. We must be content to proceed in these matters slowly but surely, knowing that each step brings us forward, and that, at last, the true beauty of the scene will be evident to all. Hence, then, if I appear abrupt in the treatment of the subject, or if my story should seem to be discontinuous, I must beg your kind indulgence; and I for my part shall endeavour to bear in mind the saying quoted by Arago, that "clearness is the politeness of those who speak in public," and like him I shall hope to prove not unpolite. (Applause.)

Before we pass, ladies and gentlemen, to the chief subject matter of our discourse this day, it will be well to review those important discoveries in the branches of science connected with our special subject, which have served as stepping-stones, as it were, by means of which we have reached our present position; and although these may be well known and familiar to some of you, yet I think it may be well to slightly mention the most important of them.

The most important discovery which opened the way to our present knowledge regarding the properties of that vivifying radiance which the sun pours down upon the earth, was made by Newton. In the year 1675, Newton presented to the Royal Society his wonderful and ever memorable treatise on Optics; and amongst the numerous important discoveries which were contained in those researches was one regarding the composition of solar

light. Newton was the first to explain that the white solar light consisted of different coloured rays. He passed a beam of white solar light through a triangular piece of glass called a prism; and instead of the white point of light which he allowed to enter into his dark room, he saw on the opposite side of the wall a bright band, consisting of all the different colours, blending in beautiful harmony one with another, from red to violet, passing through all the intermediate stages of orange, green, and blue; and Newton showed—and this was the most important part, perhaps, of his discovery—that this splitting up of the white light by the prism could only be accomplished once; that a second prism did not split up this light again into another variety of coloured rays. He proved that if, for instance, the yellow light from the first spectrum be allowed to pass through an opening in a dark screen, and behind this screen another prism was placed, his yellow light was not again split up, but was shown on a further screen as yellow light. In fact, the second prism did nothing more than spread out again the light which was first obtained. Newton also showed that coloured light thus obtained, when brought together again, produced on the eye the sensation which we term whiteness. For this purpose he passed the beam of white light through the opening in a shutter, obtained the spectrum by means of a prism, and then, looking at this spectrum through a second prism, instead of observing the coloured band, observed a spot of white light, thus showing that the whole of these differently coloured rays, when brought again together by means of the prism, produce on the eye the sensation of white light.

That white light can be thus split up by means of the prism, I can easily show you by the help of the electric lamp which, in Dr. Tyndall's hands, reads you here so many beautiful lessons. Now, in order to explain this coloured spectrum which we get from all white light, Sir David Brewster supposed that the solar spectrum was made up of three super-imposed monochromatic spectra, each having a specific colour, and each having its point of greatest intensity at a different position in the spectrum. Thus, for instance, Brewster supposed that the different colours in the solar spectrum were produced by the super-imposition of a blue, yellow, and red spectrum. This, however, was proved by Helmholtz to be incorrect. Helmholtz proved that each particular ray has its particular colour, and that light of each degree of refrangibility is monochromatic; that we cannot separate, by any possible means, the green ray into a yellow ray and blue ray; that that ray is green and remains green, and is not made up of a mixture of yellow and blue. This he showed by experiment; and he likewise proved that the super-imposition of the yellow and the blue did not produce on the eye the effect of green.

Here we have the white electric light which is extremely intense—so much so that we can scarcely bear to look at it. By passing the beam of light through the two prisms which we have here, we split it up into differently coloured rays. This is the kind of effect which we observe, of course, with the white solar light. The differences between the white solar light and this light obained by the ignited carbon points, is the subject which I wish to bring before your attention shortly.

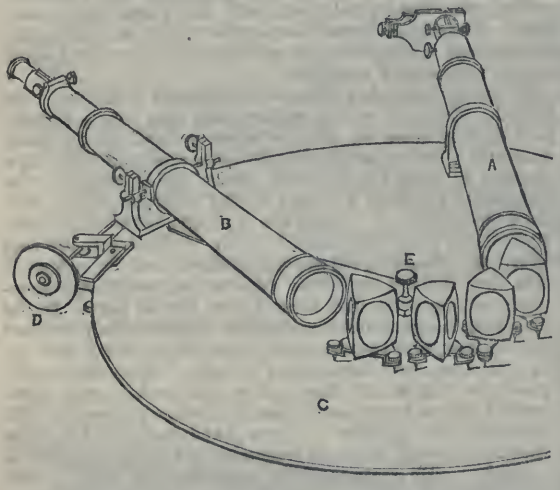
We see here that the red is the least refrangible portion of the spectrum; but we pass on through every gradation of red, yellow, green, and blue to the violet, or most refrangible rays. Now, just as we find rays possessing this peculiar colour—as we find a particular kind of green ray, a particular kind of red ray, and a particular kind of blue ray, so we likewise find that beyond the red and beyond the violet we have still rays present although they cease to affect the eye. This can be easily shown by placing a thermometer at the extremity of the visible red rays. We shall then see that beyond the red part which acts upon the eye we have really rays, because the thermometer rises; and further on, beyond the violet, we shall find that we have rays capable of producing another kind of action—capable of producing chemical action. Now, these heat-producing rays, and these chemically active rays, both of which exist in portions of the spectrum, invisible to the eye, do not essentially differ from the rays of light. There is no difference of kind between the heating rays, the chemically active rays, and the light-giving rays. We might suppose, and in fact it is the case, that I, for instance, was able to discern rays up to a particular point in the red, and my friend might be able to see further. The rays do not stop at a given interval where I cease to see them, it is only that my eye, or my retina, or my brain ceases at that point to be sensitive for them. The eye or the brain of some one else might probably see a great deal further. Therefore, we must dismiss from our minds any idea of an essential difference between the heat-giving, the light-producing, and the chemically active rays. They differ from one another only as the light-giving rays differ from one another, viz., in wave length and in intensity of vibration.

If we now examine the solar spectrum with care, we find that the light which we have just seen does not represent exactly what we observe when we look at the solar spectrum; for we notice that the solar spectrum contains a number of dark bands—dark bands which intersect the differently-coloured portions of the solar spectrum, and show the absence of certain kinds of rays in the solar light. These lines were first noticed by Dr. Wollaston in the year 1802. He observed only seven of them, and he supposed that they divided the seven various colours of the spectrum, as they were then termed, into special parts. In the year 1814 the great German optician, Fraunhofer, examined this subject, and re-discovered these lines, which now go by the name of "Fraunhofer's lines;" since he investigated the matter very thoroughly, and used a much more perfect optical instrument than Wollaston possessed. He measured the position of these fixed lines on the solar spectrum, which we must remember are always present in sunlight. He measured the position, and drew a very beautiful map of these fixed dark lines in the solar spectrum. I can give you a representation of Fraunhofer's drawing of the dark lines in the solar spectrum by means of a photographic reduction from his original drawing in the "Transactions of the Munich Academy" for 1815. This drawing is not coloured; it simply represents the different lines. Fraunhofer drew no less than 576 of these lines. He noted that the same lines always occur in the same portion of the solar spectrum; and he named these lines and measured their refractive indices; that is to say, their relative positions in the solar light. This line, which is a very important one,—with which we shall have a great deal to do in our future Lectures,—the line *D*, is in the orange; the line *b* is in the green; the line *E* is likewise in the green; the line *F* is just in the limit of green and blue, and the line *G* is in the blue; whilst the two double lines, which are called *H*, are in the violet, and are scarcely visible to the unassisted eye when looked at in the ordinary way. This curve on Fraunhofer's drawing shows the intensity of the light in the various portions of the spectrum.

Several physicists have examined this subject since Fraunhofer's time, and have drawn diagrams representing the lines in the solar spectrum. Amongst these, as the most recent, are those of Sir. David Brewster and Dr. Gladstone, of which I have here a diagram. Here you see a photographic reduction of the lines as drawn by Sir David Brewster and Dr. Gladstone. This top diagram represents the whole length of the solar spectrum; and you see that it is cut up into an enormous number of lines; several hundred certainly have been drawn in this way by Sir David Brewster and Dr. Gladstone. But the most complete drawing which we now possess of the lines in the solar spectrum are those recently made by Professor

Kirchhoff. These have been made by the help of a very perfect and a very beautiful apparatus, a representation of which I can show you.

The sunlight was allowed to enter through the fine vertical slit which was placed at the end of the tube *A*. This tube was fixed on a horizontal iron plate *c*, and an object-glass was placed at the end of the tube *A*, so as to render the solar rays passing through the slit parallel as they entered the first refracting surface of the prisms. These prisms were made of the most perfect workmanship by Steinheil, the celebrated optician of Munich, and were placed on this horizontal iron table *c* with very great care, so that their refracting surfaces were all exactly perpendicular to the surface of the plate. They were then placed at the angle of minimum deviation of the light that was about to be observed, and arranged so that the



beam of white light on passing through these prisms was refracted or bent out so far that the whole length of the spectrum, drawn upon the scale which Kirchhoff saw in this instance, would be twenty feet. Kirchhoff, looking through the eye-piece of the telescope *B*, saw what he describes to be—and I can, from my own personal knowledge, bear witness to it—one of the most magnificent sights that he ever beheld; and he obtained in this way the lines with a degree of distinctness and a degree of accuracy which certainly had never been attained before. By means of a micrometer screw *D*, attached to this telescope, it can be moved round the axis *e*, and the cross wires in the eye-piece can thus be brought from line to line. In this manner Kirchhoff measured in a portion of the spectrum the positions of the dark lines which it contains, going from line to line, just as a carpenter measures a room with a two-foot rule. Making his measurements in this way, Kirchhoff drew representations of what he saw, and I can show you on the screen a photographic reduction of these interesting drawings.

The first one which we will take is the one beginning with the line *D*, that line which, as I mentioned to you, is contained in the orange portion of the spectrum, and which we shall have to refer to again and again. There are the lines *D*. You will notice that it is no longer a single line, but that it is split up into a double dark line; and so beautiful was the instrument which Kirchhoff used, that he could notice between these two black lines another less distinct line, which, however, we see here. In order to obtain a mode of reference for the positions of the various dark lines thus seen, Kirchhoff drew a scale divided into millimètres, and under this scale he drew his dark lines, taking any one as an arbitrary starting-point. Now you will notice that these lines possess different degrees of blackness. This indicates the variety of shade or of distinctness which these lines exhibit in the spectrum. This effect Kirchhoff produced in his drawing by means of differently-shaded lines. He used six differently-tinted inks; and with these he drew lines of different thicknesses; so that in this photograph we have a very fair representation of the variation in intensity which these lines exhibit. I do not call your attention for the present to the lines which are drawn below; of these we shall have to speak more especially on a future occasion. It is with these upper set of lines that we have at present to do—the dark lines which occur in the solar spectrum. This portion of the spectrum you see here is placed under the other, in order to facilitate the impression; for these drawings are chromo-lithographs, if one may call them so, or tinto-lithographs; they were printed from Kirchhoff's own drawing, on six different stones, with six differently-shaded inks; and thus a chromo-lithograph with different shades of the same colour was obtained; and in order to facilitate the printing, instead of the spectrum being altogether in one line, it was drawn in two sheets, a quarter of that portion of the spectrum which had been examined being put in each length. We have, then, four lengths. Here you see two of them, and here you see the other two lengths. Now you notice that at the right hand corner, at the top, those two very dark lines are the lines to which I called your attention in Fraunhofer's diagram, *b*. The line in the middle of the lower half is the line *F* which we noticed occur in the portion just bordering on the blue.

These dark lines which I have now shown you, exhibit only one-third of the total length of the spectrum. I cannot look at these beautiful drawings, in which the more one looks at them, the more one finds to admire, without some feelings of regret, for I know at what cost to the talented observer these lines were marked and these beautiful drawings were made. By the constant and trying observation which the subject rendered necessary, Professor Kirchhoff has, I am sorry to say, permanently injured his eyesight, so that for the present, at any rate, he is perfectly unable to continue these glorious investigations which he has so well opened out.

A full description of Professor Kirchhoff's interesting researches, including the plates of the dark solar lines, has just been published by Messrs. Macmillan, of Henrietta Street.

Whilst we are on this subject, I wish to point out to you that these dark lines not only exist in the visible portions of the spectrum, but also exist in the portions which contain the heating rays and which contain the chemical rays. I cannot show you any of the lines which occur in the ultra-red portion of the spectrum; but I can show you those which occur in the ultra-violet. Thanks to the beautiful researches of Professor Stokes on fluorescence, these lines have become perfectly well known, and I can show you a diagram of some of the lines which occur in the ultra-violet portion of the spectrum. These lines, *H*, are those we noticed in Fraunhofer's diagram as being in the violet. When we allow the solar rays to pass through a quartz prism, we find that rays pass through to which glass is perfectly opaque. The diagram is a copy of the effect produced on a film of sensitive collodion which was exposed to the action of these ultra-violet rays passing through quartz prisms. The white spaces indicate the position in which there is no light; they are the Fraunhofer lines in the ultra-violet sunlight. You see here that the lines stretch out a long way beyond the visible portion of the spectrum, or that to which the eye is ordinarily sensitive.

I will call your attention now, ladies and gentlemen, to the portion of the spectrum on which Professor Kirchhoff has shown us those beautiful lines. This being a representation of the whole of the solar spectrum, those lines,

which on the drawings which lie on the table extend over a space of six feet, come from *E* in the yellow to *F* in the blue, so that you see that no more than one-third of the visible spectrum has as yet been mapped by Professor Kirchhoff. The rest will, no doubt, in time be done; but all these lines which we notice on this diagram are contained between the line *D* in the orange and the line *F* in the blue. It is very important for us to have a clear idea about these lines; because I shall have in a future Lecture to explain the cause of the existence of these dark lines in the solar spectrum, and we shall see in what way these rays give us a clue to the explanation of the chemical composition of the solar atmosphere. Fraunhofer first, in the year 1814, proved that these lines were contained in all solar light. He proved that they were contained in the light of the planets. He observed the spectrum caused by Venus light, and noticed that in the light of Venus these same dark lines occurred; he saw that the line *D* occurred; he saw *b* double; and on measuring the distances from *D* to *E* and from *E* to *F*, he found that the relative intervals between these lines were exactly the same in the light from Venus as in the light from the sun. He also examined the light which was given off from the brightest fixed stars—Sirius, for instance; and there he made this most important observation,—that in the Sirius-light, lines or bands were present which were not present in either direct sunlight, or the reflected planet-light. Hence he concluded that starlight differs in its qualities essentially from sunlight, and that these lines which we notice in the solar spectrum are, in some way or other, produced in the sun. This is a conclusion which has been confirmed by all subsequent observations.

If we now ask ourselves, in what way do bodies artificially give off light? we shall find that in almost all cases, except in the case of phosphorescent bodies, light is emitted only when the body becomes hot; that light is produced by high temperature. We all know that the higher the temperature the greater will be the *quantity* of light which is given off. We all know the difference between the black heat of the smith's forge and the white heat of the glass furnace. Let us now ask ourselves whether the *quality* of the light emitted by bodies at various temperatures is different. If I take this piece of platinum wire, for instance, and if I heat it gradually, as I can do, we shall see that first of all we have dark rays of heat given off. [The platinum wire was attached to a battery.] The platinum wire, which does not give us any light, will show us that it is hot by inflaming a small piece of phosphorus which we have here. We can thus prove that the platinum wire, although it does not visibly glow, is yet hot enough to ignite our piece of phosphorus. If we diminish the length of our heated wire, we shall thereby increase the heat, and we observe that the wire gradually becomes of a dull red heat; it gradually gets brighter and brighter until it at last becomes white hot. Now then, what is it that is going on in this platinum wire? It is this: first of all, at a certain given temperature the platinum wire gives off red rays, or it becomes red hot; at a higher temperature it begins to give off yellow rays, or it becomes yellow hot; and at a certain higher temperature it gives off blue rays, and becomes what we may call blue hot; and then at a still higher temperature violet rays are given off together with the red, together with the yellow, together with the blue; and we then observe that it is white hot. We then get that combination of colour which produces on our eye the sensation of whiteness, and we then say that the body is white hot.

That this is the case I hope to be able to show you. I have here three cylinders, one is filled with a blue liquid, one is filled with a yellow liquid, and the other is filled with a red liquid,—that is to say, the space between the outer and inner cylinders is filled with liquid. I have a hollow space here within which I can place a coloured flame. If I heat any substance in the interior of this coloured glass, that substance will become visible to my eye through the blue glass when the heated body gives off blue light. Blue light comes through this liquid, and no other kind of light can pass. That no red light and that no yellow light will pass through this I can easily show you. Here, for instance, I can make a yellow flame. I will place the lamp in the interior of this cylinder, and then if I place a little bit of common salt in the flame, we do not see the flame through the jar. Although this flame is glowing now with an intense degree of yellow light, we shall observe no light coming through. If I place a red flame in the same jar, we shall not be able to see that this flame is coloured at all red in this blue glass. The same holds good, then, for these other vessels. Here I have some yellow solution through which only the yellow light will pass. No blue light and no red light can pass through this cylinder; and here I will show you that we get no red light. The flame is now burning with a bright red flame in the cylinder, surrounded by the yellow solution, but nothing is seen but a slight yellow tinge. Through this cylinder, on the contrary, red light will pass, and only red light.

Now, if what I say is true, if the platinum wire, on being heated, first gives off red light, then gives off yellow light as well, and lastly gives off blue light, in addition to the other two, I shall be able to show you this fact by gradually heating up the wires. We will now put our wire in contact with the battery, and then, when the lights are turned down, I will show, first of all, that we have, when we gradually heat the platinum, the red light only visible. The wire is now glowing. We have an intensely red light given off. [The platinum wire was in three pieces, one of which was placed in each of the three differently coloured cylinders.] The red platinum wire is visible through the red cylinder, but it is not visible through the yellow and the blue cylinders. If I now increase the temperature of my platinum wire, I shall see it through both the red and the yellow liquid; but I shall not see it through the blue cylinder. Here we have the red and the yellow glowing, but we cannot see, or we can scarcely see, the blue. That shows that at the temperature which the platinum wire at this moment possesses no blue light is given off; that is to say, the temperature at which the platinum wire has arrived is not sufficient to cause it to give off blue rays, although both red and yellow rays are emitted. When I increase the temperature still more, all the wires glow, blue light is given off, and the platinum wire appears white hot.

It is the property of all solid and liquid matter, that when it is gradually heated the kind of light given off undergoes an alteration, so that the *quality* of the light varies with the temperature; but we are acquainted with a kind of matter which acts in a totally different way; we are acquainted with a kind of matter which we cannot heat so that it will give off every kind of light, or even these three particular kinds of light. If I make a gaseous body luminous, I find that it has the property of giving off only a certain kind of light; that is to say, if I take, for instance, this body—common salt—and make it a gas, which I can do easily by placing it in a colourless coal-gas flame, in which it is heated to the point at which it gives off light—this yellow light. This sodium gas cannot be heated up to a point at which it will give off any other kind of light than this yellow. We can never get blue-hot sodium vapour; we can never even get red-hot sodium vapour; we can only get it yellow hot. In all the different temperatures to which we expose this sodium vapour we get only the same yellow colour. We have here, now, first of all, the flame and coal-gas of which the temperature, according to calculation, is 2350 degrees centigrade. Now, here we have a flame of carbonic oxide gas, and its flame possesses a much higher temperature than the coal-gas; its temperature is 3042 degrees centigrade. Here we have a flame of hydrogen burning in air; the temperature of that flame, which is higher still, is 3259 degrees centigrade. Here we have the oxy-hydrogen

flame, that is to say, a mixture of oxygen and hydrogen which burns together and produces a much higher temperature—a temperature of 8061 degrees centigrade. We shall notice that when we bring a sodium compound into these different flames, which vary in their temperatures, we get the same yellow colour produced in all; and if I obtain a much higher temperature, which I can do by means of the electric spark, we likewise get only this yellow light. Here I have an electric spark of a very much higher temperature than any of these flames,—how much higher, I suppose nobody knows,—but still the luminous sodium vapour remains unchanged in its colour. If I heat another gaseous body which will give us a red light, we shall in like manner see that this red light of the luminous gas is visible at all these different temperatures, even at the highest temperature. But we must remember that there are exceptions to this rule; and those exceptions I shall have the pleasure of bringing before you on the next occasion. The general rule is, however, as I have stated it, that *luminous solids* give off a *different quality* of light when they are differently heated; and *luminous gases* give off the *same kind of light* at all different temperatures.

If we now examine these kinds of light given off by solids and by gases more narrowly—if we examine it by means of a prism, what do we find? We find that so long as we have a solid body to work upon—as long as we have a glowing solid or a glowing liquid which is producing the light for us, so long do we get a continuous spectrum—a spectrum without a break.

But when we examine the spectra of luminous gases, we see that these spectra are discontinuous and broken. Here we have a continuous spectrum. It is the spectrum of the white hot solid carbon points. We have here, between certain limits, light of every degree and refrangibility emitted. Solid bodies, then, when heated emit light of every degree and refrangibility; whereas gases when heated emit light of only certain degrees of refrangibility; and it is this peculiarity which lies at the basis of the science of spectrum analysis. If it were not for this, we should get from different bodies spectra absolutely identical. On looking at the spectra and glowing solid or liquid iron, glowing solid or liquid carbon, and glowing solid or liquid lime, we cannot thus distinguish the chemical nature of those bodies; but if we can by any means make the iron, the carbon, and the lime in a state of vapour, then that luminous vapour gives us the power of saying that iron is present, carbon is present, or lime is present; for we get from each and every elementary body the vapour of which is ignited to its point of luminosity, a spectrum which is characteristic of, and which is peculiar to, this body. With the applications of this principle we shall have more especially to do in our next Lecture.

I will show you simply to-day that we do get bright bands when we employ the vapour of a substance to give us light. Mr. Ladd will now place in the electric arc vapour of the metals copper and zinc. He will place a piece of brass, for instance, in the lamp, and we shall see those bright lines which, I have no doubt, many of my audience have already seen in this place before.

In this way, then, by examining the spectra of the elementary bodies in the state of gas, we get the evidence of the presence of such elementary bodies. On this coloured diagram I have a drawing of the spectra produced by the glowing vapours of the alkalies and alkaline earths. Now, every substance produces a peculiar bright spectrum having differently coloured bands. When we examine this with a proper instrument we find that these bands split up into lines as fine and distinct as the fibres of a spider's web; and each metal gives lines having different positions in the spectrum, and, therefore, having peculiar colours.

But we must not imagine that it is only the metals that can be thus detected. This same law holds good for the spectra of the non-metallic bodies. Thus I can show you that the body hydrogen, for instance, will give us a spectrum peculiar to itself. I cannot, I am sorry to say, show you the absolute spectrum produced by hydrogen, for many of these most glorious phenomena cannot be presented on a large scale. These things we must see with our own eyes, and only one pair of eyes at once; but I can show you that the colour of the electric spark in hydrogen is quite different from the colour of the electric spark in air. I will pass the electric spark through air and through hydrogen, and you will see the difference of colour. The gas becomes luminous, as the electricity passing through heats the little particles of hydrogen to the point at which they give out light. Then they give out light of one particular kind only, a representation of which you see on this diagram. The hydrogen spectrum consists only of one bright red line, and of one bright blue line here,—that is to say, it gives off only two kinds of light, one a red kind of light, and the other a blue kind of light. On the other hand, the colour which glowing nitrogen gives off is totally different; its spectrum contains many beautiful violet bands.

In the next Lecture, ladies and gentlemen, I shall lay before you the application of these principles to the analysis of terrestrial matters, and explain the mode in which the spectra of the various metallic and non-metallic elementary bodies can be obtained, and also the mode in which we can detect the smallest quantities of these various elementary bodies when present.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, April 1, 1862.

J. P. JOULE, *LL.D., F.R.S., President, in the Chair.*

MR. ALFRED FRYER stated that he had recently been making a series of experiments with the oxyhydrogen light, with a view to determine what substance made incandescent produced the greatest amount of light. He operated on various salts of calcium, magnesium, strontium, barium, and also upon some other substances. The best results were obtained from magnesium. The sulphate of magnesia, when baked, yielded a bright light, but was decomposed by the heat; and the sulphuorus acid escaping was very unpleasant. Calcined magnesia succeeded the best of all; but when the powder was used, the gases blew it away. When the powder was mixed with water, and afterwards dried, the cake was friable; and when the dry powder was pressed into a mould by means of hydraulic pressure, the cake split up into laminæ when subjected to the gases. After many experiments with the materials in different proportions, it was found that sulphate of lime one part, and calcined magnesia two parts, mixed with water and modelled into a cake and dried, produced the best results. This, however, is not all that could be desired, as in time the cake becomes cracked and fissured by the gas. The illuminating power is to that of lime, pressure and volume of gas being equal, as 54 is to 27. The experiments have been conducted with oxygen and the coal gas supplied to Manchester. The jet used is a form supplied by Mr. Dancer, a jet of oxygen being surrounded by an annular jet of the coal gas. Mr. Dancer has further improved the jet by allowing the oxygen pipe to project beyond the hydrogen, and by not contracting the aperture of the hydrogen pipe. Mr. Alfred Fryer exhibited the light which he had explained, and the effect produced was very striking.

Professor ROSCOE read a communication, by Professor Clifton and himself, entitled, "*On the Effect of Increased Temperature upon the Nature of the Light Emitted by the Vapour of certain Metals or Metallic Compounds.*"

A Paper was read, entitled, "*Notes on Calorific Phenomena,*" by J. C. DYER, Esq., V.P.

The author states that the essences of matter, their number and their forms, are only known to us by their

observed properties and mutations; that conflicting theories on physics arise from the various interpretations given to the same phenomena; and if unable to reconcile such differences, further inquiry, with that view, may not be improper, whilst the laws of nature rest on debateable grounds. That practically matter, in its aggregate, is found to consist of two sorts or classes—the "ponderable" and the "imponderable"—gravity and elasticity serving to distinguish their respective inherent properties. That as no tests of weight or measure can apply to the latter, its mutations and action on other bodies are the sole means we have of forming any judgment concerning its agency in the laboratory of nature. That the calorific element, or heat, is assumed to be the one "sole imponderable element which pervades matter and space throughout the universe," and it constitutes the elastic forces reacting upon and balancing the gravitating forces in all other bodies. The elemental state of heat must be taken as distinct from its other three states of sensible, radiating, and specific heat, commonly recognized. That elemental heat is acted upon by mechanical and chemical forces, and the changes which it undergoes from the one to the other conditions of heat, give rise to atmospherical phenomena known as electrical, magnetic, and optical; as also to the entire range of meteorological changes, as set forth in the paper. That by the mechanical forces of the earth's motion in its orbit and diurnal rotation, acting upon the elemental medium, its equilibrium is disturbed and motions generated which afford rational explanations of the luminous and ordinary electrical and magnetical conditions of the atmosphere, as indicated by their respective meteors. That by the action of chemical forces, great mutations of heat are continually going on; for example, the heat which on a vast scale abounds in vapour, is given out as clouds are formed, and accounts for the positive and negative electricity; and also, when redundant, for lightning from thunder clouds. As much heat is evolved above the clouds, where cold prevails, it must become elemental or neutral there, and justifies the inference that it is identical with the electric fluid, as above said. That electricity and magnetism are but diverse actions of one element has been proved, and their action on matter proves their materiality. The mechanical and chemical action of light proves its materiality also; and that light and heat are identical has been clearly established. The plurality of imponderable elements is, therefore, disproved by the fact that the mutations of the one element fully account for all of the phenomena attributed to several. The gravitating and elastic properties of matter constitute their statical and moving forces, for ever balancing each other. The former of these forces would consolidate the material universe "with lightning speed," but for the reaction of the latter force. There is no reason why the force of gravity should be measured by the established laws of falling bodies, except that experiment has shown the velocities actually attained by them *in vacuo*. But this vacuum is the absence of air only, not that of the elastic "medium;" and it is this that holds the poise of matter throughout illimitable space.

MICROSCOPICAL SECTION.

March 17, 1862.

E. W. Binney, *Esq.*, *F.R.S.*, *F.G.S.*, *in the Chair.*

Twelve specimens of soundings were received from Captain George Randall, of the barque *Brazil*, taken on the north coast of Brazil; also five specimens from Captain George Murray, of the ship *Finzel*, taken off Robin Island, Table Bay; coasts of Sumatra, Java, and St. Helena.

Mr. H. A. Hurst made a donation of eight slides of diatomacæ of various kinds; also specimens of fibre from the bombax, or East Indian cotton-tree, and the fibre of the asclepias syriacus from Bengal. Some conversation arose upon the adaptation of these fibres as substitutes for cotton, but, although fine and silky, there is not sufficient strength in the staple to render them fit for manufacturing purposes.

Mr. Blandford presented, through Mr. Hurst, a number of specimens of the tongues of mollusca from Burmah, upon which Dr. Thomas Alcock reported that there were four species,—two being fresh water, *Melania variabilis*, a species of paludomus; and two land shells, different species of cyclophorus. Cyclophorus belongs to a section of the order *Pulmonata*, distinguished by having an operculum or door to the mouth of the shell, and by having a type of teeth similar to that of the pectinibranchiata. Cyclostoma elegans is a British representative of the same group.

Mr. Cheetham exhibited a prism which he uses to illuminate objects under the microscope with the variously coloured lights of the spectrum in succession, instead of ordinary light. He finds that details of structure are more distinctly brought out by some of the colours than others; the blue and green rays are also very pleasant to work with, and easily varied by throwing the required part of the spectrum on the mirror below the stage.

Mr. Sidebotham brought before the notice of the meeting Mr. Petschler's process for producing vegetable forms with crystals of bichromate of potash in gelatine, which was discovered by him in the preparation of glass plates for photographical purposes, and exhibited at the Microscopical *Soirée* given to the British Association at the last meeting. Specimens on large glass plates were handed round, which, when magnified, aptly represent mosses, ferns, and algæ, in beautiful ramifications, which vary in many ways, dependent upon the strength of the solution, temperature, state of the atmosphere, and other causes. Mr. Sidebotham called especial attention to the peculiarity of the form of crystallisation, and to the fact that an inorganic salt, in contact with organic matter, should produce vegetable forms.

The Secretary then read a paper by Mr. Petschler, describing the plates and the process. Glass plates, Nos. 1, 2, and 3, were coated with collodion, on the surface of which a hot mixture of gelatine and bichromate of potash had been poured, then allowed to cool and to dry spontaneously. In a few hours the crystals began to form and ramify themselves over the plate. The mixture was composed of three parts of gelatine and water, twenty grains to the ounce, to one part of a saturated solution of bichromate of potash. Plate No. 4, the same mixture spread hot without collodion. On the corner of the plate the crystals have been dissolved out with water, showing skeleton traces in the gelatine left behind. Plate No. 5 is covered with collodion and a solution of bichromate without gelatine. Plate No. 6 was first covered with the mixture and then with glycerine, but no crystallisation took place. It was then dried with strong heat, gelatine and bichromate poured over hot, and then allowed to crystallise. Plate No. 7, prepared as No. 4, with gelatine and bichromate without collodion. After the crystals were formed, the plate was dipped into a solution of nitrate of silver, which changed the salt into the red chromate of silver, insoluble in water, but soluble in hyposulphite of soda, ammonia, &c. In one corner the crystals were dissolved out, leaving their casts in the gelatine. Plate No. 8 was prepared as 1, 2, and 3, by collodion and the mixture, and after the formation of the crystals changed into red chromate of silver as No. 7. Plate No. 9, prepared as No. 6, with glycerine dried with great heat, then coated with the mixture, and treated with nitrate of silver as No. 7. The great variety and beauty of these forms of crystals could with difficulty be represented by drawings. The Author believes that no chemical combination takes place between the salt and the gelatine, but that the latter acts simply as a medium. The gelatine, when firm, retains a certain quantity of moisture, which is favourable to crystallisation; but when the moisture is driven off by heat the crystallisation is suspended.

In the course of the conversation which ensued, the Chairman referred to the ramified form in which the salts of some metals were found naturally in agate, slate, and even trap rock, where the oxide of manganese was frequently found to have assumed similar forms.

Mr. Mosley suggested that the arborescent forms might perhaps arise from the tenuity of the solution, from the resistance of the gelatine to allow of crystallisation in the usual rhombic form, and possibly to the subtle electrical or galvanic action supposed to be excited during crystallisation. Some years ago he had obtained from solution of bichromate of potash tree-like forms with spreading branches and pendent rhomboids, which under the polariscope appeared like a tree with gems of rich colours for fruit. Mr. Mosley also exhibited an edition of Baker's work on the Microscope (London, 1785), with many engravings of vegetable forms in crystals of manna, salts of antimony, copper, &c.

Mr. Brothers exhibited a drawing of a small animal he found leaping on the surface of the water in his aquarium, supposed to be identical with the species of *Podura* found last year by Mr. Lynde.

NOTICES OF BOOKS.

Address of the Chairman of the Great Central Gas Consumers' Company to the Gas Consumers of the City of London. 1861.

This pamphlet is a defence of the Great Central Company for joining the Chartered Companies in raising the price of gas when the Metropolis Gas Regulation Act of 1860 came into operation. The legality of the matter will have to be settled in a few days by the judges; the policy of allying themselves with the Companies they started to oppose the Directors must settle for themselves. As the Act of 1860 compels the supply of gas 14 per cent. better than the Great Central Company contracted to supply for four shillings, there would seem to be no immorality, whatever the impolicy, in their raising the price 11 per cent.

CORRESPONDENCE.

Downing v. Chance.

To the Editor of the Chemical News.

Sir,—A friend has just called my attention to your "editorial comments," and to your "report" of the trial, "Downing *v.* Chance," at Worcester Assizes (which you erroneously print Stafford), and which appeared in your Number for April 5.

In your "editorial" you make me say, "does not remember the composition of cyanogen, for the compounds of hydrogen are so complicated;" and in your report of the trial you make me say, "He could not just then refresh his memory as to which two gases cyanogen was composed of at the moment; some of the compounds of hydrogen were so difficult."

I said neither of these. The question put in cross-examination by "Counsel" was, "What is the composition of cyanogen?" Believing that the question was only put to cause a wrangle, as cyanogen could have nothing to do with the case, I replied, "I decline to answer."

There is all the difference between giving a wrong answer, and not replying at all.

Of course the question will arise, "Why refuse to answer?" Perhaps if I had had five minutes' reflection, I might have thought differently; but at the moment I considered that I exercised a sound discretion in thus stopping a probable series of irrelevant questions, not put for the elucidation of the truth, but solely to bewilder the jury.

By a singular coincidence, a few days after, the same chemists and the same "leading counsel" again appeared in "hostile array" before the same "judge," at Stafford Assizes, when full opportunity was given by me to "the other side" to put any questions they pleased, if they dared; but they knew better.

I just add that myself and Mr. Williams can well afford to suffer any amount of "criticism," under the not trifling satisfaction that in both "trials" "special juries" gave verdicts to our clients, the "plaintiffs."—I am, &c.

Alfred Bird.

Birmingham, April 15.

Paraffin Matches.

To the Editor of the Chemical News.

Sir,—In your last number, at page 209, you notice my patent for paraffin matches, and append some remarks which are not in accordance with the merits of the case. I therefore send you a halfpenny box of these matches, that you may see for yourself that they burn with a beautiful flame without either smoke or smell.

You are correct in stating that stearine has long been used in match-making in place of brimstone, but it is difficult to make the wood of such matches catch, whereas with paraffin they ignite immediately; and you overlook the fact that such matches are dearer than common matches, whereas the great merit of my patent paraffin matches is that they are as cheap as the common brimstone match, thus making it an article for the million,—a large halfpenny box of matches without any smell, in place of the obnoxious sulphurous match.—I am, &c.

R. Letchford.

Clarifying Wine.

To the Editor of the Chemical News.

Sir,—May I, as a subscriber to your valuable periodical from the first, ask the solution of the following questions?—"Why pure albumen (the white of egg) is used for the clearing of red wines in preference to pure Russian isinglass?" and, also, "Why pure Russian isinglass is used for the clearing of white wines in preference to pure albumen?" and, also, "What is the difference in their action?"—the constituents of the wines being in most cases similar, at least so far as the quantity of tannic acid is concerned. I am, &c.

In vino veritas.

Plymouth.

MISCELLANEOUS.

Monochlorinated Sulphuric Acid.—Rosenstiehl (*Comptes-Rendus*, t. liii. p. 658) dropped fused chloride of sodium into a retort containing anhydrous sulphuric acid, and applied sufficient heat to melt the acid. He then distilled to dryness, and obtained an oily, colourless liquid, sp. gr. 1.762, which boiled between 145° and 150°, fumed a little less that anhydrous sulphuric acid, and carbonised energetically organic matters. Analysis led to the following as the formula of the compound, $S_2 O_5 Cl$. With manganates it disengaged chlorine, gave chlorochromic acid with chromates, and chloride of acetyle with acetate of soda,—proofs that the new body is an energetic chlorinizer.

ANSWERS TO CORRESPONDENTS.

Strychnine.—In answer to the question of H. W. C., we can refer him to a method of Mr. Horsley's, which he will find reported in the "Proceedings of the British Association for 1856."

F. R. G.—You would most likely have chloride of nitrogen formed.

THE CHEMICAL NEWS.

VOL. V. No. 125.—April 26, 1862.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Researches of Pasteur respecting the Theory of Spontaneous Generation, translated and condensed by M. C. WHITE, *M.D.*

THE theory of spontaneous generation was long since proposed to account for the origin of beings whose germs were too minute or too obscure to attract attention. One after another the different organisms supposed to arise from spontaneous generation have been proved to originate from germs. At present the question of spontaneous generation concerns only the origin of *entozoa* and those minute organisms which can be studied only with the aid of the microscope, as moulds (minute fungi) and infusoria, both animal and vegetable. The common theory that the spores or germs of these minute organisms are constantly floating in the atmosphere ready to start into activity whenever they meet with a suitable nidus, has found an able advocate in M. Pasteur, of the Normal School of Paris, who has published in the *Comptes-Rendus** a series of valuable papers on this subject, the substance of which I have translated.

In order to collect and examine the solid particles floating in the atmosphere, Pasteur placed soluble gun-cotton in a glass tube, and, by means of an aspirator, caused a current of atmospheric air to pass through it for several hours. The cotton was then dissolved in a mixture of alcohol and ether, and the atmospheric dust deposited at the bottom of the fluid in a conical glass was examined in the microscope. The sediment thus collected contained grains of starch and such other dust as is ordinarily found on surfaces exposed to the air. When submitted to the action of concentrated sulphuric acid, the starch was soon dissolved, while other particles remained undissolved and had all the characteristics of the spores of ordinary mucedines, which are known to resist the solvent properties of concentrated sulphuric acid. [It is worthy of notice, that certain minute fungi are capable of decomposing a solution of sulphuric acid. A few years since, a little mould developed in the solution of sulphate of copper, used for electrotyping in the department of the U. S. Coast Survey at Washington, proved an intolerable nuisance. It decomposed the salt, assimilating the sulphuric acid, and rejecting the copper which was deposited around its threads in a metallic form. From this it appears that sulphuric acid does not prevent, but may rather assist the growth of certain fungi.—*Tr.*]

To determine the action of atmospheric air, and of atmospheric dust upon fermentation, putrefaction and the appearance of organisation, Pasteur adopted the following methods:—

* *Comptes-Rendus*, 1860, t. l. 51,

A flask was about half filled with a fluid consisting of water containing in solution about ten per cent. of sugar and from two to seven parts in a thousand of the scum of beer. The neck of the flask was drawn out in the flame of a lamp and attached to a platinum tube, $\frac{1}{25}$th of an inch in diameter, which was then heated to redness. The fluid was boiled for two or three minutes to expel all air from the flask, when it was allowed to cool very gradually, and as it cooled the air which entered the flask was calcined, and all organic germs it contained were destroyed by passing through the red-hot platinum tube. When the flask had thus cooled to the temperature of the surrounding air, the neck was hermetically sealed. The flask was then removed to an oven, and kept at a temperature of 80° or 90° F. for an indefinite period, without producing any organisms or undergoing any change whatever.

To test the influence of atmospheric dust upon a fluid thus hermetically sealed, Pasteur placed a pledget of cotton or asbestos in a small tube, and caused a current of common air to pass through it by means of an aspirator. This small tube containing the cotton or asbestos, loaded with atmospheric dust, was then transferred to a larger T-shaped tube, one end of which was connected by india-rubber with the sealed flask, another end was connected with a platinum tube heated to redness, and the third being connected with an aspirator, the apparatus was easily charged with calcined air, and all the common air was expelled. The neck of the flask was then broken within the T-shaped tube, and the small tube containing the atmospheric dust was passed into the flask, with access only of calcined air. The neck of the flask was then again hermetically sealed by means of the blow-pipe. Many flasks were prepared in this way, and in every case, after standing in a warm situation for twenty-four to thirty-six hours, vegetation appeared in the same manner as if the contents of the flask were exposed to the open air; but the mould or mucedines appeared first in the little tubes carrying the cotton, which was often thus filled to its extremities. The organic growths which appeared were the same as in flasks exposed to the open air,—viz., of infusoria, *bacterium;* of mucedines, the *penicilium*, *ascophora*, *aspergillus*, and some others. When calcined asbestos alone was introduced no vegetation appeared.

It was thus demonstrated that amongst the dust suspended in ordinary air there are always organised corpuscles, and that these powders when mixed with a suitable liquid, in an atmosphere of itself inactive, give origin to *bacteria* and *mucedines* such as are furnished by the same liquid in the open air.

Pasteur confirmed these results by another method. Similar quantities of the same fermentable liquid were introduced into a series of flasks in all respects alike. The necks of the flasks were all drawn out over the flame of a lamp, and bent into a variety of different forms, but the tubular neck of each flask was left with

an opening $\frac{1}{25}$th of an inch or more in diameter. In some of the flasks the liquid was boiled for several minutes, but three or four were not heated to the boiling point. All the flasks were then set away in a quiet place, free from currents of air. After twenty-four or forty-eight hours, according to the temperature, the flasks in which the liquid was not boiled after being put into them (although all the liquid had been boiled before it was put into the flasks) were found to be troubled and covered little by little with mucor. The liquid which had been boiled in the flasks remained limpid, not only for days, but even for entire months, although all the flasks were left open. There can be no doubt that the curves and sinuous forms of the necks served to secure the contained fluid from the fall of germs.

The common air entered these flasks as they were cooling, but so slowly during the gradual cooling of the hot liquid, that the germs were either destroyed by the heat or were deposited in the curvatures of the narrow necks of the flasks, so that no viable germs reached the liquid. When the neck of one of these flasks was broken off, and the remaining portion placed vertical, in a day or two the liquid became mouldy or filled with bacteria. This method, which so well explains the preceding, and which can be so readily practised by any one, carries conviction to unprejudiced minds. It gives also peculiar interest to the proof which it presents to us, that there is nothing in the air except its dust, which is a condition of organisation. It thus appears that oxygen acts only to sustain life furnished by germs, while of gas, fluids, electricity, magnetism, ozone, things known or unknown, there is nothing in the air except the germs which it carries which can originate organic life.

(To be continued.)

On Soil-Analysis, by Professor S. W. Johnson, *of Yale College.*

(*Continued from page* 199.)

We now come to Dr. Owen's fourth result of soil-analyses,—viz., its power of indicating "the suitability of the soil for any particular crop." Closely related to this is the fifth item,—viz., that analysis can show "what addition any soil, either uncultivated or cultivated, requires to render it productive and remunerative for any given crop; and, of course, the deficiency in the soil of one or more of the eleven elements determined by chemical analysis."

We cannot help feeling that the above assertions, which are here made unqualifiedly, were intended to be understood with a large amount of reserve, and subject to various conditions. Otherwise, we must regard them as quite unjustified, if not absurd. The chemical analysis of soil reveals nothing as to its tenacity or lightness, its porosity or retentiveness for water; yet these physical and mechanical conditions more than anything else determine the adaptation of a soil for any particular crop. The best grass lands are not the best wheat lands, and although it would scarcely be questioned that wheat requires a richer soil than grass in order to produce an average crop, and although, as we know, it often happens that many successive hay crops may be removed from a meadow without sensible diminution of the yield, while uninterrupted cropping with wheat nearly always reduces the capacity of the soil in a very few years below a profitable point; yet each average hay crop removes from a field more of every ingredient of vegetation than the grain and straw together of an average harvest of wheat.

Such, at least, is the testimony borne by the most recent and trustworthy data. Dr. Anderson, of Glasgow, basing his calculations on the best analyses, and on the extensive agricultural statistics gathered in late years by the Highland and Agricultural Society of Scotland, makes the annexed estimate of the amount of the principal ingredients removed from an acre by average crops of seven staple British farm products (*Trans. Highland and Agricul. Soc.*, 1861, p. 568).

On comparing the amount of matters removed from an acre by the wheat and hay crops, we find that the latter requires four times as much potash, lime, and sulphuric acid, twice as much silica, and one-fifth more nitrogen.

Again, we know that oats are raised on soils which are considered too poor for the profitable production of wheat, and the Table shows us that an average crop of oats requires more of every mineral ingredient than is necessary for a corresponding wheat crop.

In fact, wheat is the crop to grow which continuously

	Produce per Imperial Acre.	Total Weight in lbs.	Total Mineral Matters	Potash.	Soda.	Lime.	Magnesia.	Chlorine.	Sulphuric Acid.	Phosphoric Acid.	Silica.	Nitrogen.
Wheat:—												
Grain	28 bush. at 60 lb.	1680	34·12	10·11	1·20	1·04	4·80	...	0·32	16·22	0·43	29·20
Straw	1 ton, 3 cwt.	2576	114·48	20·70	2·84	8·53	2·23	...	3·55	3·16	73·47	16·13
Total ...			148·60	30·81	4·04	9·57	7·03	...	3·87	19·38	73·90	45·33
Barley:—												
Grain	33 bush. at 53 lb.	1749	44·24	9·40	0·30	0·76	3·10	1·12	0·85	15·52	13·19	34·98
Straw	18 cwt.	2016	99·14	11·24	1·14	5·81	2·75	1·30	1·10	7·22	68·58	6·03
Total ...			143·38	20·64	1·44	6·57	5·85	2·42	1·95	22·74	81·77	41·01
Oats:—												
Grain	34 bush. at 40 lb.	1360	48·89	11·00	...	5·31	4·04	0·20	...	26·07	2·27	27·54
Straw	1 ton.	2240	143·53	30·71	6·10	10·29	5·50	5·55	5·18	7·35	72·85	14·10
Total ...			192·42	41·71	6·10	15·60	9·54	5·75	5·18	33·42	75·12	41·64
Beans and Peas:—												
Grain	25 bush. at 60 lb.	1650	55·97	30·00	0·31	3·01	4·00	...	1·76	16·65	0·24	46·10
Straw	1 ton.	2240	108·51	48·61	13·14	29·37	3·74	7·00	2·07	0·74	3·84	26·88
Total ...			164·48	78·61	13·45	32·38	7·74	7·00	3·83	17·39	4·08	72·98
Turnips	13½ tons.	30240	213·75	57·35	44·71	28·60	4·65	10·35	39·02	22·57	6·50	60·48
Potatoes	3 tons.	6720	55·58	28·92	2·85	1·20	2·11	3·21	10·24	5·76	1·29	26·00
Hay	2½ tons.	5600	391·31	129·79	4·80	35·46	9·62	39·61	16·57	21·79	133·67	56·22

requires, according to universal agricultural experience, land richer than that needed for any other of the seven crops whose chemical statistics are given in Dr. Anderson's Table, and notwithstanding, with the exception of barley and the potato tuber, it removes the least from the soil.

The farmer knows that wheat delights in a deep, rather heavy soil,—one which holds moisture well and yet is not wet. Barley and oats flourish on soils that are too dry and light, and grass on those which are too wet for wheat.

But how does the matter stand when these external conditions are taken into account? Does not analysis aid us then in a good degree? Let us take a case similar to what has repeatedly occurred in actual practice. We have a soil which, as the result of long cultivation or from natural deficiencies, is incapable of yielding a remunerative crop of wheat. Its texture is good, it has produced wheat abundantly, and needs nothing but a little of the right kind of manure to restore its power of giving a crop. We put upon it Peruvian guano at the rate of 300 lbs. per acre, and the harvest is a good one. The entire addition to the soil is but $\frac{300}{3000000}$ths = one hundredth per cent. The amounts of phosphoric acid, of alkaline earths, and nitrogen added, are, for each, but but $\frac{1}{600}$th per cent. of the soil taken to the depth of a foot. These quantities are rather minute for even the improved analysis of the present time to estimate successfully.

Calculations like this show that the chemist cannot discriminate by his analysis between, first, a soil which is unproductive from the temporary exhaustion of some of its available ingredients; second, the same soil which is rendered fertile again for a year by the use of 300 lbs. of guano; and third, the same made over-rich so that nothing will grow on it, by an application of a ton of guano.

On page 18 of the Second Kentucky Report, Dr. Owen remarks as follows:—"During last summer a soil was collected in Bullit county, from an old field which had been fifty or sixty years in cultivation, and which will now no longer produce clover. I venture to predict that when the analysis of this soil shall be completed it will be found to be deficient in some of these constituents,* and the analysis will probably show what other green crop might succeed better for the renovation of such land."

On page 230 of the Third Kentucky Report, Dr. Peter gives the analysis of this soil, and says:—"The inability of this soil to produce clover is explained by its very small proportion of lime, and rather small amount of sulphuric and phosphoric acids. The addition of plaster of Paris or some of the calcareous marls would probably restore it to the capability of supporting a clover crop."

The per-centage of the ingredients which Dr. Peter considers deficient are as follows:—

Carbonate of lime	0·072	= lime 0·040
Sulphuric acid	0·055	
Phosphoric acid	0·070	

Small as are these quantities, the smallest of them, viz., that of lime, yet amounts to 1200 lbs. per acre, which is enough to supply ten clover crops of three tons each, and as by the analysis it all exists in the form of carbonate, it must all be available. We know from the vegetation experiments of Boussingault, Ville, and Sachs, that plants are capable of absorbing from a limited amount of soil the whole of any soluble nutritive substance present, provided its quantity be no more than the plants require, and the other elements of fertility are at hand in excess.

(To be continued.)

* The mineral ingredients of plants.

Preparation of Formic Acid by means of Carbonic Acid, by MM. KOLBE *and* SCHMIDT.

A MIXTURE of bicarbonate and formiate of potassium is obtained in twenty-four hours by spreading potassium in thin layers under a receiver closed by tepid water, and into which an atmosphere of carbonic acid is introduced.

Sodium behaves in the same way, but the yield is less.

The saline mixture is white. When neutralised by sulphuric acid and distilled, it evolves the formic acid, which boiled with carbonate of lead forms beautiful needles of formiate of lead.

No carbonic acid is produced when a concentrated solution of carbonate of potash is submitted to electrolysis.—*Annal. der Chem. und Pharm.*, cxix. 251.

TECHNICAL CHEMISTRY.

Chemistry and the Manufacture of Iron.

No. I.

Fire-bricks.—Composition Method for Testing, &c.—As an analytical chemist, I have had much to do with ironmasters and blast furnace managers, and find that, although some of them are acquainted to a certain extent with chemistry,—these are very few,—the majority of them know little or nothing about it. The consequence is, that an analysis of either the material used or produced is not so generally appreciated as it ought to be.

In three or more papers I shall endeavour to describe as fully as possible the chemistry relating to this manufacture, and to point out the importance of an analysis to those engaged in it.

Fire-bricks.—In this country fire-brick is generally used for constructing the furnace, and it is of the first importance that this should be of good quality. From an analysis of a brick, along with its physical appearance, an intelligent manager ought to be able to say whether or not it will answer his purpose.

A fire-brick, to be generally useful about blast furnaces, must not only withstand high temperatures without "running," but also the action of melted slag when at this temperature. Some bricks will stand almost any temperature if let alone, but when brought into contact with melted slag they fuse, or, again, if touched by the workmen's tools they break easily; and hence, although very suitable for some special purpose where heat alone is required, they are generally of very little use indeed. A good fire-brick ought always to be very close, and possess a good specific gravity. Closeness of grain and density sometimes make up for a little inferiority in composition. Of course, I speak now of bricks that are to be exposed to chemical action as well as heat. Under other circumstances this is not of so much consequence.

Fire-bricks generally contain silica, alumina, oxide of iron, and potash. Silica and alumina are the principal constituents; in fact, the more nearly the brick is free from the other bases the better it may be considered, because the greater the number of bases a silicate contains the more easily it is fused. For instance, a double

silicate of magnesia and lime is more readily fused than either of the compounds alone. To a certain extent, also, the less the per-centage of alumina the better. The following table shows analysis of four fire-bricks, which differ much in composition:—

Analysis of Fire-bricks.

	No. I.	No. II.	No. III.	No. IV.
Silica . . .	70·93	59·13	82·71	63·17
Alumina . .	26·81	29·26	11·84	33·20
Lime . . .	1.01	1·76	2·61	1·27
Magnesia . .	·50	2·03	·45	1·30
Peroxide of iron .	·98	5·58	2·50	1·42
Potash . .	a trace	2·68	·31	·40
	100·23	99·44	100·42	100·76

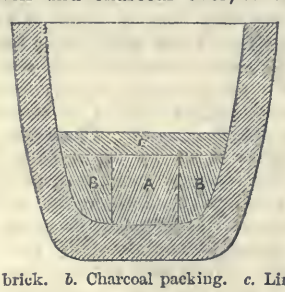
No. I. is a first class Stourbridge fire-brick. It was found to stand a very intense and long-continued heat. In the hearth of a blast-furnace it remained little acted upon by either slag or iron. By referring to the table, it will be seen that the per-centage of silica is very high; besides this only 2·49 per cent. of directly injurious bases are present.

No. II. is a very inferior Newcastle brick. When tried in coke ovens it was found to "run." In the cupola, too, it required so frequently renewing, that for this purpose it is now very seldom used.

No. III. is a brick made from a mixture of clay and sand. Owing to the very large per-centage of silica which it contains, it is well adapted either to stand intense heat or to be placed in positions much exposed to the action of slag. There is an opinion, which is held by some ironmasters, that a silicious brick is not calculated to withstand the action of a basic slag; there is a danger, they say, of the lime contained in the slag uniting with the silica of the brick and forming a fusible silicate of lime. Tried by an experiment, which I shall detail directly, this is not a correct opinion. The infusibility of the silica in a great measure prevents this action. Besides, a brick containing more base is much more likely to form a double silicate of lime and alumina, which, melting at a lower temperature than silicate of lime alone, would, in the same space of time, expose more surface to the action of fresh portions of slag.

No. IV. is a sample of a very good Newcastle fire-brick.

In the following manner the comparative value of a number of samples of bricks can be readily determined:—Take a piece about one cubic inch in size from each brick, and fix them in separate plumbago crucibles with charcoal, in the manner shown in section in diagram; now cover both brick and charcoal over, to the depth of a



a. Fire brick. *b.* Charcoal packing. *c.* Limestone.

quarter of an inch, with limestone; the crucible must then be covered in the usual way, and exposed to the highest heat of the wind-furnace for an hour. After the crucibles are cool, break the lids off and examine the pieces of brick; they will be found to have been penetrated to different depths by the limestone; the one acted upon most must be considered the worst. Exposed in this way, Nos. I. and III. were very little acted upon; No. IV. was semi-fused to about one-eighth of an inch in depth; and No. II. to a quarter of an inch.

(To be continued.)

Analysis of an Artificial Manure, by Dr. T. L. Phipson, *F.C.S., Member of the Chemical Society of Paris, &c., &c.*

The artificial manure of which I publish the analysis was manufactured by the Guaranteed Manure Company of London, according to my directions, and is daily produced at the rate of several thousand tons a-year. It appears to be the most perfect artificial manure that has yet been produced, both as regards its chemical composition and its effects upon the land.

Water	10·00
Fibrine, Albumine, Gelatine	11·76
Humic acid, Grease, Sugar	7·74
Sulphate of ammonia	12·30
Sulphate of lime	26·50
Sulphate of magnesia	1·45
Sulphate of potash	1·20
Sulphate of soda	0·20
Biphosphate of lime, CaO, PhO^5 . .	11·00
Phosphate of lime,—3 CaO, PhO^5 . .	5·50
Phosphates of iron and alumina . .	2·53
Phosphate of magnesia	0·12
Chloride of sodium	2·45
Chloride of potassium	0·05
Chloride of calcium	traces
Oxide of iron	0·82
Oxide of alumina	0·73
Oxide of manganese	traces
Soluble silica	0·40
Silicate of alumina, magnesia, and iron .	1·98
Sand, with a little charcoal . . .	3·10
	99·83
Other matters and loss	0·17
	100·00

Total nitrogen = 4·11 per cent., equal to
Ammonia = 5·00 per cent.
Soluble phosphates equal to soluble phosphate of lime, 18·00 per cent.
Soluble salts (sulphate of lime excepted) = 28·85 per cent.

It is evident that, whatever theory we profess with regard to the fertilising action of manures, such a composition as the above must constitute a more valuable fertilising agent than Peruvian guano. This, in fact, has been found to be the case in practice.* It is not sufficient to consider what elements are found in a manure; we must also take into consideration the state in which they are presented to the plant. Pure gelatine, though very rich in nitrogen, is no fertiliser; coprolites and bone-meal require to remain a long time in the soil before they become assimilable, and in certain soils where no acids are produced they never can be absorbed.

* See an interesting account of some experiments with twelve kinds of manure, by Captain Campbell, of Craigie (Ayrshire), published in the *Ayr Advertiser*, January 9, 1862.

PHARMACY, TOXICOLOGY, &c.

On the Preparation of Resin of Podophyllum, by Edward Parrish.

The process for preparing resin of podophyllum consists in exhausting May apple-root with strong alcohol, concentrating the tincture, and throwing it into water to precipitate the resin; collecting, washing, and drying this. Upon each of these points a few comments are offered.

Exhausting the Root.—In my former experiments I used the steam-displacement apparatus invented by the late C. Augustus Smith, and found alcohol at the boiling point, used in this way, to produce a very concentrated tincture, though the use of the steam displacer involves a good deal of unnecessary trouble in small operations. For treating one or two pounds of the powdered root, which should be fine, a large funnel is convenient, the powder being moistened with a very little, say six fluid ounces of alcohol to the pound, and poured upon a plug of tow or cotton in the apex of the funnel, well shaken and packed after each addition. On a further addition of alcohol, the tincture passes, drop by drop, very strong, so that a pound may be thoroughly exhausted with from one and a-half to two pints. The proper point to desist from the addition of the menstruum is conveniently ascertained by dropping the percolate into water, when, if it contains an appreciable portion of resin, each drop will occasion a slight cloudiness.

Concentrating the Tincture.—In large operations it will be desirable to recover the alcohol, which may be done with very little trouble with a pharmaceutical still. In evaporating a pint or two of the tincture, an evaporating-dish on a sand-bath, or submitted to the regulated flame of a gas-furnace, will serve a good purpose. The extent of the evaporation is a point of importance, to determine which I have made a number of experiments. If the tincture is not concentrated enough, a considerable proportion of the resin will remain in solution when added to water; if too much, it will not mix with water sufficiently to produce a favourable separation of the resin; in the case of a recipe furnished with a view to its insertion in the Pharmacopœia, I obtained three-fourths as much resin on the partial evaporation of the liquid after the separation of the first precipitate as was precipitated on the original admixture with water. The best point at which to arrest the evaporation appears to be at from two and a-half to three and a-half fluid ounces of the evaporated tincture to each pound of the root treated.

Precipitating by Water.—The proportion of water to which the evaporated tincture should be added is not unimportant. I think four parts of water to one is, perhaps, most desirable. If the specimen of the root treated was highly resinous, and the extraction was very complete, the alcoholic fluid extract may be rather thick and even of a syrupy consistence at the degree of concentration above indicated. In this case it is better to add it to the water while hot and comparatively fluid.

Collecting the Precipitate.—In some instances in which this process has been varied to test the eligibility of certain modifications, the resin has been so imperfectly precipitated as in part to pass through a filter, remaining suspended in the filtrate; in others, though arrested by the filtrate, it could be only partially separated after drying; in no case could it be successfully collected by subsidence; so that several experiments were made to find the best method of collecting it. The most successful of these consisted in heating the whole aqueous liquid, as contained in the precipitating vessel, in a water-bath till, just below the boiling point, nearly all the resin was fused and collected on the bottom and sides of the jar; then by a spoon or spatula the main portion could be collected together, and by rotating the mass all adjacent particles could be made to adhere to it. The "Eclectics" are in the habit of adding muriatic acid to the water to aid the separation of the resin. I have found this highly advantageous, both with reference to collecting the precipitate as only partially separated by water alone, and to procuring the remaining portion after the more completely separated precipitate has been removed. There is an objection on the part of some to such an addition, under the supposition that the change must be a chemical one, but I observe no difference between specimens, whether collected with or without this addition, and am inclined to attribute the more complete coagulation of the particles of resin under the influence of the acid, which may be used in very small proportion, to a mechanical rather than a chemical alteration.

The Drying of the Precipitated Resin in powder is not a very easy matter. It is rather unsuitable to wrap in paper, especially where artificial heat is to be used, which is apt to fuse it and occasion its absorption by the paper. In one case in which it had been collected on a filter, I was obliged to re-dissolve it in alcohol, and then pour it out on plates of glass, in the manner directed for citrate of iron. It was readily scraped off from the glass, but was not in handsome scales. If collected in mass by fusion under water, as above described, it may be kneaded and pulled so as to wash it thoroughly and lighten its colour, and may thus be dried without the least difficulty by wiping with paper and exposing to the air at ordinary temperatures. If prepared in powder it is readily reduced by trituration. I prefer it in lumps or pieces, in which condition it is more characteristic, and resembles resin of jalap of the shops. It is more characteristic, and less liable to sophistication or adulteration, when in the condition of broken mass, than in that of powder in which it is usually sold.

The Yield, by the process described, varies from three to five per cent. of the root. There is, perhaps, always some loss in the course of the process, which is proportionably less in operating on large quantities.—*American Journal of Pharmacy.*

Process for the Extraction and Investigation of Poisonous Alkaloids, by MM. V. Uslar *and* J. Erdmann.*

On the Characteristic Reactions of some Poisonous Alkaloids, by M. J. Erdmann.†

On some Characteristic Reactions of Nitric Acid, by M. J. Erdmann.‡

Many difficulties attend the extraction of an alkaloid when, as in medico-chemical researches, it is associated with other organic matters. The following is a method recommended both by its simplicity and its generality. It is founded on the following facts:—

1. Free vegetable alkaloids are soluble in amylic alcohol, especially by aid of heat.

2. Pure or alkaline water does not remove the bases thus dissolved; but,

* *Annalen der Chemie und Pharmacie*, vol. cxx., p. 121.

† *Ibid.*, vol. cxx., p. 188.

‡ *Ibid*, vol. cxx., p. 193.

3. It separates them completely when it has been previously acidulated with hydrochloric acid, the organic chlorides which are formed being almost insoluble in amylic alcohol.

The following is the method of operation:—Reduce the suspected matter into a pulp with water, slightly acidulated with hydrochloric acid. Then leave it to digest for two hours at a temperature of from 60° to 80° C.; pass a wet cloth over it, and exhaust the residue with acidulated warm water, and after mixing the liquids add a slight excess of ammonia; concentrate over an open fire, and dry in a water-bath. After exhausting the residue with warm amylic alcohol, filter it through a paper previously moistened with amylic alcohol.

The filtered product is generally mixed with fatty or colouring matters, which must be eliminated by quickly shaking up the liquid with almost boiling water, acidulated with a little hydrochloric acid. The amylic alcohol then yields the alkaloid, while it retains the greater part of the fatty or colouring matters. Draw it off by a small india-rubber pipe, § then shake the warm aqueous liquid with a fresh supply of amylic alcohol, and the foreign matters will be got rid off without much trouble, so that the acid solution containing the alkaloid in the state of hydrochlorate is completely decolorised. Slightly concentrate this solution, add a slight excess of ammonia, and then amylic alcohol, which after repeated shakings takes up the alkaloid.

After duly separating the two layers of liquid, withdraw the upper one, containing alcohol and alkaloid, and attack the lower layer by adding a fresh portion of warm amylic alcohol; then mix the alcoholic liquids and evaporate them by a water-bath, and the residue is generally pure alkaloid. If it has preserved its colour, the operator need not continue the operations just described; that is to say, dissolve in hydrochloric acid, shake with amylic alcohol, and draw off by a small pipe; supersaturate with ammonia, shake with amylic alcohol, and eliminate it by evaporation in a water bath.

It is very seldom that the alkaloid is not completely purified by this treatment; should it not be, the process must be repeated. ‖

The authors have verified their process in various ways. Hydrochlorate of morphine mixed with panada, or putrid meat, exposed to the sun for fifteen days, was integrally detected by the special reaction it gives with sesquichloride of iron; nevertheless, the experiment was tried with less than a decigramme of hydrochlorate, mixed with 1 or 2 kil. of organic matter. The different portions employed varied between 0·054 grammes and 0·005 grammes.

They have also recovered a drop of nicotine and two drops of coniine respectively added to 750 grammes of panada.

The same with 9 milligrammes of strychnine, 8 milligrammes of narcotine, as well as with a mixture formed of 0.012 grammes of morphine and 0·013 grammes of narcotine mixed with a pulp of vegetables and meat, and left for four days to putrefy.

The alkaloids when recovered were separated from each other by ether.

§ This precaution is essential to prevent the inhalation of the vapours of amylic alcohol.

‖ To all acquainted with the alteration produced in nicotine and coniine by the presence of air, it will be difficult to understand how alkaloids can escape the causes of decomposition to which they are exposed during this process, which not only does not protect them from the action of the air, but exposes them to it in presence of ammonia at the temperature of a water-bath.

On Fermentation as a Cause of Various Diseases, by M. Polli.

Chemists who have for several years been successfully studying the phenomena of fermentation, have observed that this mode of reaction, among organic principles, possesses an importance much greater than is generally supposed. It is, in fact, to fermentation that the spontaneous decomposition of animal and vegetable tissues is owing,—as dry rot, eremacausis, gangrene, &c., and the whole series of successive transformations which organic matters undergo until they are converted into water, carbonic acid, ammonia, and mineral matters. It is by fermentation that fatty bodies give glycerine; that salicine furnishes glucose; that myronate of potash is converted into essential oil of mustard; that neutral substances, such as urea and allentoin, form ammonia; that amygdaline produces the poisonous substances,—oil of bitter almonds and Prussic acid.

Ferments act by contact or by catalysis. Sometimes they are living creatures; sometimes very active unorganised substances. Diastase, emulsine, and pepsine act as ferments. They can cause organic substances to double, become hydrated, or isomeric.

According to M. Polli, there exists considerable analogy between the processes of fermentation and several organic metamorphoses which take place in certain maladies: an albuminoid matter which, in a certain deteriorated state, acts as a ferment, and particular substances preceding from its action.*

But analogy is insufficient. It has been shown by carefully made experiments that the composition of the blood during disease undergoes alterations and variations; and that artificial disease, closely resembling natural ones, can be produced by introducing into the blood-vessels substances acting as ferments. Multiple abscesses induced by injecting pus into the veins of dogs; septic affections caused by the injection of purulent putrid matters into the veins of animals; diseases with all the characteristics of typhoid fever provoked by the injection of putrefied blood into the circulating current; finally, contagious diseases, such as the glanders, which are produced by injecting glandered humours, prove that a general affection can be simply produced by introducing into the blood a substance to play the part of a ferment. There are diseases produced by morbific ferments, which may be called catalytic maladies, in which the morbific matter, inducing metamorphoses by contact with the alterable principles of the blood, is the primary cause of all the symptoms presented by the animal economy. It is impossible to deny that fermentation takes place in the blood.

Admitting that the starting-point of many diseases is the action of a specific ferment in the blood, is it possible to prevent its effects to render it inactive in the living organism, as can be done by chemical means outside the body? This is the cardinal point which gives interest to this pathological question.

M. Polli believes that he has proved, by a series of facts and conclusive experiments, that it is possible to neutralise morbific ferments in the blood of animals by chemical substances which do not act in a manner incompatible with life; and that with these substances it is we must hope successfully to treat diseases of which fermentation is the primary cause.

* M. Pasteur says that the ferment is not an albuminous matter altered by oxygen, but an organised creature of which the germ is brought by the air; and that the presence of albuminous matter is a condition indispensable to all fermentation, because these substances are necessary for the development of the ferment.

It is well known that sulphurous acid gas prevents alcoholic and acetic fermentation, as well as the fermentation of animal matters and of organic matters in general. Thus it arrests, if it be already begun, the fermentation produced by saliva and diastase in contact with starch, the fermentation occasioned by myrosine in a paste of black mustard-flour, that produced by emulsine on the amygdaline of bitter almonds, &c.

M. Polli has proved that alkaline or earthy sulphites possess the same antiseptic and decolorising properties. This is an important fact, since it allows of the application of sulphurous acid in therapeutics. He thinks that he has grounds for believing that the action of sulphurous acid and sulphites on colouring matters, as well as on ferments, is not a deoxygenation, nor a combination, nor destruction, but simply a molecular modification.

This action of sulphurous acid and sulphites affords an explanation of the valuable property possessed by these chemical compounds of preventing, or energetically arresting, the action of morbific ferments artificially introduced into the blood of animals, without altering its composition in a manner incompatible with life.

From a number of experiments on dogs, mentioned in his memoirs, M. Polli has determined the quantity of the safe and efficacious dose of sulphite for internal administration, the changes they undergo in the organism, and their curative action on affections produced by injecting putrid or contagious matters into the blood.

The following are some of his experiments, selected from those of the last-mentioned series :—

1. Ten grammes of sulphite of soda were given to a dog in a space of five days, then one gramme of pus was introduced into the femoral vein. The animal became spiritless and refused food, but its spirits returned the next day, and it ate willingly. The experiment repeated two days after, yielded a like result. The animal was perfectly cured in a few days.

2. One gramme of pus was injected, in two portions, into the veins of a dog more robust than the subject of the preceding experiment. The animal became dull, but ate the next day; the following day it was very low, the breathing difficult, the wounds sanious, the left leg and foot swelled, and it died ten days after.

3. An equal quantity of putrid blood was introduced into the veins of three dogs; one died five hours after, another after five days' illness, and the third, to which some sulphite of soda had been given, recovered rapidly after a little illness.

4. Numerous other experiments with putrid blood and glandered mucus prove that animals die with every symptom of general infection, whenever sulphite of soda is not administered, and that, on the contrary, they speedily recover under the influence of this medicament.

If these facts are confirmed by other experiments, M. Polli will have rendered an inestimable service to therapeutics, and will have thrown some light on the still obscure origin of numerous diseases.

PHYSICAL SCIENCE.

On the Molecular Dissymmetry of Natural Organic Products, by L. Pasteur, *Member of the Chemical Society of Paris.*

(Continued from p. 204.)

III. Here we encounter a fact worthy of fixing the attention, even though we should regard it alone and isolated from the whole of the considerations about to follow. It is this:—

All artificial products of the laboratory, and all mineral species, have a superposable image. On the contrary, most of the natural organic products (I might say all, if I had to name only those which play an essential part in the phenomena of animal and vegetable life), all products essential to life are dissymmetric, and of the dissymmetry which causes their image to be non-superposable.

Before proceeding further, I desire to set aside some objections which cannot fail to present themselves to your minds.

IV. Quartz, you will say, is one. We have seen in the last lecture that quartz possesses the two characters of dissymmetry,—the hemihedrity in form observed by Haüy, and the rotary phenomena discovered by Arago. Nevertheless, all molecular dissymmetry is absent in quartz. To understand it, let us advance a little further in the knowledge of the phenomena we are considering. We shall find, moreover, the explanation of the analogies and differences already previously indicated between quartz and natural organic products.

Permit me to describe roughly, though in the main accurately, the structure of quartz and that of natural organic products. Imagine a winding stair, the steps of which shall be cubes, or any other object with a superposable image. Destroy the stair, and the dissymmetry will have disappeared. The dissymmetry of the stair was the result only of the mode of putting together its elementary steps. Such is quartz. The crystal of quartz is the stair all built. It is hemihedric. For this reason it acts on polarised light. But let the crystal be dissolved, melted,—its physical structure destroyed in any manner,—and its dissymmetry is suppressed, and with it all action on polarised light, as would happen, for example, in a solution of alum, a liquid formed of molecules of a cubic structure distributed without order.

Imagine, on the contrary, the same winding stair formed of irregular tetrahedrons for steps. Destroy the stair, and the dissymmetry will still exist, because you have to deal with an assemblage of tetrahedrons. They may be in any position, but each one of them will have its proper dissymmetry, nevertheless. Such are organic bodies in which all the molecules have a proper dissymmetry, which is translated in the form of the crystal. When the crystal is destroyed by solution, there results from it a liquid active for polarised light, because it is formed of molecules pell-mell, it is true, but each having a dissymmetry in the same direction, if not of the same intensity in all directions.

V. Quartz, then, is not molecularly dissymmetric, and, up to the present time, we do not know any example of a mineral which possesses molecular dissymmetry. I have said that we must extend this proposition to the artificial compounds of laboratories. Here still we may have some scruples. We may object, for example, that native camphor, which is dissymmetric, yields artificially camphoric acid, also dissymmetric; that aspartic acid is derived from asparagine,—and I might cite many other like examples. But no one doubts that camphoric and aspartic acids owe to camphor and to asparagine their proper dissymmetry. This exists in the mother products, and it is transferred, modified more or less by substitution, from the mother products to their derivatives. We are unable to produce better evidence in general, of the preservation of the primitive type in a series of products connected by a common origin, than the permanence of the optic property.

When I affirm that no artificial substance has yet presented molecular dissymmetry, I mean to speak of artificial substances, properly so called, formed entirely of mineral elements, or derived from non-dissymmetric bodies. For example, alcohol is not dissymmetric. Its moleculi, if we could isolate and study it, placed before a mirror would present an image superposable to it. Now, there is no derivative from alcohol which is not dissymmetric. I could multiply examples of this kind without end. Further, take any dissymmetric body, and if you subject it to somewhat energetic chemical reactions, you will assuredly see the dissymmetry of the primitive group disappear. Thus, tartaric acid is dissymmetric. Pyrotartaric acid is no longer so. Malic acid is dissymmetric. Malëic and paramalëic acids of M. Pelouze are not. Gum is dissymmetric, mucic acid is not.

Artificial products, then, have no molecular dissymmetry. I do not know how to indicate the existence of a more perfect separation between the products originating under the influence of life and all others. We insist a little, because you will see in the sequel of this lecture the physiological aspect of these studies disengages itself more and more. Let us pass in review the principal classes of natural organic products.

Cellulose, feculæ, gums, sugars, tartaric, malic, quinic, tannic acids, morphia, codeia, quinia, strychnia, brucia, essence of turpentin, of lemon, albumen, fibrin, gelatin. All these immediate principles are molecularly dissymmetric. All these substances have the rotary power when in solution; a character necessary and sufficient to establish their dissymmetry, even when, in the absence of possible crystallisation, hemihedrity would be wanting for the recognition of this property.

All the substances most essential to the vegetable and animal organism figure in this enumeration.

There are many natural substances which are not dissymmetric. But are they natural in the same sense as the others? Do we not necessarily see in such bodies as oxalic acid, hydruret of salicyle, fumaric acid, the derivatives of natural substances properly so called, formed by actions analogous to those of the laboratory? These products seem to me to be in the vegetable organism what urea, uric acid, creatine, glycocol, are in the animal organism,—rather excretions than secretions, if I may so speak. It will be very interesting to follow this point of view experimentally.

We may add to this, that many bodies, non-dissymmetric in appearance, may be *paratartarics*. A word is wanting in chemical language to express the fact of a double molecular dissymmetry concealed by the neutralisation of two inverse dissymmetrics, the physical and geometric effects of which rigorously compensate each other.

The double proposition to which we have just been led upon the habitual dissymmetry of immediate organic principles, and upon the absence of this character in all the products of inorganic nature, enables us to enlarge and render more and more precise our manner of viewing the subject of this remarkable molecular property.

VI. In 1850, M. Dessaignes, whose ingenious skill is known to chemists, announced to the Academy that he had succeeded in transforming the bimalate of ammonia into aspartic acid. It was a step which tended to confirm the important results that M. Piria had obtained some years previously. M. Piria had succeeded in transforming asparagine and aspartic acid into malic acid. M. Dessaignes in his turn showed inversely that malic acid could be reformed into aspartic acid.

Thus far there was nothing but what was very natural in the observation of M. Dessaignes, even in the optical point of view. For my part, I had recognized that asparagine, aspartic acid, and malic acid, were active on polarised light. The chemical passage of one of these bodies to the others was not surprising.

Some months later, M. Dessaignes made another step forward. He announced that not only the bimalate of ammonia, but also the fumarate and maleate of ammonia had equally the property of being transformed by heat into aspartic acid.

Here I perceived an impossibility; or, if the thing was as M. Dessaignes asserted, this skilful chemist had made a discovery which he did not suspect. Indeed, I observed that fumaric and malic acids and all their salts were without action on polarised light. If, then, M Dessaignes had succeeded in transforming their salts of ammonia into aspartic acid, he must have realised for the first time the production of a dissymmetric body by the aid of compounds which are not.

But it appeared to me more reasonable to believe that the aspartic acid of M. Dessaignes differed from natural aspartic acid decidedly in the absence of the molecular rotary property. M. Dessaignes, it is true, had carefully compared the properties of the artificial acid with those of the natural acid, and, as he said, found them identical. Better than any one, from the example of M. Mitscherlich, of whom I spoke at our last meeting, I knew how delicate is the determination of the identity of chemical species in studies in which the greatest similitude of properties often conceal profound differences. I did not hesitate to believe, therefore, that the new fact announced by M. Dessaignes had need of confirmation.

I attached so much importance to the elucidation of this question, and in the anticipation even of the results which I am about to have the honour to lay before you, that I immediately made a journey to Vendome, where I submitted my doubts to M. Dessaignes, who at once presented me with a specimen of his aspartic acid. On my return to Paris, I found immediately, in fact, that the acid of M. Dessaignes was only an isomer of the natural aspartic acid, that is, the acid derived from asparagine, and that it differed from it, as I had foreseen, by the rotary property, entirely absent in the artificial acid, not at all doubtful in the natural acid. But all the other physical and chemical properties possessed the greatest analogies, so great that M. Dessaignes, who was not put on his guard by any preconceived idea, had concluded that the two substances are really identical.

What attracted me most in the examination of the new compound (which by itself offers no very remarkable crystallisable combinations), was its transformation into malic acid. It is, indeed, known that M. Piria, just mentioned, long ago gave the means of obtaining malic acid from asparagine and aspartic acid, and I was assured by the most exact proofs that this malic acid was identical with that of the sorbus, the apple, the grape, and tobacco.

I then treated the new acid in the manner discovered by M. Piria, and transformed it into a new malic acid very similar to the natural acid, so close to the latter, that a chemist would have great difficulty in distinguishing them even if warned of their real dissimilarity; only this malic acid had no action on polarised light, and it was the same with all its saline combinations.

There are certain derivations of these two malic acids, the comparison of which does not very clearly manifest the true mutual dependence of the molecular arrange-

ments of these curious isomerics; but there are others in which it is plainly seen. Let us consider, for example, the ordinary active bimalate of lime, and the corresponding inactive bimalate. Their chemical composition is exactly the same, and their crystalline forms are alike, with this difference, that the form of the active bears four small hemihedric faces, always absent in the form of the inactive. Whence it results that placed before a glass the image of the active cannot be superposed on it, while the image of the inactive is absolutely identical and superposable to the reality which gives it. Excepting the hemihedric faces, there is a perfect resemblance between the two forms.

Who could doubt, after that, the relations of molecular arrangements of these two salts? Is it not evident that we have here to deal with a malic acid identical to the natural, except the simple suppression of its molecular dissymmetry?

This is the natural malic acid untwisted, if I may so express it. The natural acid, in the arrangement of its atoms, is a winding stair; this one is the same stair, formed of the same steps, but straight, instead of being spiral.

It might now be asked if the new malic acid is not the paratartaric of the series; that is, the combination of the right malic acid with the left malic acid. That is scarcely probable; for then, not only with an inactive body we would have made an active body, but would have made two, one right and one left. Besides, I have ascertained that just as there exists a non-dissymmetric inactive malic acid, so there is also a non-dissymmetric inactive tartaric acid, very different from paratartaric acid, and which cannot be resolved into right tartaric and into left tartaric acids. Here it cannot be doubted that we have to deal with right or left tartaric acid rendered non-dissymmetric.

I have also discovered inactive amylic alcohol, which yields a whole series of inactive products corresponding to the series of active amylic alcohol.

Thanks to the discovery of inactive bodies, we are in possession of a fruitful idea: a substance is dissymmetric, right or left, by certain modes of isomeric transformations, which must be sought and discovered in each particular case; it may lose its molecular dissymmetry, untwist itself, to use a coarse illustration, and effect in the arrangement of its atoms a disposition with a superposable image. So that every dissymmetric substance offers four varieties, or, rather, four distinct sub-species: the right body, the left body, the combination of the right and the left, and the body which is neither right nor left, nor formed by the combination of the right and left.

(To be continued.)

On the Effect of Increased Temperature upon the Nature of the Light Emitted by the Vapour of certain Metals or Metallic Compounds, by Professors* ROSCOE *and* CLIFTON.

IN a letter communicated to the *Philosophical Magazine* for January last, we stated that in examining, with Steinheil's form of Kirchhoff and Bunsen's apparatus, the spectra produced by passing the induction spark over beads of the chlorides and carbonates of lithium and strontium, we had observed an apparent coincidence between the blue lithium line, which is seen only when the vapour of this metal is intensely heated, and the common blue strontium line called Sr. δ. We further stated that on investigating the subject more narrowly by the application of several prisms and a magnifying power of 40, we came to the conclusion that the lithium blue line was somewhat more refrangible than the strontium δ, but that two other more refrangible lines were observed to be coincident in both spectra. Having constructed a much more perfect instrument than we at that time possessed, we are now able to express a definite opinion on the subject, and beg to lay a short notice of our observations before the Society. Our instrument is in all essential respects similar to the magnificent apparatus employed by Kirchhoff in his recent investigations on the solar spectrum and the spectra of the chemical elements. It consists of a horizontal plane cast iron plate, upon which three of Steinheil's Munich prisms, each having a refracting angle of 60°, are placed; and of two tubes fixed into the plate, one being a telescope having a magnifying power of 40, movable with a slow-motion screw about a vertical axis placed in the centre of the plate, and the other being a tube carrying at one end the slit, furnished with micrometer screw, through which the beam of light passed, and at the other end an object-glass for the purpose of rendering the rays parallel. The luminous vapours of the metals under examination were obtained by placing a bead of the chloride or other salt of the metal on a platinum wire, between two platinum electrodes, from which the spark of a powerful induction coil could be passed. In order to obtain a more intense, and therefore a hotter, spark than can be got from the coil alone, the coatings of a Leyden jar were placed in connection with electrodes of the secondary current respectively. When this arrangement was carefully adjusted, the two yellow sodium lines were observed to be separated by an apparent interval of two millimètres, as seen in the least distance of distinct vision.

The position of the blue line, or, rather, blue band of lithium, was then determined with reference to the fixed reflecting scale of Steinheil's instrument, by volatilising the carbonate of lithium, in the first place on a platinum wire between platinum electrodes, and secondly, on a copper wire between copper electrodes. A bead of pure chloride of strontium was then placed on new platinum and copper wires between two new platinum and copper electrodes, and the position of the blue line Sr. δ read off upon the same fixed scale; a difference of one division on the scale was seen to exist between the positions of the two lines, the lithium line being the more refrangible. The salts of the two metals were then placed between the poles at the same time, and both the blue lines were simultaneously seen, separated by a space about equal to that separating the two sodium lines. When experimenting with this complete instrument, we were unable to observe any other blue lines in the pure lithium spectrum than the one above referred to; we have, however, noticed the formation of four new violet lines in the intense strontium spectrum, and we now believe that the other two lithium lines mentioned in our letter to the *Philosophical Magazine* are caused by the presence of the most minute traces of strontium floating in the atmosphere, and derived from a previous experiment. We have convinced ourselves by numerous observations that the currents of air caused by the rapid passage of the electric spark between the electrodes are sufficient to carry over to a second set of electrodes placed at the distance of a few inches, a very perceptible quantity of the materials undergoing volatilisation. The greatest

* Read at the the last meeting of the Manchester Literary and Philosophical Society.

precautions must hence be taken when the spectra of two metals have to be compared; and no separate observations of the two spectra can be relied upon, unless one is made a considerable space of time after the other, and unless all the electrodes which have been once used are exchanged for new ones.

Kirchhoff, in his interesting "Memoir on the Solar Spectrum and the Spectra of the Chemical Elements,"† noticed in the case of the calcium spectrum that bright lines which were invisible at the temperature of the coal-gas flame became visible when the temperature of the incandescent vapour reached that of the intense electric spark.

We have confirmed this observation of Kirchhoff's, and have extended it, inasmuch as we, in the first place, have noticed that a similar change occurs in the spectra of strontium and barium; and, in the second place, that not only new lines appear at the high temperature of the intense spark, but that the broad bands characteristic of the metal or metallic compound at the low temperature of the flame or weak spark, totally disappear at the higher temperature. The new bright lines which supply the part of the broad bands are generally not coincident with any part of the band, sometimes being less and sometimes more refrangible. Thus the broad band in the flame-spectrum of calcium named Ca β, is replaced in the spectrum of the intense calcium-spark by five fine green lines, all of which are less refrangible than any part of the band Ca β; whilst in place of the red or orange band Ca α, three more refrangible red or orange lines are seen. The total disappearance in the spark of a well-defined yellow band seen in the calcium spectrum at the lower temperature, was strikingly evident. We have assured ourselves by repeated observations that, in like manner, the broad bands produced in the flame-spectra of strontium and barium compounds, especially Sr α, Sr β, Sr γ, Ba α Ba β, Ba γ, Ba δ, Ba ϵ, Ba η, disappear entirely in the spectra of the intense spark, and that new bright non-coincident lines appear. The blue Sr δ line does not alter either in intensity or in position with the alterations of temperature thus effected, but, as has already been stated, four new violet lines appear in the spectrum of strontium at the higher temperature.

If in the present incomplete condition of this most interesting branch of inquiry we may be allowed to express an opinion as to the possible cause of the phenomenon of the disappearance of the broad bands and the production of the bright lines, we would suggest that at the lower temperature of the flame or weak spark, the spectrum observed is produced by the glowing vapour of some compound, probably the oxide, of the difficultly reducible metal; whereas, at the enormously high temperature of the intense electric spark, these compounds are split up, and thus the true spectrum of the metal is obtained.

In conclusion, we may add that in none of the spectra of the more easily reducible alkaline metals (potassium, sodium, lithium) can any deviation or disappearance of the maxima of light be noticed on change of temperature.

On a Means of Increasing the Intensity of Metallic Spectra, by WILLIAM CROOKES.

I HAVE for some years been in the habit of employing the metallic chlorates for the purpose of obtaining very brilliant spectra with an ordinary gas flame. It is difficult to imagine anything more gorgeous than some of the spectra thus produced. A small lump of chlorate of baryta, supported within the sixteenth of an inch of a colourless gas flame, decomposes with decrepitation, imparting its peculiar green and red tints to the flame, and exhibiting in the spectroscope an appearance perhaps unsurpassed in the whole range of metallic spectra. Each line stands out with extreme brilliancy, and a multitude of new lines, both green and red, come into view. A fused bead of chlorate of potash being introduced into the flame, decomposes at first with less rapidity than the baryta salt, but after a few moments it goes off with almost explosive violence, whilst at the same time that portion of its spectrum which is usually continuous splits up in parts into numerous fine lines. Chlorate of soda sometimes gives an appearance of inversion, the usual line appearing dark on a dazzlingly brilliant ground. On shading off the more luminous portion of its spectrum, traces of lines were visible in the more refrangible portion.

Chlorate of lithia gives, in addition to the red and orange line, the very brilliant blue line observed in its electric spectrum. Another more refrangible blue line is also seen when the ignition is at its greatest intensity. Chlorate of strontia shows, in addition to brilliant lines in the lower portion of its spectrum and the well-known blue line, three other blue or violet lines some distance apart. Chlorate of lime gives the blue band very brilliantly, and several other lines hitherto unrecorded.

Besides these, many other metals show brilliant and characteristic spectra when ignited in the form of chlorates. Copper is especially vivid, and its spectrum is remarkable for changing its appearance according to the state of decomposition of the chlorate, the final spectrum differing in several respects from the one observed when the salt is first introduced into the flame. The lead and cadmium spectra are also very beautiful, and exceedingly definite.

It will be seen from the above notes that the employment of the chlorates gives us a ready means of producing spectra of an intensity which approaches that high degree hitherto only obtainable by the employment of the electric spark; the blue strontium and lithium lines mentioned by Professors Roscoe and Clifton being readily visible by this means.

The chlorates may be readily prepared by precipitating chlorate of baryta (which may be obtained in commerce) by the desired sulphate or carbonate, and evaporating the filtrate; or by adding to it the equivalent quantity of sulphuric acid, filtering through asbestos or gun-cotton, and neutralising the resulting chloric acid with the metallic carbonate or oxide.

Unfortunately, I have not succeeded in obtaining any characteristic spectrum from arsenic in any of its compounds. A delicate spectrum test for this metal would be of the highest importance to toxicologists, but it appears, with the means I have employed to examine it, to give a continuous spectrum. Antimony, likewise, gives no better results.

† "Kirchhoff on the Solar Spectrum," &c. Translated by H. E Roscoe. Macmillan, Cambridge. 1862.

Royal Institution. — Tuesday, April 29, at Four o'clock, C. T. Newton, Esq., "On Ancient Art." Thursday, May 1, at Two o'clock, Annual Meeting. Friday, May 2, at Eight o'clock, R. M. Milnes, Esq., M.P., "On the International Exhibition." Saturday, May 3, at Three o'clock, Professor Anderson, "On Agricultural Chemistry."

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, April 17, 1862.

Dr. A. W. Hofmann, F.R.S., President, in the Chair.

A Paper, by Dr. E. Divers, "*On the Action of Bicarbonate of Ammonia upon the Salts of Magnesia,*" was read by the Secretary. It had usually been supposed that magnesia was only partially precipitated from solution by carbonate of ammonia, and not at all in the presence of a sufficient quantity of ammoniacal salt; this, however, was not strictly correct, for on adding a solution of carbonate of ammonia to a solution of sulphate of magnesia a precipitate was produced, and this took place even in the presence of a large quantity of chloride of ammonium. If an excess of carbonate of ammonia were added, the precipitate consisted of a double carbonate of magnesia and ammonia; to produce this double compound, at least four equivalents of carbonate of ammonia for every equivalent of magnesia were necessary. In a very dilute solution no precipitate was produced, unless a considerable excess of carbonate of ammonia were added; but after this addition a precipitate was produced, when the solution contained only $\frac{1}{5000}$th part of magnesia. The precipitated double carbonate contained single equivalents of magnesia and ammonia. The solubility of the precipitate in water and ammoniacal salts was then tried. On washing a portion of the salt on a filter with water, part was dissolved, while the remainder was decomposed into carbonate of ammonia, which dissolved, and carbonate of magnesia, which remained on the filter. On projecting the salt gradually into a large quantity of water, it dissolved to some extent, but as soon as the water was saturated it began to decompose the excess of salt. A solution prepared in this manner might be kept for some time without decomposing; but the addition of carbonate of ammonia produced a precipitate. A solution of chloride of ammonium dissolved a smaller quantity of the salt than pure water; and it was almost insoluble in water containing carbonate of ammonia. It was desirable to have a process for preparing this double salt with facility, as it promised to be useful in medicine.

The President said that it was curious that the insolubility of carbonate of magnesia in ammoniacal salts should have been overlooked for so long a time, but the probable reason was that the precipitation only took place after some time.

Professor Graham remarked that it was not mentioned in the Paper which had just been read, what carbonate of ammonia was used, but that it was probably the sesquicarbonate; he considered that carbonic acid might be viewed as a tribasic acid, for a compound might be prepared containing the three bases—potash, magnesia, and water—combined with three equivalents of carbonic acid; sesquicarbonate of soda was also an analogous compound.

The President remarked that the bibasic character of carbonic acid was borne out by the manner in which it was set free during the action of reagents on organic compounds; under these circumstances two equivalents of carbonic acid were almost always evolved; also, that with respect to the precipitation of magnesia, the solution was generally rendered very alkaline with ammonia before precipitation, under which condition the carbonate would not perhaps form so readily; he also stated that Mr. Watts had written a note on the Paper to the effect that this precipitation of magnesia by carbonate of ammonia was referred to in Rose's "Handbook of Analysis."

The President then made a verbal communication on some organic compounds obtained from the refuse of the manufacture of aniline. The products obtained at different aniline works varied to a considerable extent in their nature; and this variation appeared to be due to a difference in the hydrocarbons employed in the manufacture; for in England, that portion of the coal oil which boiled below 100°C. was employed, but at some works in France, all that distilled below 120°C. was considered fit for the purpose; in fact, there existed in coal-tar a substance (which had been examined by Mr. Church) having the same composition and vapour density as benzol, but differing from it in some of its properties, and especially in its boiling point, which was 98°C., so that only a small portion of this would distil below 100°C., the greater part being held back by the hydrocarbons possessing a higher boiling point. On treating this distillate by the method for preparing aniline, a great number of substances were obtained; these had not as yet been completely examined; they might be separated by fractional distillation. Towards the end of the distillation two fatty bodies passed over, which were so viscid that the vessel containing them might be inverted; but they did not crystallise. They both, however, formed crystallised salts with acids, and the sulphate of one of the bases was almost insoluble in water, the other sulphate being soluble; by this means the two bases might be separated; the salts of that forming the more soluble sulphate were fluorescent, and resembled those of uranium in colour; it was isomeric with aniline, but combined with only half as much acid, so that the formula would have to be doubled, the composition of the hydrochlorate being $C_{12} H_{14} N_2$, HCl. The composition of the other base was expressed by the formula $C_{12} H_{11} N$, the hydrochlorate containing $C_{12} H_{11} N$, HCl; this agreed in composition with diphenylamine, a substance which could not be prepared by a process similar to that employed for the formation of the corresponding ethyl compounds, since the chloride of phenyl had no action on ammonia.

NOTICES OF BOOKS.

On Pepsine. By M. Boudault. Translated by W. S. Squire, Ph.D., F.C.S. Second Edition. Churchill, London.

This is the translation of a paper read by M. Boudault before the Paris Academy of Medicine. The Parisian Scientific Societies resemble in one respect our Society of Arts, and also some of our Medical Societies. Any person with an invention to make public, reads a paper before these Societies, investing the subject with a certain amount of scientific interest to save appearances, and then the communication is forthwith published, the implied authority of the Society being found to be a first-rate advertisement.

M. Boudault's paper is a very good account of the properties of natural pepsine, or rather gastric juice, concluding with a recommendatory notice of Pepsines, Nos. 1, 2, 3, and 4, prepared by the author.

An account of the manner in which pepsine is said to be made will be found at page 54, vol. ii., of the Chemical News, in an interesting paper by Mr. Napier, of Dublin, to which we refer any of our readers to whom the subject may be a novelty, with the additional information that pepsine is now principally obtained from the stomachs of pigs, and an enterprising man would no doubt do well if he set up a laboratory at Cork or Waterford. The anatomists of the School of Salerno, in the absence of human "subjects," dissected the bodies of swine, "as likest the human form divine"; and according to some chemists, the gastric juice of the same quadrupeds bears the nearest resemblance to that of man.

The value of pepsine (manufactured pepsine we mean) as a medicine is differently estimated by medical men. Dr. Chambers, in a lecture appended by Dr. Squire to M. Boudault's paper, praises it highly; while Dr. Leared, in

his work on "Imperfect Digestion," altogether denies its value. We cannot undertake to arbitrate in the matter, but infer that pepsine sometimes does good and sometimes does not. Dr. Squire has done well to bring M. Boudault's paper under the notice of the profession, and medical men will judge for themselves. What natural pepsine is they will learn distinctly enough from this paper. What commercial pepsine is and has been, is a different question, on which if we had time and space a good deal might be said.

NOTICES OF PATENTS.

1364. *Paper.* E. Hartnall, Ryde, Isle of Wight. Dated May 31, 1861. (Not proceeded with.)

It is proposed to employ in the manufacture of paper a certain kind of sea-weed called *zostera marina*, which is very abundant on our shores and occurs in the form of pale green, rounded concretions. This vegetable production is known also under a great variety of designations, such as "grass-wrack," "oar-weed," sea-apples," &c.; and under the French synonym of "*Pommes de mer*," a patent was granted for the same application to M. Mennons,* on May 3, 1861, who appears thus to have anticipated Mr. Hartnall's invention.

1367. *Alkaline Carbonates.* R. Laming, Priory Road, Kilburn. A communication. Dated May 31, 1861. (Not proceeded with.)

The inventor employs the sulphate of soda or potash as the starting-point from which to manufacture the carbonates of these bases. He mixes the sulphate with any suitable carbonaceous matter, and prepares a crude alkaline sulphide by exposing this mixture to a high temperature in a crucible or convenient furnace. The product obtained in this manner is charged into a long tube and steam mixed with air passed over it as long as sulphuretted hydrogen continues to be evolved, when the residual product is removed and treated with water to dissolve out the carbonate of soda or potash, which may then be purified by crystallisation or in any other way.

1382. *Obtaining Products from Coal.* W. A. Shepard, Pall Mall, London. Dated June 1, 1861.

For the purposes of this invention, comparatively large blocks of coal are introduced into closed chambers of fire-brick, iron, or other suitable material, where they are submitted to a process of slow distillation. At the lower part of this chamber is a small fire-place, which is separated from the main receptacle for the coal by a set of vertical bars or grating, thus furnishing a means of starting the process of distillation from without. From the back of the chamber, at different levels, are two outlets, which lead the products into separate condensing arrangements, the highly volatile matters, selecting by preference the upper exit pipe, and in order to facilitate the escape of these vapours a jet of steam is employed to establish a current in the required direction. At the lower outlet the tarry products and heavy oils are principally collected, these run off into a tank provided for their reception. The coal from which products are to be obtained is placed in an iron crate or basket, which corresponds in dimensions with the interior of the chamber, and the construction is so arranged that it may be lowered from above into the chamber, the upper part of which is then closed to prevent the escape of any products from the coal; or the front grate being movable the crate holding the charge may be run in or trucks. A fire having been kindled in front of the vertical grating, the flames pass through and ignite the lower portion of the charge, the coals in the chamber thus become coked gradually, and further from the fire as the process is continued, whilst the products are collected in the manner described.

An arrangement very similar to the above was that originally used for the purpose of obtaining paraffin and other matters from Irish peat. It was, however, more simple in construction, and provided with only one outlet for the escape of the volatile products.

* Patent No. 1109. Chemical News, vol. v. p. 97.

Grants of Provisional Protection for Six Months.

264. Edward Henry Cradock Monckton, Fineshade, Northamptonshire, "Improvements in the application of electricity for obtaining ammonia and other useful products during the combustion of coal and fuel, and in the apparatus employed therein."—Petition recorded 31st January, 1862.

614. Richard Wright, Albany Road, Cambe-well, Surrey, "Improvements in heating and clarifying saccharine fluids."

638. James Duncan, Greenock, Renfrewshire, N.B., "Improvements in the manufacture of vinegar."

704. George Bennet, Manchester Buildings, Westminster, Middlesex, "An improvement in the coating and covering of wrought iron for the purpose of preserving it and preventing oxidation."

715. George Brooks Pettit, New Oxford Street, London, "An improved method of and apparatus for heating water and other liquids, applicable also to the evaporation of liquids."

747. Marc Antoine François Mennons, Rue de l'Echiquier, Paris, "The application to the manufacture of paper pulps of a vegetable product not hitherto used for that purpose."—A communication from Fréderic Thierry Pitoy-Millot, Faubourg St. Pierre, Nancy, Departement de la Meurthe, France.

750. Henry Bailly, Salter's Hall Court, Cannon Street, London, "Improvements in the manufacture of paper from wood, and in apparatus used therein."—A communication from Ernest Antreux and Michel and Joseph Cotton, La Rochelle, Lower Charente, France.

822. Alfred Fryer, Manchester, "Improvements in the manufacture of sugar, and in separating liquids from sugar and other substances."

830. Leo de la Peyrouse, Panton Square, Middlesex, "Improvements in the preservation of animal substances."

Notices to Proceed.

2861. Henry Bird, Liverpool, "Improvements in the construction of bottles and other vessels, and in stoppers for the same to indicate whether they contain poison."—Petitions recorded 13th November, 1861.

2903. Theophilus Redwood, Montague Street, Russell Square, London, "Improvements in the manufacture of starch, and of a vegetable sizing powder."—Petitions recorded 19th November, 1861.

2972. Charles Stevens, Charing Cross, London, "An improved indelible anti-corrosive ink."—A communication from Louis Croc, Paris.

3108. William Henry Tooth, Rhodeswell Road, and William Yates, jun., Parliament Street, Westminster, London, "Improvements in the manufacture of iron and steel, and in the machinery, apparatus, or furnaces used therein, and for the production of gas to be employed in such manufacture."—Petition recorded 11th December, 1861.

264. Edward Henry Cradock Monckton, Fineshade, Northamptonshire, "Improvements in the application of electricity for obtaining ammonia and other useful products during the combustion of coal and fuel, and in the apparatus employed therein."—Petition recorded 31st January, 1862.

583. Henry Bunning, Field House, New Cross, Deptford, Kent, "Improvements in the manufacture of lubricating grease or compounds."—Petition recorded 3rd March, 1862.

289. Thomas Mossom Meekins, LL.D., Chancery Lane, London, "The production of a projectile and explosive force to be used in instruments of war, for an electric gas gun and electric gas shell, for a method of using the recoil of weapons for the purpose of increasing the pressure of elastic fluids for the production of a projectile force, for a method of rapidly loading weapons at the breech, and of a motive force to be used in an electric gas engine or other engines."—Petition recorded 4th February, 1862.

340. James Dickson, Tollington Road, Holloway, Middlesex, "Improvements in voltaic apparatus, and in the production of voltaic electricity."—Petition recorded 10th February, 1862.

700. John Kent, Hyson Green, Nottingham, "Improvements in cleansing and bleaching."—Partly a communication from Theodor Schnebely, Moscow.—Petitions recorded 13th March, 1862.

720. Henry Young Darracott Scott, Brompton Barracks, near Chatham, Kent, "Improvements in the manufacture of cement."

3002. Peter Spence, Newton Heath, Manchester, "Improvements in the treatment of ores for the manufacture of sulphuric acid, and in apparatus connected therewith."—Petition recorded 28th November, 1861.

3030. James Leach, Bronte Place, East Street, Walworth, Surrey, "Improvements in preparing matters to be used in the manufacture of candles."

CORRESPONDENCE.

Utilisation of Sewage.

To the Editor of the Chemical News.

Sir,—It would appear from your Correspondent's letter in the Chemical News of April 12, that this great "sanitary problem,"—viz., the "deodorisation and utilisation of the sewage of towns," which has for so many years successfully puzzled the heads of all scientific investigators, and baffled all commercial enterprise, has at length been solved by the Sanitary Manure Company, Manchester.

Taking a great interest in sanitary matters, and having chemically examined samples of their manure, I beg to offer a few remarks respecting it.

The Company's works are not yet, I believe, sufficiently advanced to supply large quantities of the manure to the agriculturalists. It is, therefore, to be hoped that many improvements will be introduced into its manufacture, so as to produce a "really valuable manure," instead of that now sent out as samples, which contains 31·25 per cent. of moisture, and 34 per cent. of mineral matters insoluble in acids. Thus we commence with 65·25 per cent. of useless material. Of the remaining, 34·75 per cent. consists of organic matter, ammonia salts, phosphates of lime, iron, magnesia, carbonate of lime, and all other constituents soluble in hydrochloric acid, of the human excrement, urine, ashes, &c.

The only truly valuable fertilising agents in this 34·75 per cent. are:—Ammoniacal nitrogenous organic matter, yielding 1·20 per cent. of ammonia; and phosphoric acid, in combination with lime, iron, and magnesia (as insoluble phosphates), equal to 5·7 per cent. of insoluble phosphate of lime. The other constituents of the manure are scarcely worth the expense of transport, which is the most important desideratum with manufacturers of artificial manures.

Commercially speaking, the manure may be said to contain two valuable fertilisers, ammonia and phosphates, but, unfortunately, these exist in such small proportions compared with the useless matter associated—viz., moisture, ashes, &c.—that it is rendered necessary that large proportions of this manure should be applied to the soil before any really beneficial action upon the crops can be expected.

The deodorising process is the best that I am acquainted with, the manure being destitute of the offensive smell which accompanies most manures containing nightsoil. I believe this Company to have taken a correct view of the great question of sewage, as their process will not only relieve our sewers and rivers from the nuisance so seriously complained of, but will make that which is now wasted valuable for agricultural purposes. If the Company would prepare their manure without the water, ashes, &c. (amounting to upwards of 65 per cent.), and were it supplied at a moderate price, it must create a demand from farmers. If this is found impracticable, then a corrective addition of bones, woollen waste, superphosphate of lime, animal matters, ammoniacal salts, and nitrate of soda, become necessary, in my opinion, before it can safely be asserted to be "a better and safer fertiliser than guano." The probability of this last clause I leave for more experienced agricultural chemists than myself to decide.

I trust the above remarks will lead the manufacturers of sewage manures a step in the right direction, by showing the necessity of preparing a richer fertiliser from sewage than has yet been produced. The admixture of ashes, rubbish, and water will assuredly withhold from them that support from agriculturalists and scientific men which is requisite to ensure remunerative success in this great sanitary enterprise.—I am, &c.

Fearnside Hudson,
Analytical and Consulting Chemist.

Laboratory, Corporation Street, Manchester, April 19.

Dangers and Detection of Carbonic Oxide.

To the Editor of the Chemical News.

Sir,—Carbonic oxide burns with a flame; but I would advise "W." not to experiment too much on it, but get rid of the stove. It is not generally known that carbonic acid gas is as respirable as chloroform vapour, and is now much used for irritable cough, &c., as a remedy; but carbonic *oxide* produces sudden giddiness. and acts as a deadly poison by destroying chemically the blood-globules of human blood in the vessels. It was carbonic oxide (CO) mixed with carbonic acid (CO_2) that proved so deadly to the poor Hartley Colliery miners.

The physiological tests of carbonic oxide, I rather think, are more discriminative than the chemical ones.

I am, &c.,

Charles Kidd, M.D.

Sackville Street, W. April 13.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Cast Iron.—MM. Minary and Résal have been experimenting on cast-iron for two years (*Comptes-Rendus*, t. liv., p. 212). They seem to have made the not very original discovery, that the object of the puddling operation is to burn off carbon; but some practical remarks are to be found in the paper, which, however, may not be new to all our readers. Grey or black cast iron, they say, containing no, or but little, oxide of iron, must have oxygen supplied in the refining by Bessemer's or some other process. The white crystalline cast-iron only requires a prolonged stirring. The granular white, containing a superabundance of oxide, is sooner refined, but never makes good iron. Bessemer's process, the authors say, should only be applied to grey irons. They mention, too, that the fusibility of iron increases with the proportion of oxygen it contains. Two crucibles. one with iron shavings only, and the other with iron shavings and a certain proportion of oxide of iron, were placed in a wind furnace. In the former, the shavings were only soldered together slightly; in the latter, they were fused to a button.

II. ORGANIC CHEMISTRY.

Synthesis of Glucosides.—When one equivalent of trichloride of benzine reacts at 160° on six equivalents of acetate of silver in acetic solution for thirty hours, the resulting acetic acid will be found to hold a body which reduces Barreswil's solution. If the acetic acid be saturated with carbonate of soda, and the solution is shaken up with ether, the etherial liquor on evaporation gives a coloured oil with a very bitter taste, and which reduces the cupro-potassic solution. This oil, says Rosenstiehl (*Comptes-Rendus*, t. liv. p. 178), appears to be a mixture of several glucosides; but heated on a water bath with dilute sulphuric acid, the glucosides most easily altered are destroyed, and on cooling small crystals are deposited. They are soluble in all sorts of solvents, have an extremely bitter taste, and reduce Barreswil's solution just like glucosides. When boiled with dilute sulphuric acid they evolve acetic acid. Their analysis led the author to construct the following formula to express their composition :—

$$\left.\begin{matrix} C_{24}H_{12} \\ 3\,(C_4H_3O_2) \\ Cl_9 \end{matrix}\right\} O6$$

Two equivalents of the trichloride of benzine, and three equivalents of acetate of silver, treated in the same way as before, gave a body like a true fat. The author seems to have entered upon a very hopeful course of experiments, and thinks soon to obtain glucose itself.

III. ANALYTICAL CHEMISTRY.

Errors in the Estimation of Nitrogen by Combustion with Soda Lime. — E. Mulder points out (*Chem. Centralblatt*, 1862, s. 44) some sources of error in the method of Will and Varrentrap. The cork may absorb some of the ammonia, and if the hydrochloric acid in the bulbs is not sufficiently dilute, chloride of ammonium may escape with the hot steam. He recommends that the substance to be burned should first be mixed with soda lime in fine powder, and this mixture with a larger quantity of granulated soda lime. An ammoniacal salt must be first mixed with carbonate of lime. The cork he covers with tin-foil, and for a nitrogen apparatus he uses a U tube filled with fragments of glass moistened with hydrochloric acid. Dr. Knop remarks that the column of soda lime should not be too long, for he has satisfied himself that when the column is long and is too strongly heated, some of the ammonia is decomposed. The error, however, is always small.

MISCELLANEOUS.

CURIOSITIES IN CHEMICAL EVIDENCE.

DOWNING *v.* CHANCE.

From a letter which appeared in our last Number it will be seen that Mr. Alfred Bird denies the accuracy of the statements attributed to him in the leading article and report of the above trial given in the Chemical News of the 5th instant.

We have now before us a transcript of the circuit shorthand writer's notes, and we feel it necessary in order to vindicate our own and our reporter's correctness, to print that portion of Mr. Bird's cross-examination to which allusion was made :—

Mr. Serjeant Pigott: *Q.* Let me ask you, in your judgment does the result of your experiment amount to this, that your test paper had received the muriatic acid, or hydrochloric acid gas, and nothing else? *A.* I am quite sure of that. *Q.* And that nothing else would have produced the same result? *A. Nothing else.* *Q.* Do you known what cyanogen is? *A.* Yes. *Q.* Would that have produced the same result? *A. It was not necessary to look for it!* *Q.* That is no answer, you know, to my question. Would it produce the same result? *A.* It might have thrown down a white precipitate. *Q.* If it might do so, would it do it? *A.* If cyanogen had been present. . . . . *Q.* What would have been the appearance? *A.* In the first place, there was ammonia in the paper. *Q.* Would not the cyanogen have combined with that? Would it or not according to your analytical chemical knowledge? *A.* It *might* have combined if it were present. *Q.* We do not deal in mights; it either will or it will not. Will it? *A.* It *might* have combined with it if it were present. *Q.* All of us who know nothing about it could say it might, and could say it might not. We go to you learned men to say whether it would or not.

Mr. Baron Channell: What is cyanogen? *A.* Cyanogen is not a constituent of common salt. . . . .

Mr. Sergeant Pigott: Will you tell me what cyanogen is? *A. It is a compound of two gases.* *Q.* What two? *A.* It is a compound of *nitrogen* and *hydrogen.* *Q.* Is not it an element? Tell me what you say it is? I am told it is not an element. *A. It is a compound of two gases.* *Q.* What two? *A.* It is a compound of two gases, *nitrogen* and ——. *I cannot just refresh my memory as to the other.* *Q.* Is it nitrogen and hydrogen? *A.* The subjects are so entirely at variance with the existence of hydrochloric acid. *Q.* I admit they are difficult. *A.* We might assume the presence of fifty elements, and it does not necessarily follow that I should be able at a moment's notice to give what they are composed of. *Some of the compounds are so excessively difficult that it would be perfectly impossible without scientific books to enter into their composition.* *Q.* Very well, that is an admission. Cyanogen can be analysed, and, inasmuch as you are an analytical chemist, you can tell me what it is composed of, or cannot you do so? *A.* I rather decline to answer that question. It has nothing whatever to do with the thing, and I might perhaps make some little slip for which I have no doubt my friend at your elbow, who has got his dictionary with him, would be down upon me. I came here to prove the existence of hydrochloric acid. *Q.* That is very candid indeed of you. *A.* If I take his place when he gets into the witness-box, perhaps I may catch him out.

Mr. Huddlestone: We will have our dictionary and ask him some hard words, and ask him to define them. . . .

Mr. Sergeant Pigott: Chlorine is given off in the brick works in abundance, is it not? *A. Yes, but I cannot say as to that positively.* *Q.* Is cyanogen given off in the iron works? *A.* I believe cyanide of potassium has been found, therefore cyanogen may in some cases be found. I believe it is a matter of doubt whether it is always found. *Q.* You would not tell me what cyanogen was composed of before, you know? *A.* I declined to go into its composition. *Q.* Why did you decline? You have no conscientious motives, have you? *A.* No; on these sort of things you require to speak with absolute certainty; and being under cross-examination, it is evident that the smallest lapsus of memory might involve me in perplexity, and as I am quite certain it does not affect the merits of the case I should rather decline to go into it.

When a practical chemist is capable of getting into the witness-box and exposing such ignorance of the principles of his science, there is little wonder that the evidence of experts is viewed with such suspicion by a jury.

ANSWERS TO CORRESPONDENTS.

Carob, or Locust Bean.—A correspondent wishes to be referred to a reliable analysis of this bean.

D. Blackly.—We will refer to the paper and give you the information as soon as found.

Book Received.—"School Chemistry." By R. Dundas Thompson, M.D. Second Edition. London: Longmans.

THE CHEMICAL NEWS.

VOL. V. No. 126.—May 3, 1862.

CHEMISTRY AT THE INTERNATIONAL EXHIBITION.

THE display of chemicals at this year's International Exhibition is the finest that has yet been collected together. Not only are the exhibitors more numerous than in 1851, but there are more first-class names on the list, hardly one manufacturer of eminence being absent. In our leading branches of chemical manufactures the show is wonderfully good, great pains having been taken by the Committees and the Superintendent of the Class to procure their adequate representation. The specimens of alkalies, alum, and the coal-tar dyes generally, constitute the great bulk of the exhibition. Looking hastily through the range of cases, we meet the familiar names of Allhusen and Co., Chance Brothers, the Jarrow Chemical Company, Hutchinson and Earl, Mussprat, the Walker Alkali Company, Gaskell, Deacon, and Co., and several others equally famous in the manufacture of alkali. The samples sent by these firms are fine in the extreme, some of the soda crystals being almost perfect in form. When all are so good it would be invidious to choose; but the samples of mono- and bicarbonate contributed by Hutchinson and Earl, and Gaskell, Deacon, and Co., appear to be as near perfection as they possibly can be. Mr. Peter Spence has sent a splendid cone of alum, weighing nearly five tons. A hole has been cut in the side in order to render the interior visible. Another cone of equal size and excellence has been contributed by the Metropolitan Alum Company, and forms the most prominent object in the trophy, which has been arranged by Mr. S. Howard, the Chairman of the Metropolitan Committee, and Mr. C. W. Quin, the talented and energetic Superintendent of the Class. A very fine but small crystal of alum, sent by Bray and Thompson, has already attracted great notice. In coal-tar dyes, Messrs. Perkin are of course foremost. Their collection illustrative of the manufacture of mauve is very complete. They are almost eclipsed in colour, although not in excellence, by Simpson, Maule, and Nicholson, who exhibit a magnificent crown of crystallised acetate of rosaniline, which presents the most dazzling appearance, and will soon rank amongst the lions of the Exhibition. In their wake follow Roberts, Dale, and Co. (who also exhibit some fine samples of oxalic acid prepared from sawdust), Rumney, Holliday, Dawson, Judson, Allen, and several others. Indigo and the lichen dyes are also well represented, the collections of Haworth and Brook, Pincoff, Wood and Bedford, Marshall and Sons, B. Smith and Sons, and Haas, being especially good. The collection of specimens illustrating the improvements made in dyeing and calico-printing since 1851, formed by Mr. R. Rumney at the request of Her Majesty's Commissioners, affords a wonderful proof of the activity displayed in this branch of chemical manufactures during the last eleven years. It includes the very fine collection of madder products formed by Dr. Schunck, and exhibited by him before the British Association at Manchester last year. Other chemical manufactures are well shown; some sulphate of copper in gigantic crystals, by Buck and Co., of Manchester, a sulphate of iron crown, by Buckley's trustees, excellent specimens of prussiates, by the Hurlet and Campric Alum Company, bichromate, by White and Co., and prussiates, by Bramwell and Co.,—are extremely fine. In fine and rare chemicals, Huskisson's iodides, Morson's lithia and nickel salts, Foot and Co.'s acids, the iodine products by Ward and Co., and a series of interesting products by Hopkin and Williams, are only a few of the fine displays in this branch. Dr. Stenhouse, Dr. Müller, and Mr. Church contribute interesting products chiefly discovered by themselves, the specimens of orceine and nitrotheine in the collection of Dr. Stenhouse being wonderfully pure and perfect. Mr. Crookes exhibits the new element, thallium, and several of its compounds. A series of bile products, by Bullock and Reynolds, are likewise very fine. Chemical colours and pigments receive adequate representation at the hands of Winsor and Newton, who send over 1000*l.* worth of real ultramarine; J. W. Smith, whose Magenta lake is extremely beautiful, and a number of others. Bailey exhibits some very fine porcelain colours, and Emery and Co.'s case of specimens of these materials is a perfect gem in the taste shown in its arrangement. Wilkinson and Heywood, Mander Brothers, Wallis Brothers, and Rea, send some fine samples of gums and varnishes. Lucifer matches are well represented by five or six of the first houses in the trade; and the familiar names of Everett, Day and Martin, and several others, are here in all their sable glories. Laundry starch is contributed by nine or ten well-known houses, amongst whom may be mentioned Orlando Jones and Co., J. and J. Colman, S. Berger and Co., Broomhall, Wotherspoon, Rickett, Stiff and Fry, and many more. One of the great features in the class is the splendid collection of drugs formed by the Pharmaceutical Society, which fills one of the finest cases in the whole building. This case is really a model in its way in point of convenience and good taste, and does the greatest possible credit to the Society and its architect. Arrangements have been made by which any person can examine, feel, taste, and smell any of the drugs contained in the collection. Hanbury and Co. exhibit some very fine pharmaceutical preparations, which Ransom, Watts, Bastick, Dickinson, Holland, Usher, and a host of others, try their very utmost to beat. The series of cinchona products by Howard and Sons could hardly have been produced by any other house. A magnificent mass of crystals of codeine, measuring eighteen or twenty inches in diameter, and worth upwards of 200*l.*, is exhibited by Macfarlan and Co., who also contribute some splendid specimens of anarcotine, morphia and its salts, sulphate of berberine, narceine, and other opium products. The trophy—which consists of a fine mass of alum crystals, surmounted and surrounded by crystals of prussiate, bichromate, sulphate

of copper, and other chemicals, piled in artistic forms—was an impromptu affair resulting from a private conversation between Mr. S. Howard and Mr. C. W. Quin. assisted by a sheet of blotting-paper and some very bad pens. The idea gradually grew until it has assumed its present shape. When completed, it promises to be a very magnificent mass of colour and form, and will attract a great deal of admiration even from the ignorant sight-seers, who believe all chemicals to be acids, and all acids to be spontaneously explosive.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

(*Continued from page 226.*)

Researches of Pasteur respecting the Theory of Spontaneous Generation, translated and condensed by M. C. White, *M.D.*

Fermentation of Urine.—A flask with an attenuated neck was one-third filled with fresh urine and boiled for three or four minutes and then allowed to cool, with no access of air except what was drawn through a platinum tube heated to redness. When cool, the flask was hermetically sealed, and the enclosed urine was thus exposed only to atmospheric air, deprived by heat of all viable germs. In this condition the urine remained for months without change. Into a flask thus prepared, asbestos charged with atmospheric dust was introduced by the method above described. The flask was kept at 86° F., and in about six hours mucedines and infusoria appeared, among which were *bacteria, vibriones,* and *monads,* the same as appeared in similar urine exposed to the open air. During the following days lithates and crystals of triple phosphate were deposited, the urine became ammoniacal, and its urea disappeared under the influence of the true ferment of the urine, which Pasteur believes to be organised, and whose germ could only have been introduced in the atmospheric dust in connection with the germs of infusoria and mucedines. When a flask prepared in the same manner had only calcined asbestos introduced, without atmospheric dust, neither mucedines nor infusoria appeared, neither did any fermentation take place, however long the flask was permitted to remain unopened.

Coagulation of Milk.—Fresh milk was boiled in a flask for two or three minutes only, and after being allowed to cool with access of calcined air, as in the preceding experiments, it was hermetically sealed. In eight or ten days the milk was coagulated, but when opened it was found remarkably different from milk coagulated in the open air, for it remained alkaline as fresh milk; but the milk was filled with infusoria, most frequently vibrios about $\frac{1}{500}$th of an inch in length, yet no vegetable productions were detected.

The common theory that milk coagulates in consequence of the formation of lactic acid is an error. It is also shown that vibrios may appear in milk which has undergone ebullition for several minutes at 212° F., although urine or a solution of sugar and albumen does not produce vibrios under such conditions. In other experiments the milk was boiled for longer periods under a pressure of 1½ atmospheres at a temperature of 230° or 235° F., and the flasks were sealed as before. Flasks thus prepared furnished no infusoria; the milk did not coagulate, however long it remained enclosed in the flasks; it remained alkaline even with the presence of oxygen in the form of calcined air, as stated above; and it preserved apparently all the properties of fresh milk.

Into flasks of milk thus prepared, Pasteur introduced atmospheric dust by the method detailed above, when the milk coagulated, and both animal and vegetable productions appeared as in the milk exposed to the open air. The generally admitted theory of ferments which had of late years received fresh support from the writings of chemists, now appears more and more at variance with the results of experiments. The ferment is not a dead substance without determinate specific properties. It is a being whose germ is derived from the air. It is not an albuminous substance altered by oxygen. The presence of albuminous matters is an indispensable condition of all fermentation, because the "ferment" depends upon them for its life. They are indispensable in the light of an aliment to the ferment. The contact of the atmospheric air is, primarily, equally an indispensable condition of fermentation; but it is indispensable only as being a vehicle for the "germs" of the "ferments."

There are many distinct organised ferments which excite chemical transformations, varying according to the nature and organisation of the ferment.

To confute various objections made by advocates of spontaneous generation, Pasteur undertook to determine the relative abundance of organic germs in different localities. A series of flasks were all one-third filled with the same putrescible fluid,—a solution of sugar and albumen was employed in most of the experiments. The fluid was then boiled for two or three minutes in the flasks, and the neck of each flask was drawn out to a fine point, and hermetically sealed while the fluid was hot. These flasks were then taken to different localities, and the points of the necks were broken, and the air of the several localities allowed to rush in and fill the flasks. This violent ingress of air carried in, of course, all the dust held in suspension, and all other principles known or unknown associated with it. In this condition each flask was again hermetically sealed, and the whole placed where they were kept at a uniform temperature of 80° to 85° F.,—a temperature known to be the most favourable for the development of animalcules and mucors. The results of these experiments were not what the principles generally admitted would lead us to expect, but they were perfectly consistent with the theory of the diffusion of germs.

Generally in three or four days the liquid in the flasks was found altered, but in flasks placed in identical conditions were found very different organisms,—much more varied so far as mucedines and torulas were concerned than if the liquids had been freely exposed to ordinary air. On the other hand, it frequently happened in a series of experiments that several of the flasks remained absolutely unaffected for an indefinite time, as if it had received only calcined air.

This simple and unobjectionable method of experimenting appears to demonstrate that the cause of so-called spontaneous generation does not exist in the ambient air throughout its whole extent, but that it is possible to take up in a single place and at a given instant a considerable volume of ordinary air which, without having undergone any physical or chemical change, is altogether unsuitable to give origin to infusoria or mucedines in a liquid which is invariably thus altered when it is exposed to the open air. The partial success of these experiments shows that by these movements of the atmosphere there is always brought to the surface of a putrescible liquid in an open vessel a quantity of air

sufficient to furnish germs suitable to be developed in two or three days.

It appears that the organic productions in the flasks are more various than if the contact with the air had been free, *i.e.*, the organisms in the several flasks are different. This result might have been expected, for by limiting the rush of air and repeating it with different flasks, a small number of germs would be collected in a limited portion of air, and the growth of these germs would not be obstructed by other germs, more numerous or more vigorous or rapid in their growth, capable of monopolising the soil to the exclusion of those less vigorous or less rapid in growth.

It was found that the number of negative results varied greatly with the atmospheric conditions, and that nothing was easier than to increase or diminish the relative proportion of flasks which gave birth to the organisms mentioned, or the number in which they were totally absent.

In the cellars of the Observatory at Paris, so situated as to have very little change of temperature, and where the air was remarkably quiet, the proportionate number of flasks that were opened in that locality without producing any organisms was much greater than for the same number of flasks opened in the court-yard of the Observatory, where the air was constantly agitated.

The explanation of this difference is obvious. Although the air of the cellars of the Observatory, nearly saturated with moisture, was more fitted for the production of the various kinds of mould and infusoria than the open air of the court-yard, yet the stillness of the air in the cellars allowed all ova and spores to be deposited by the force of gravity, and few or none remained floating in the air which rushed into the flasks opened in that locality. In proportion as more precautions were taken to avoid agitation of the air there was less appearance of organisation; and Pasteur concludes that if the flasks could be opened and closed in the cellars without the disturbance of the air caused by the entrance of the operator, there would be the same absence of vitality in the flasks filled with air from that locality as if they were filled with air exposed to a red heat.

The following results were obtained by Pasteur with flasks opened in widely different localities:—

Sixty-three flasks were each one-third filled with a clear liquid obtained by filtering water mingled with the scum of beer, all solid matter being removed by the process of filtering. This liquid is known to be very susceptible of change, for exposure to ordinary air for two or three days is sufficient to give birth to small infusoria or a variety of mucedines. The fluid was boiled in all the flasks, and they were hermetically sealed as in the previous experiments. Twenty of the flasks thus prepared were opened and closed in the country far from any habitation, at the foot of those heights which form the first plateau of the Jura mountains.

Twenty other flasks were filled with air upon one of the mountains of the Jura, 850 mètres (2789 feet) above the level of the sea. Another series of twenty flasks were carried to Montanvert, near the Mer de Glace, to an elevation of 2000 metres (6562 feet), where they were filled with air and hermetically sealed like the others.

Of the twenty flasks opened in the level country, six developed organic productions. Among the twenty flasks opened upon the plateau of the Jura, only five developed organisms; but of the twenty flasks filled with air at Montanvert, when a strong wind was blowing from the gorges of the Glacier des Bois, one only produced organisms.

These experiments show that the air from elevated localities is remarkably free from those germs which give origin to organic products.

In collecting air for these experiments the following precautions were adopted to avoid as far as possible the intervention of dust carried by the operator or deposited on the outside of the flask or other implements required in performing the experiments. The elongated neck of the flask was first heated in the flame of a lamp, and a scratch was made upon the glass with a file. The flask was then raised above the head with the end of the neck turned towards the wind, and the point was broken off with long iron forceps, the branches of which had previously passed through flame to destroy any dust adhering to their surface, so that it might not remain to be driven into the flask by the sudden rush of air when the point of the flask was broken. Great pains were taken lest the agitation of the liquid in the flasks during transit might exert some influence unfavourable to the development of infusoria or mucedines.

The following results are therefore without objection, and they show the entire difference between the air of the plain or of elevations and that of inhabited places. Pasteur's first experiment at the Glacier des Bois was interrupted by a circumstance which has not been foreseen. He had taken to close the points of the flasks, after they were filled with air, an eolipile lamp fed with alcohol. The whiteness and glare of the ice, in the light of the sun, was so great that it was impossible to see the jet of alcohol flame, and as it was agitated by the wind it could not be directed upon the glass with sufficient steadiness to melt the point and hermetically seal the flask. As no means were at hand to render the flame visible, the flask could not be sealed, and there remained chances of error by the admixture of other powders. The three flasks which had been opened were therefore taken to the small village of Montanvert, and sealed at his lodgings the next morning, after they had been exposed all night to the dust of the chambers where he slept. Of these three flasks only two produced either infusoria or mould. Since the number of flasks altered in this experiment is greater than that in those which followed (the twenty flasks previously noticed), Pasteur concludes that the agitation of the liquid during the journey had no influence upon the development of germs.

It therefore appears to be satisfactorily demonstrated:

1. That the air of inhabited places contains a greater relative number of fruitful germs than the air of uninhabited regions.

2. That the ordinary air contains only here and there, without any continuity, the condition of the first existence of generations sometimes considered spontaneous. Here there are germs and there there are none.

3. There are few or many, according to the localities. Rain diminishes the number; but after a succession of fine days they are more numerous. Where the atmosphere has been for a long time quiet germs are wanting, and putrefaction does not take place as in ordinary circumstances.

Gay Lussac, Schwann, and Pouchet have performed various experiments upon liquids in contact with common air, with heated air, with artificial air, and with oxygen gas, using a mercurial bath to isolate the substances experimented upon. Some of their results have appeared to favour the theory of spontaneous generation. Pasteur has ascertained that mercury taken from the bath in any laboratory is itself loaded with organic germs. He took a globule of mercury, surrounded by an atmosphere of

calcined air, and passed it into a flask of putrescible fluid by the process detailed in the former part of this paper. In every experiment of this kind after two days an abundant growth of organic products appeared.

The same experiments were repeated with the same liquids, with no change of manipulation, with the same kind of mercury, except that the mercury was first heated to destroy the germs it contained, and no growths whatever appeared in the flasks.

From all these experiments Pasteur concludes that powders suspended in the air are the exclusive origin, the first and necessary condition of life in infusions in putrescible bodies and in liquids capable of undergoing fermentation. It is easy to collect and observe with the microscope atmospheric dust, among which may always be found a great number of organised corpuscles which the experienced naturalist will distinguish as the germs of inferior organisms.

[Some infusoria are not more than $\frac{1}{24000}$th of an inch in diameter, and if we suppose that the ova of infusoria and the spores of minute fungi are no more than one-tenth part the linear dimensions of the parent organism, there must be an incalculable amount of germs no larger than $\frac{1}{240000}$th or $\frac{1}{100000}$th of an inch in diameter. Since, according to Sullivant and Wormley, vision with the most powerful microscope is limited to objects of about $\frac{1}{80000}$th of an inch, we need not be surprised if infusoria and other organisms appear in putrescible liquids in far greater numbers than the germs in atmospheric dust visible by the aid of the microscope would lead us to expect.—TR.]

Pasteur proposes to continue these investigations, and expresses the hope that the way may thus be opened for a successful investigation of the origin of different diseases.—*American Journal of Science*, Vol. xxxii. No. 94.

On the Quality of the Milk Sold in the Poor and other Districts of Dublin, by WILLIAM J. WOUFOR *and* SYDNEY R. PONTIFEX, *Students in the Laboratory of the Museum of Irish Industry, Dublin.*

(ABSTRACT.)

A PAPER on the above subject was read at the meeting of the Royal Dublin Society, held on Monday evening, April 14. The investigation was undertaken for the purpose of ascertaining the quality of the milk sold in the poor districts of Dublin; but as the quality of the milk can be greatly altered by the kind and quality of the food upon which the cows are fed, samples of milk from the more wealthy districts were analysed in order to ascertain whether the poor were worse served than their more affluent neighbours.

Twenty samples of milk from as many different dairies were examined; thirteen of the samples were purchased in very poor districts, and seven were obtained from richer districts of the city. The examination proved that the only adulteration was water.

The numerous published analyses of milk of known purity made by various chemists prove that when cows are fed with proper food the quantity of water in the milk is constant, although the proportions of the various substances forming the fixed constituents is variable; the normal quantity of water in pure milk is 87·35 per cent.; this quantity is never exceeded beyond a few tenths per cent. more or less. The average quantity of water in the thirteen samples from the poor districts of Dublin, examined by the authors of this paper, was 90·28 per cent., and the average quantity in the better districts was 89·4 per cent. Although, taken collectively, the milk from the richer districts is slightly purer than that from the poor districts, yet, when taken singly, the analyses show that the best milk—in fact, perfectly pure milk—was obtained from one of the very poorest districts (Sherriff-street) in the city, and that the rich district of Rathmines is supplied with as poor a milk as the poor street, Brabazon-street. It will be seen from the analyses that three out of the twenty milks examined were pure, and that the remaining seventeen were more or less adulterated.

According to the published Report of the *Lancet* Sanitary Commission, out of the twenty-three London milks examined by them,—

1st. Twelve were genuine.

2nd. Eleven were adulterated.

3rd. That the adulteration consisted in all cases of *water*, the per-centages of which varied from ten to fifty per cent., or one-half the article.

It will be seen from the mean of the analyses of the seventeen adulterated samples of Dublin milk, that the amount of water added is 3·5 per cent.; in no case did it exceed 5·6 per cent.; so that the Dublin people are supplied with a much purer article than the inhabitants of London; and it may be remarked that the districts selected by the *Lancet* Commission appear to be all in the better parts of London.

The authors described the methods they adopted for the estimation of the different substances, and they pointed out that specific gravity of the milk, whether determined by the instruments called lactometers, or by the still more accurate plan of the specific gravity bottle, is no certain indication of the purity of the milk.

Two analyses of the different milks were made. The following are the means of the two analyses, with the names of the streets in which the milk was procured. The authors give in their paper also the names of the vendors of the milks.

The investigation was carried out in the laboratory of the Museum of Irish Industry, under the direction of Professor Galloway.

Rathmines Road.

Specific gravity			1028·62
Total amount of organic matter			9·275
Butter	2·695		
Caseine	1·591		
Albumen	1·222		
Sugar	3·790		
		9·298	
Ash		·720	
Water		90·005	
			100·023

Cullenswood Avenue.

Specific gravity			1025·35
Total amount of organic matter			9·004
Butter	2·217		
Caseine and albumen	2·632		
Sugar	4·17		
		9·019	
Ash		·655	
Water		90·340	
			100·014

Duke Lane.

Specific gravity			1032·37
Total amount of organic matter			11·503
Butter	2·733		
Caseine and albumen	2·756		
Sugar	5·500		
		10·989	
Ash		·835	
Water		87·662	
			99·486

North Strand.

Specific gravity			1029·71
Total amount of organic matter			9·727
Butter	2·790		
Caseine	2·483		
Sugar	4·356		
		9·629	
Ash	·670		
Water	89·600		
			99·899

Anne Street.

Specific gravity			1030·71
Total amount of organic matter			11·225
Butter	3·11		
Caseine	2·91		
Sugar	5·30		
		11·22	
Ash	·665		
Water	88·11		
			99·995

Pill Lane.

Specific gravity			1019·93
Total amount of organic matter			10·13
Butter	4·9		
Caseine	1·967		
Sugar	3·270		
		10·13	
Ash	·62		
Water	89·24		
			100·00

Townsend Street.

Specific gravity			1018·58
Total amount of organic matter			6·453
Butter	2·19		
Caseine	1·725		
Sugar	2·400		
		6·315	
Ash	·625		
Water	92·95		
			99·890

Francis Street.

Specific gravity			1023·56
Total amount of organic matter			7·553
Butter	2·05		
Caseine	3·273		
Sugar	2·117		
		7·440	
Ash	·610		
Water	91·836		
			99·886

Brabazon Street.

Specific gravity			1023·47
Total amount of organic matter			9·001
Butter	3·135		
Caseine	2·828		
Sugar	2·991		
		8·954	
Ash	·596		
Water	90·403		
			99·593

N. King Street.

Specific gravity			1032·02
Total amount of organic matter			8·776
Butter	3·44		
Caseine	1·263		
Sugar	3·90		
		8·603	
Ash	·430		
Water	90·793		
			99·826

Sherriff Street.

Specific gravity			1029·59
Total amount of organic matter			11·692
Butter	4·78		
Caseine	1·408		
Sugar	5·419		
		10·607	
Ash	·717		
Water	87·590		
			99·914

Barrack Street.

Specific gravity			1023·05
Total amount of organic matter			7·285
Butter	1·817		
Caseine	2·473		
Sugar	2·765		
		7·055	
Ash	·517		
Water	92·147		
			99·719

Charles Street.

Specific gravity.			1023·70
Total amount of organic matter			7·700
Butter	2·365		
Caseine	1·985		
Sugar	3·271		
		7·621	
Ash	·600		
Water	91·700		
			99·921

Ranelagh.

Specific gravity			1026·95
Total amount of organic matter			9·01
Butter	2·385		
Caseine	3·601		
Sugar	2·887		
		8·873	
Ash	·640		
Water	90·350		
			99·863

Harrington Street.

Specific gravity			1027·47
Total amount of organic matter			9·686
Butter	2·890		
Caseine	3·447		
Sugar	2.336		
		8·673	
Ash	·576		
Water	89·739		
			98·988

Great Britain Street.

Specific gravity			1026·99
Total amount of organic matter			9·534
Butter	3·000		
Caseine	3·346		
Sugar	2·654		
		9·000	
Ash	·599		
Water	89·867		
			99·466

Hanover Quay.

Specific gravity			1020·83
Total amount of organic matter			6·847
Butter	1·6		
Caseine	1·887		
Sugar	3·158		
		6·645	
Ash	·482		
Water	92·67		
			99·797

L. Kevin Street.

Specific gravity			1023·34
Total amount of organic matter			8·730
Butter	2·486		
Caseine	1·991		
Sugar	4·243		
		8·720	
Ash		·525	
Water		90·745	
			99·990

Bride Street.

Specific gravity			1023·28
Total amount of organic matter			7·921
Butter	2·285		
Caseine	2·210		
Sugar	3·318		
		7·813	
Ash		·629	
Water		91·456	
			99·892

Church Street.

Specific gravity			1023·74
Total amount of organic matter			7·425
Butter	1·95		
Caseine	2·013		
Sugar	3·402		
		7·365	
Ash		·700	
Water		91·875	
			99·940

On Soil-Analysis, by Professor S. W. Johnson, *of Yale College.*

(Concluded from page 227.)

Twelve or thirteen years ago, Dr. Anderson, in his capacity of Chemist to the Highland and Agricultural Society of Scotland, had occasion to investigate two soils which had become "clover-sick," and he caused them, together with similar adjacent soils which still produced clover, to be most minutely analysed. Without reproducing his figures, which may be found in the *Transactions* of the Highland and Agricultural Society for 1849—51, p. 204, we will merely quote some of the remarks which accompany the analyses:—"The results of these analyses are certainly of an unexpected character, and appear to me to indicate that, in this instance, the failure of the clover cannot have been dependent upon the chemical constitution of the soil. In both cases the results of the analyses of each pair do not present a greater difference than would be obtained from the analyses of two portions of soil from different parts of any field."

In the present year, Stoeckhardt (*Chemischer Ackersmann*, No. ii., 1861, p. 85) has published an account of several "clover-sick" soils from Schlanstaedt, which reveal to analysis a greater content of every nutritive mineral ingredients, both soluble in water and in acids, than exists in another soil from Frankenstein which produces clover and wheat as well. What proves beyond a doubt that the inability of these soils to yield clover depends upon something besides their chemical constitution, is the fact that lucerne and esparsette still flourish upon them admirably; and further, clover itself, if sown with one of these last mentioned crops, succeeds very well.

A great truth in agriculture is this: Each kind of agricultural plant requires that its seeds be surrounded with certain conditions in order that they may germinate readily and healthfully, so that when the mother cotyledons are exhausted, the young plants shall attack the stores of food in the soil with that vigour which is needful in order to appropriate them without hindrance.

The fact that winter wheat is more delicate and fastidious in its infancy than most other crops, is perhaps the main reason why it does not succeed well on many good lands, and why it cannot be continuously produced from the same soil, year after year. It is a matter of experience that wheat requires a rather firm seed-bed; beans, oats, and mangold-wurzel approach wheat in their requirements, while barley, peas, and turnips are best suited in a light tilth. On the other hand, climate, weather, and tillage so influence the character of the soil, that even on light lands wheat may find all the conditions of its growth. The bed which is produced by inverting a clover sod, and allowing it to consolidate by time and rains, or by passing a heavy roller over it, is eminently adapted to wheat, even on a rather light soil.

The fact that in the cases given above from Stoeckhardt, clover succeeded when sown with lucerne or esparsette, would indicate that possibly the condition of the seed-bed was the cause of failure.

These and other facts, which might be adduced to almost any extent, indicate sufficiently that chemical analysis alone, even if we admit its full nicety and accuracy, can at the best furnish us with a knowledge of but a few of many conditions which must co-operate in profitable agricultural production, and, as a consequence, its part in guiding the farmer is but very subordinate. Taking into the account its evident uncertainty and clumsiness when applied to estimating the minute quantities which affect vegetable growth, the part it can play becomes still more subordinate—we hesitate not to say, insignificant.

As we write, a fragment from a scientific journal brings to our notice a discovery which, if real, strengthens our views in an unexpected manner. It is well known that iodine is so immensely diluted in sea-water—the soil of marine-plants—that none of our tests, though they are among the most delicate, serve to detect it directly, and it is doubtful if it has been detected even in the highly concentrated mother liquors which remain after separating the crystallisable salts, yet the fuci find and accumulate it, and we must grant that it is present there for them, in sufficient quantity.

Again, Prince Salm Horstmar several years since, in his admirable researches on the influence of the individual mineral ingredients of plants on the development of oats and barley, found that he could not by any possibility exclude chlorine from his experimental plants. His soils and pots, the salts and water he fed his plants with were so purified that he could not detect this element in them, and yet he invariably discovered it in the ashes of the plants. So, too, he found titanic acid in the produce grown on the most carefully purified soils. Now, it is mentioned in the Chemical News that he finds a few hundredths of lithium are indispensable to the ripening of barley. This element Bunsen has but recently shown to be everywhere distributed, yet it has been hitherto entirely unnoticed in all soil- and plant-analyses, because of its occurrence in almost infinitesimal quantity.

It must be well borne in mind that Agriculture herself—so-called Practice—is able of her own resources to judge somewhat of the value of soils, is able to know if a soil be fertile or poor, is able to pronounce upon its adaptation to crops, and can to a certain extent decide what is a good manure for this or that field.

We are free to assert that the knowledge which is now to be gathered from experience, is able, in ninety-nine

cases out of one hundred, to give a more truthful verdict as to the capacity of a soil, than any amount of analysis, chemical, mechanical, or otherwise, can do. We would give more for the opinion of an old intelligent farmer than for that of the most skilled chemist in most questions connected with farming. Doubtless the farmer would make some blunders from which chemistry might save him, but the chemist would be likely to do more violence to agriculture than the farmer would to chemistry.

By these statements, which may, but should not, surprise some of our scientific friends, we merely intend to express an opinion as to the present relative position towards agriculture of those who regard the art from a chemical, and those who see it from an experimental point of view.

If any one has fuller and more inspiring notions of the importance of science in its applications to agriculture than we have, we desire to sit at his feet and share the higher *afflatus*. But our inspiration, if it be of the sort that works enduring benefit, must be based on clear ideas of the directions in which advance is possible, and on a full perception of the difficulties that lie before us, and the means of overcoming them.

We have great faith that chemistry and that chemical analysis have done and are to do a work for agriculture, that shall lay that venerable art under everlasting obligations to the youthful science. But not by soil-analysis alone or mainly is this to be achieved. We do not assert that soil-analysis is worthless—we believe that the probabilities of its uselessness in direct application to practice are so great that we would rarely base any operations on it alone, and yet it may, in many cases, promote science and give us data for conclusions that are of practical use. But for these purposes it must form part of a system of observations and trials, must be a step in some research, must stand not as the index to a barren fact, but as the revelator of fruitful ideas.

We hold that soil-analysis long ago played out the part which Dr. Peter would have it perform. In the hands of Sprengel it was fertile with new truth, but it must henceforth be a tool for occasional use, and not an engine of discovery. With our advance in knowledge there must be an advance in methods of finding out the unknown. Soil-analysis was indeed a means of insight into the secrets of vegetable growth, but it carried with it the measure of its limit. What we call telescopes do not enable us to see the end!

To study the soil in the hope of benefiting agriculture, we must regard all its relations to the plant. We must examine it not merely from those points of view which theoretical chemistry suggests, but especially from those which a knowledge of practical agriculture furnishes. This is becoming more and more the habit of agricultural chemists, and the results are of the happiest kind.

Let us remember what the illustrious Nestor of Agricultural Science, Boussingault, has said as the summing up of his protracted experience and study :—

"At an epoch not far distant it was believed that a strict connection existed between the composition and the quality of arable soil. Numerous analyses shortly modified this opinion as too positive. The sagacious Schübler even sought to prove in a research that has become classic, that the fertility of a soil depends more upon its physical properties, its state of aggregation, power of absorption, &c., than upon its chemical constitution."

"The physical properties, in my opinion, do not enable us, more than the chemical composition, to pronounce upon the degree of fertility of the soil. To decide this point with some measure of certainty, it is indispensable to have recourse to direct observation ; it is necessary to cultivate a plant in the soil, and ascertain with what vigour it developes there : the analysis of the plant afterwards intervenes usefully, to indicate the kind and quantity of the elements that have been assimilated."—(*"De la Terre Végétale considerée dans ses Effets sur la Vegetation,"* page 283 of *"Agronomie, Chimie Agricole et Physiologie, Tome premier*, 1860.")

There has been much progress made in our knowledge of the soil during the last ten years. This advance has not consisted in revealing to us the presence of new elements (lithia perhaps excepted), nor in fixing with any more certainty the quantitative limits which separate barrenness from fertility, it has not shown what is the composition of a silurian or a sub-carboniferous, a drift or a tertiary soil, it has not defined the soil adapted to wheat or that productive of clover, it has not indicated the manures which this or that soil needs; but content with the fact that all soils which naturally support vegetation contain the elements of vegetation, it has sought to ascertain in what forms these elements are assimilable, how they may be made available, what changes or reactions in the soil affect its productiveness; how fertilisers act indirectly (their influence often having no relation to any supposable direct action), how the soil affects the life of the plant otherwise than by feeding it, &c.

We are approaching, in fact, by slow degrees to an understanding of the physiological significance of the soil, a grand result to which chemistry and physics co-operate.

We trust that in future people will not less but more appreciate the value of science in its practical and especially its agricultural bearings; that here, as in Germany, France, and England, the labours of those who seek to unite practice with science may be fostered and sustained. But to this end scientific men must be cautious that in endeavouring to help, however honestly and laboriously they may work, they do not hinder.—*American Journal of Science.*

On Sulphur Determinations in Coal, Coke, &c., by W. Crossley, *Analytical Chemist, Middlesbro'-on-Tees.*

In this short article I would draw attention to the fact, that many analytical chemists, and some of them eminent men too, determine the sulphur in coke, &c., by the old process of boiling in nitric acid. This has frequently thrown a doubt on the correctness of my analysis.

A short time ago, I determined the sulphur in a sample of coke, and obtained 1·09 per cent. I found afterwards a sample of the same coke had been sent to a very eminent chemist, and he only found about ·50 per cent. Having done many previous analyses of this same coke. I made inquiry, and found he had adopted this process,

The following experiments were done to test the accuracy of the two processes,—viz., first, fusion with a mixture of nitrate of potash, chloride of sodium, and carbonate of potash; and secondly, boiling in nitric acid. To give the nitric acid process every chance, I used a coke containing very little sulphur :—

1. I fused one gramme of this coke with the following mixture in a platinum crucible until all the carbon had burnt away :—

16	grammes of	chloride of sodium
8	,,	nitrate of potash.
4	,,	carbonate ,,

The fused mass was dissolved out of the crucible with water acidified with HCl, and evaporated to dryness; the mixture was then moistened with HCl, and, after adding some water, filtered, to separate the insoluble matter; the sulphuric acid was then precipitated and weighed in the usual manner.

2. I boiled another gramme of the same coke with nitric acid; evaporated to dryness; again added nitric acid, and again evaporated to dryness; the dried mass was then moistened with HCl, filtered, and the sulphuric acid determined.

The following shows the results of these two experiments:—

1. Sulphur	=	·603 per cent.
2. ,,	=	·477 ,,
Difference	=	·126 ,,

Further, by fusion with nitrate of potash, I determined sulphur in some sulphide of iron, and afterwards made a mixture of pure carbon and this sulphide in such proportions that it contained exactly 1·26 per cent. of sulphur.

I now determined the sulphur, first, by the nitric acid process; and, secondly, by fusion. I obtained the following results:—

1. Sulphur	=	·93 per cent.
2. ,,	=	1·23 ,,
Difference	=	·30 ,,

I cannot exactly explain why there should be this difference. I know it has been affirmed that some of the sulphur escapes oxidation; but where it exists in such small quantity as above, I think it can scarcely help being acted upon. Is there not rather a portion of the sulphuric acid driven off by evaporation to dryness? The following experiments seem to point to this conclusion:—

1. I boiled one gramme of the above-mentioned coke in nitric acid, to which one gramme of nitrate of potash had been added, for the purpose of uniting with the sulphuric acid formed, and thus prevent its expellation on evaporation to dryness.

2. A gramme of the mixed carbon and sulphide of iron was treated in a similar manner.

From these experiments I obtained the following results:—

Sulphur in coke	=	·604 per cent.
,, mixture	=	1·200 ,,

These experiments seem to support the above opinion; but it will need further experiment to determine if this be a safe process for determining sulphur.

The following Table shows the results of all these experiments on both coke and mixture:—

Coke.

Sulphur by NO_5 process	=	·417 per cent.
,, fusion	=	·603 ,,
,, nitric acid, with addition of $KO.NO_5$	=	·604 ,,

Mixture.

Sulphur by NO_5 process	=	·93 per cent.
,, fusion	=	1·23 ,,
,, NO_5, with addition of $KO.NO_5$	=	1·20 ,,

I have drawn attention to the above subject because I think it essential that chemists should be careful that they do not throw discredit on analyses by adopting incorrect processes.

Remarks upon the Method of Obtaining Nitrogen by the Decomposition of Solution of Ammonia by Chlorine, by Alfred Anderson, *Professor of Chemistry in Queen's College, Birmingham.*

It is stated in some treatises on chemistry* that nitrogen, in a pure state, may be obtained by the action of chlorine upon liquid ammonia: thus, by the decomposition,

$$NH_3 + 3\,Cl = 3\,HCl + N.$$

I was led to believe, however, from what I observed in making this experiment, and further, from the well-known reaction of chlorine and water,

$$Cl + HO = ClH + O,$$

that pure nitrogen would not be generated in this way, and that an abstraction of the hydrogen from a portion of the water of the ammonia solution by the chlorine, would result simultaneously with the decomposition of the ammonia itself.

When chlorine is passed into the strongest solution of ammonia, a brisk effervescence is produced, with occasionally a sharp cracking noise, attended also with a considerable evolution of heat. At the same time, a tolerably copious extrication of nitrogen takes place, always, however, containing in admixture (both at the commencement and termination of the operation) a considerable proportion of oxygen. In addition to these two gases, the only other product is chloride of ammonium, for I do not find that any chlorate of ammonia is formed throughout the process. Neither is there any of the highly explosive chlorine-nitrogen compound produced, so long as a portion of ammonia in some excess be present in the solution. If it be suffered to cool, however, when saturated with chlorine, deposition of this violent body in oily drops may speedily occur.

The mixture of the two gases, collected in a perfectly dry state, over mercury, gave on analysis the following results:—

	Bar.	Ther.	Corrected vol. at 60° and 30 in. bar.
Vol. of dry gas. 245 Mm.	29·75 in.	56°	244·8 Mm.
Vol. of gas after absorption of the oxygen. 215 Mm.	29·22 in.	56°	211·0 Mm.
			33·8 Mm.

244·8 millimètres of the nitrogen (as heretofore considered) contain, therefore, 33·8 of oxygen = 13·8 per cent.

The following equation will explain the changes occurring in this decomposition:—

$$5\,NH_3HO + 4\,Cl = 4\,NH_4Cl + N + O.$$

I may remark, that in repeating these experiments with solutions of ammonia of different degrees of saturation, I have ascertained that the proportions of oxygen and nitrogen in the evolved mixture of those gases are subject to considerable variation. But I have not found the oxygen to exceed, in any instance, the amount stated in the analysis now given; neither have I observed that any definite series of decompositions control the relative proportions of oxygen and nitrogen so obtained.

Determination of the Specific Gravity of Mineral Substances, by Dr. T. L. Phipson, *F.C.S., Member of the Chemical Society of Paris, &c.*

I make use of a very simple method for taking the specific gravity of minerals. It consists in measuring the volume of water displaced by a given weight of the

* "Miller's Elements," vol. ii. p. 539.

substance experimented upon. I take a glass cylinder, graduated in cubic centimètres and fractions of cubic centimètres, and after pouring in some water the height of the latter in the cylinder is noted. A given weight of the mineral is then introduced, and when the air-bubbles have disappeared, the height of the liquid is noted again. Now, 1 cubic centimètre of water weighs 1 gramme; therefore, if, after the introduction of 5 grammes of mineral, I find the water has risen 2·5 cubic centimètres, $\frac{5}{2{\cdot}5}$ gives the specific gravity of the mineral. This method necessitates only one weighing.

PHARMACY, TOXICOLOGY, &c.

On the Alteration of Tincture of Iodine, and the Means of Preventing it, by M. Dropet.

It is known that tincture of iodine does not long preserve its colour, a portion of the iodine changing to hydriodic acid. M. Dropet endeavours to show that the hydrogen necessary for the reaction comes from the water, and not, as is supposed by other chemists, from the alcohol. Among other experiments, he has shown that a tincture prepared with almost absolute alcohol, 34 centigrammes out of 3 grammes of iodide, were in eighteen months transformed into hydro-iodic acid. Another tincture, prepared with the same proportion of alcohol at 95°, lost 41 centigrammes; and a third, with alcohol at 86°, 67 centigrammes. These tinctures were preserved together in a dimly-lighted cupboard. M. Dropet concludes that in making tincture of iodine it would be better, for two reasons, to replace the alcohol at 86° by that at 95°. In the first place, the tincture keeps better; in the second, it is made more quickly, since iodine is much more soluble in concentrated than in weak alcohol.—*Repertoire de Pharmacie*, xviii. 214.

Chloride of Lime as an Insecticide.

In scattering chloride of lime on a plank in a stable, all kinds of flies, but more especially biting flies, were quickly got rid of. Sprinkling beds of vegetables with even a weak solution of this salt effectually preserves them from the attacks of catterpillars, butterflies, mordella, slugs, &c. It has the same effect when sprinkled on the foliage of fruit trees. A paste of one part of powdered chloride of lime and one-half part of some fatty matter, placed in a narrow band round the trunk of the tree, prevents insects from creeping up it. It has even been noticed that rats and mice quit places in which a certain quantity of chloride of lime has been spread. This salt, dried and finely powdered, can, no doubt, be employed for the same purposes as flour of sulphur, and be spread by the same means.—*Dingler's Polytechnisches Journal*, clxi. 240.

On the Preparation of Cinnabar, by Magnus Firmenich, *of Cologne.*

Cinnabar is most generally prepared in the dry way, and on a large scale, by fusing one part of sulphur and seven parts mercury, and subjecting the fused mass to sublimation; or, as in Idria, by mixing the two elements in rotating barrels, and subliming afterwards from iron vessels. The process with sulphide of potassium is less known, but deserves the preference on account of the intensity of the colour, and for the durability in fire of the product. A pure pentasulphide of potassium is necessary; those prepared by boiling an excess of sulphur in potassa, or by fusing potash with sulphur, are unfit for the purpose on account of the hyposulphite or the sulphate of potassa which is formed. Pure sulphide of potassium can only be obtained by reducing sulphate of potassa with charcoal; by saturating its solution afterwards with sulphur the liquid is rendered fit for the process. Hessian crucibles are filled to three-fourths of their capacity with an intimate mixture of 20 parts finely powdered sulphate of potassa, and 6 parts powdered charcoal, and heated, well covered in a furnace until effervescence ceases. This monosulphide is dissolved in 3½ parts of rain water, heated to boiling in an iron kettle, filtered and cooled to separate any undecomposed sulphate of potassa; the purified liquor is again boiled and saturated with powdered sulphur, of which four equivalents are required. It must be well protected from the atmosphere.

To prepare now the cinnabar, bottles are filled with 10 lbs. mercury, 2 lbs. sulphur, and 4½ lbs. of the above liquor; they are moderately heated, and placed in a swing in boxes, which are lined with straw, usually contain two such bottles, and are rocked against a straw cushion to increase the motion. The bottles commence to get warm after 1½ to 2 hours, and the mixture assumes a greenish brown colour; the mercury combines with the sulphur of the dissolved sulphide, which is replenished by the sulphur of the mixture; to keep the latter in a loose condition, the bottles ought to be turned occasionally. The combination is completed in about 3½ hours, and the colour of the mixture is now dark brown. After having been cooled slowly, the bottles are placed in a room, the temperature of which is between 35° and 43° R. (111° and 122° F.), for two or three days, during which time the mixture is well agitated three or four times daily. The temperature has an important influence on the shade of the colour, which is lighter the cooler the mixture has been on being put in the swing. Light carmine cinnabar, with a tinge of yellow, is obtained by exposing the bottles in winter to the cold atmosphere for one hour, or by setting them in summer in cold water for the same space of time.

The cinnabar is now to be freed from the excess of the sulphur. About half a quart of water is added to each bottle, and the contents thrown on a filter; the cinnabar is then treated in stone pots with caustic soda; and, after the sulphur is dissolved, the liquor is decanted and the residue washed repeatedly with fresh water, which usually requires two or three days. The complete removal of the sulphur and of the alkaline liquor is most important, the durability in fire depending on the former, and the permanence of the colour upon the latter. In order to dry the cinnabar, it is first transferred to the grate in a drying closet, where a very moderate heat is used for desiccation, until it breaks into pieces, and does not appear moist to the touch. Placed upon iron pans, it is introduced into a drying oven, where it is constantly turned, and gradually heated to 50° R. (144° ·5 F.) This last manipulation is finished in about five hours. By the higher heat the cinnabar assumes temporarily a darker shade, and its durability in fire is much increased thereby.

This process I believe to be superior to all others, for the qualities of the product are equal to or surpass those of cinnabar made in other ways, and at the same time the cost is much lower.—*Polytechn. Centralbl.*

PHYSICAL SCIENCE.

On the Molecular Dissymmetry of Natural Organic Products, by L. PASTEUR, *Member of the Chemical Society of Paris.*

(Continued from p. 233.)

VII. This general conclusion from the preceding studies throws new light upon our ideas of molecular mechanism. We see that if natural products, organised under the influence of vegetable light, are ordinarily dissymmetric, contrarily to what mineral and artificial products offer us, this disposition of elementary particles is not a condition of existence of the molecule, that the twisted organic group can untwist itself and then take the general character of artificial or mineral substances. On the other hand, it seems to me logical to regard the latter as susceptible of presenting a dissymmetric arrangement of their atoms in the manner of natural products. The conditions of their production are to be discovered.

As final analysis and summing up of what precedes, the groups of elementary atoms which constitute compound matter may cover two distinct states, corresponding to two general types, to which every material object may be referred. The form of the group is with a superposable image or with a non-superposable image; but this latter type is double, because its inverse can exist on the same ground as itself. We must add the case of the association of these two inverse types, which recalls the union by pairs of the identical and non-superposable members of the superior animals. So that there is, in reality, four remarkable dispositions for the groups of atoms which constitute matter. All our efforts should tend towards producing them for each particular species.

There is in almost all these considerations such exactitude that it seems impossible to call them in question. How refuse to admit that a right body has its possible left, knowing as we know the signification of the right or left character? This would be to doubt that an irregular tetrahedron has its inverse; that a dextrorse helix has its sinistrorse inverse; that a right hand has its possible left.

And hence, if the mysterious influence to which the dissymmetry of natural products is due should come to change in sense or direction, the constituting elements of all living beings would take an inverse dissymmetry. Perhaps a new world would be presented to us. Who could foresee the organisation of living beings, if the cellulose which is right should become left, if the left albumen of the blood should become right? There are here mysteries which prepare immense labours for the future, and from this hour invite the most serious meditations of science.

VIII. Only because chemistry has been, up to the present time, powerless in the preparation of dissymmetric bodies, might we fear that we may be always ignorant of the mode of production of the inverse bodies of natural organic substances. Happily this fear is exaggerated. I have ascertained, indeed, that, by ordinary chemical processes, such as the action of heat, a right body can be changed to its left, and inversely. Thus, in warming right tartaric acid under certain determinate conditions, which it would be too long to specify here, it is transformed into left tartaric acid, or, rather, into paratartaric acid; and inversely, under the same conditions exactly, left tartaric acid becomes right.

Here are ten or twelve grammes of entirely pure left tartaric acid, which were thus procured. Their preparation cost me much trouble. But M. Biot desired to study in a very particular manner the characters of dispersion of this left tartaric acid, so remarkable on account of its origin. He desired himself to be at the cost of the operation,—very expensive, because the transformation depends upon the employment of the tartrate of cinchonine or of quinine, and the base is lost because the tartrate must be heated to a temperature which destroys it.

I have prepared by this process sufficient paratartaric acid to take from it twelve grammes of left tartaric acid, which presents rigorously, in an inverse sense, the same optical characters as tartaric acid.

Every analogous transformation of a natural dissymmetric body into its inverse should be regarded as a great advance in organic chemistry.

IX. At the conclusion of our first lecture, I alluded to observations to which it is time we should give all the attention they merit. These observations are relative to the comparison of the physical and chemical properties of right and corresponding left isomerics. I have already insisted upon the perfect identity of all their properties, excepting, however, the inversion of their crystalline forms, and the opposition of direction of their optical deviations. Physical aspect, lustre of the crystals, solubility, specific gravity, single or double refraction, all is not only alike, similar, very near, but identical, in the most rigorous acceptation of the word.

This identity is the more remarkable, as we shall see it replaced by a general and considerable opposition of the properties of these same substances, when they are found in particular conditions which I am about to point out.

We have seen that we must distribute all chemical compounds, artificial or natural, mineral or organic, in two great classes,—compounds non-dissymmetric with non-superposable image.

This stated, the identity of properties of which I have just spoken in the two tartaric acids and their similar derivatives, constantly exists with the absolute characters I have named, whenever these substances are placed in presence of any compound of the class of bodies with superposable image, such as potassa, soda, ammonia, lime, barytes, aniline, alcohol, the ethers,—in a word, of all compounds, whatever they may be, not dissymmetric, not hemihedric in form, without action on polarised light.

Submit them, on the contrary, to products of the second class with non-superposable image, asparagine, quinine, strychnine, brucine, albumen, sugar, &c.,—bodies dissymmetric like them all change in an instant. Solubility is no longer the same. If there is combination, the crystalline form, specific gravity, the quantity of water of crystallisation, the more or less ready destruction by heat, all differ as much as the most distant isomeric bodies differ.

Here, then, we have the molecular dissymmetry of bodies introducing itself in chemistry as a powerful modifier of chemical affinities. In presence of the two tartaric acids, quinia does not behave like potassa, only because it is dissymmetric, and potassa is not. Molecular dissymmetry is hence offered as a property capable by itself, as far as dissymmetry, of modifying chemical affinities. I do not believe any discovery has yet entered so far into the mechanical part of the problem of combinations.

Let us endeavour to present the cause of these identities and of these dissemblances. Let us suppose a dextrorse helix and a sinistrorse helix separately penetrating two blocks of identical wood, and in right lines. All the mechanical conditions of the two systems will

be the same. It will be no longer so from the moment these same helices shall be associated with blocks, themselves shaped in helices of the same direction or of opposite directions."

(To be continued.)

PROCEEDINGS OF SOCIETIES.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, April 15, 1862.

E. W. Binney, *F.R.S., F.G.S., Vice-President, in the Chair.*

The Rev. Robert Harley, F.R.A.S., Corresponding Member of the Society, made a communication "*On the Theory of the Transcendental Solution of Algebraic Equations.*"

Dr. R. Angus Smith, F.R.S., read a Paper entitled "*On the Putrefaction of Blood,* No. 2." The following is an abstract:—When I first began to examine the products of the putrefaction of blood, it was merely with the object of ascertaining the nature of the gases, and of ascertaining whether any matter in them exists in a so-called organic condition, and, if so, in what quantity. I have ascertained the nature of the gases. So far as I see, however, I have added no new one; but I believe that for the first time I have given the proportionate amount of each. I have also decided on a simple and certain method of collecting some organic substances from the gases, namely, the use of caustic potash, which I find superior to acid salts. Other methods also I have found promising, but I have been compelled to give up the inquiry, at least for a long time, on account of the extremely nauseous emanations which penetrated every room in the laboratory, and were, no doubt, waiting for a favourable opportunity of changing into deadly poisons. These, by proper arrangements, might be avoided. I mentioned formerly that the temperature of 54° Fahr., or 12° C., was a very marked one in favouring putrefaction. I now find that on to 120° Fahr., (49° C.) at least, the process, if it ceases, may be set in motion by raising the temperature. After that point it is difficult to induce putrefaction. I give here a specimen of the

Progress of the Decomposition.

	Gases Absorbed.	Not Absorbed.
Nov. 9	88·65	11·35
,, 12	91·32	8·68
,, 13	91·56	8·44
,, 14	95·90	4·10
,, 15	96·04	3·96
,, 18	98·26	1·74
,, 19	98·50	1·50
,, 20	98·95	1·05

After the decomposition had proceeded to such an extent that it was difficult to obtain even a few bubbles more, the gases existed in the following proportion:—

Carbonic acid	97·09
Sulphuretted hydrogen	1·93
Hydrogen	0·18042
Carbonic oxide	0·13968
Marsh gas	0·07295
Nitrogen	2·51715
	100

A trace of a compound of cyanogen was found, and a small amount of phosphorus was obtained in the acid solution. Ammonia was found in considerable quantities. These substances exist along with the gases, and are all I have hitherto determined. Other experiments gave me much more hydrogen, and I am prepared for a considerable variation in the amount of the several gases. The nitrogen came to a minimum whenever the decomposition became slow. This might be interpreted in two ways—first, by the absence of air to continue the process; and second, by absence of the nitrogen from the decomposed albuminoids. I do not see from my experiments a sufficient proof of the elimination of nitrogen. The amount was in a state of constant decrease, suggesting a gradual removal of atmospheric air. I mentioned that by passing the gas through lead and other metallic salts, only a small amount of organic matter was collected; but by passing it through caustic potash the amount was considerable. A flocculent matter fell, but the chief amount remained in solution. The solution was boiled down, and, when heated, a perfectly fresh odour of soup was spread through the room; everything offensive had been removed, and the smell was for the first time very agreeable. Here we find that the substances sought for are decomposed by the very means which we take to retain them. But in this experiment we see a demonstration that substances of an organic nature pass over with the gases. When strong sulphuric acid was added to the potash solution there was an abundant black precipitate of carbon and carbonaceous matter. More than enough for an analysis had been made. There was a fatty odour from it when sulphuric acid was added, leading me to think of Chevreul's remarks on a similar occasion. As these compounds were not retained by acid salts, but by alkalies, I concluded that they were acids; but on allowing some of the solution to stand for a few hours, I was surprised to find that the organic matter had almost disappeared. We see clearly how differently the organic substances act from carbonic acid. I took home a small piece of cotton wool through which the gas had passed for some days. My intention was to examine it with the microscope. Less than a grain of this cotton was taken out of the tube in which it was enclosed, but so thoroughly did the room become offensive that some friends, not aware of my pursuits, were much annoyed. This is one of those many facts which lead to the conclusion that the amount of carbonic acid is entirely incapable of showing the true condition of an atmosphere unless we estimate the gas at once on its formation, as then it is mixed with organic matter. If, however, we allow even a short time to pass, a separation takes place, the carbonic acid diffuses, and the organic matter clings to substances to be gradually given off; the tendencies of the two are entirely different, and the separation begins at once. When the gases were previously passed through charcoal, it was difficult to obtain a trace of organic matter. I imagine that I see my way now to a very satisfactory and comparatively easy investigation, although for the time I must leave it to others.

NOTICES OF BOOKS.

Metallurgy. By John Percy, M.D., F.R.S. London: John Murray, Albemarle Street.

(FOURTH NOTICE.)

Methods of Estimating Copper.—In continuation of the author's remarks upon the disadvantages and inaccuracy of the Cornish method of dry assay, we propose now to give a brief description of the several wet processes recommended as being applicable for the estimation of copper in all kinds of ores and metallurgical products. The first step in any of these operations consists in procuring a solution of the copper by means of acids; the blue and green native carbonates are easily attacked by dilute nitric acid; but in the case of copper-pyrites and other sulphuretted ores it is expedient to roast a weighed portion of the sample in a small porcelain capsule, then to moisten with concentrated sulphuric acid, lastly adding nitric acid,

when the whole of the copper is readily dissolved, and the liquid requires merely to be boiled in order to expel the red nitrous fumes, and diluted with water to furnish a solution fit for testing.

The amount of copper may now be determined either by standard solution of cyanide of potassium, as proposed by Mr. Henry Parkes in 1851; or by solution of hyposulphite of soda, as recommended by Mr. E. O. Brown;* by a process of precipitation, as disulphide, suggested by the author; or by the "coloration test," which last seems only applicable to the examination of copper slags.

In carrying out the volumetric estimation according to the directions of Mr. Parkes, a solution of cyanide of potassium is slowly added to a blue ammoniacal solution of copper, when the latter gradually loses its colour, and finally becomes quite colourless; upon this chemical reaction the estimation of copper by cyanide of potassium depends. By ascertaining by direct experiment the amount of cyanide of potassium solution required to discharge the colour in an ammoniacal solution containing a given weight of copper, it is easy by a comparative experiment to determine the amount of copper in a given weight of ore.

For the preparation of the standard solution 2000 grains of fused cyanide of potassium are to be dissolved in two quarts of water, to produce a solution of which 1000 grains measure will be equal to about ten grains of metallic copper. This solution should be preserved in green glass stoppered bottles, and kept as much as possible away from the light; it is liable to a slow decomposition, which will necessitate the standard being checked at intervals of one or two weeks. In order to standardise the solution, a burette, holding 1000 grains measure, is filled to the zero mark, and a piece of pure electrotype copper, previously cleaned by means of dilute nitric acid, washed and dried, is accurately weighed. About eight grains may be conveniently taken; this is dissolved in a pint flask by dilute nitric acid, and, after the energy of the first action has subsided, the solution is warmed and ultimately boiled to expel all the nitrous acid fumes. It is diluted with cold water to the bulk of nearly half-a-pint, treated with ammonia in excess, and to the deep blue solution the cyanide is added from the burette until the colour is so nearly discharged that a faint lilac tint only remains. This will generally become quite bleached on standing at rest for a short time, so that the cyanide must not be added too hastily towards the end of the operation. It will be advisable to control the standard by a second experiment upon another weighed portion of copper, and to stop short of bleaching entirely the faint lilac tint of the solution. A piece of white paper folded and placed under and behind the flask during the decolorisation, will aid in recognising the proper tint of the solution.

In applying this process to the examination of copper ores, a known weight of the finely-powdered sample is introduced into a beaker provided with a glass cover, and moistened with strong sulphuric acid; strong nitric acid is then added, and the whole digested on a sand-bath until nitrous fumes are no longer given off. Should a small quantity of sulphur be separated in the treatment of pyrites ores, the small globules may be taken out, burnt, and the residual copper dissolved in a few drops of nitric acid and mixed with the remainder. Water is now to be added and left in contact for a short time to extract all the metallic salt from the insoluble residue, which need not be filtered off; and so, likewise, when ammonia is added in the next place, any peroxide of iron which may thus be precipitated is left in the solution, for it is apt to contain a small proportion of copper when first thrown down; but this is entirely removed by the cyanide of potassium later in the experiment.

When the ore contains much iron it is considered desirable to remove the hydrated peroxide by filtration, in order to be enabled to determine with greater precision the last effects of the cyanide of potassium; and in the event of requiring to know the amount of iron present in the ore, the precipitated peroxide on the filter is re-dissolved in dilute sulphuric acid, reduced to the state of protoxide by metallic zinc, and then tested in the usual way with a standard solution of permanganate of potash.

The metals which interfere with this mode of valuing copper ores, are silver, nickel, cobalt, and zinc. The first may readily be separated by adding a few drops of hydrochloric acid to the original solution; the other metals may be excluded by following one of the methods pointed out by the author for that purpose.

The process described by Mr. E. O. Brown is particularly applicable to the determination of copper in gun-metal, brass, and other alloys which contain no large amounts of iron and lead. It is founded on the reactions between salts of copper and the neutral iodides, and on the conversion of the liberated iodine into hydriodic and tetrathionic acids by a standard solution of hyposulphite of soda.

These reactions may be thus expressed:—

$$2(CuO,\overline{A}) + 2KI = Cu_2I + I + 2\,(KO,\overline{A})$$
$$I + 2(NaO,S_2O_2) = NaI + NaO,S_4O_5$$

The completion of the second reaction is manifested by the bleaching effect produced upon the blue iodide of starch by the addition of the hyposulphite. A convenient strength of solution for this purpose may be made by dissolving 1300 grains of the crystallised salt in two quarts of water. The iodide of potassium must be free from iodate; and a clear solution of starch employed.

From 8 to 10 grains of the copper or alloy are dissolved in dilute nitric acid, and the red nitrous fumes expelled by boiling. The nitrate of copper is converted into acetate by adding carbonate of soda until a portion of copper remains precipitated, and then re-dissolving in acetic acid. The solution is diluted with water, and about 60 grains of iodide of potassium in the form of crystals dropped into the flask, and allowed to dissolve. The standard solution of hyposulphite of soda is now poured in from a burette, until the greater part of the dark-coloured free iodine disappears. A little of the starch solution is now added to make its presence more apparent, and the addition of the hyposulphite continued until the bleaching is completed, when the pale yellow colour of the sub-iodide of copper will alone be visible. The amount of copper in the ore or alloy is calculated from the number of divisions indicated upon the burette.

Copper ores containing much iron (which interferes by reason of the dark red colour of the acetate) may be dissolved in nitric acid, and treated with sulphuretted hydrogen to precipitate the copper, the sulphide being collected on a filter, washed, and re-dissolved in nitric acid to produce a solution suitable for testing by this process. Or the hyposulphite may itself be employed to furnish a precipitate of disulphide of copper, according to a method next to be described.

Quarterly Journal of the Chemical Society, April, 1857.

NOTICES OF PATENTS.

1387. *Manufacture of Kamptulicon.* W. R. Jeune, Bow, Middlesex. Dated June 3, 1861.

The india-rubber to be employed in the manufacture is rolled into the form of sheet, which is then soaked in a liquid, such as coal naphtha, until softened and much swollen. In this condition it is covered on both sides with cork dust, and again passed between rollers to effect the proper incorporation of the materials. The compound of india-rubber and cork is then formed into kamptulicon by being again rolled between two fabrics, the one, coarse, being left on the finished material to give it strength and serve as a backing; the other fabric is finer in texture, and is

removed from the sheet immediately after leaving the rolls, having served the temporary purpose of producing a fine surface. In order to facilitate the removal of this upper fabric it is recommended to use it damp, or to prepare one face of it with a coating which shall have the effect of preventing adhesion. At this stage the sheet is placed finally in a drying chamber, where it is left until the softening liquid is completely evaporated.

1392. *An Improved Combination of Metals for the Production of a White Alloy resisting the action of Vegetable Acids.* M. A. F. Mennons, Paris. A communication. Dated June 4, 1861. (Not proceeded with.)

This alloy is prepared by fusing together the following metals in the proportion to make 1000 parts, viz.,—banca tin 875 parts, antimony 50 parts, nickel 55 parts, and bismuth 20 parts.

It will be seen that this alloy differs from common pewter by containing bismuth in the place of lead, and nickel in addition. The employment of the last named metal may possibly counteract in a measure the tendency to corrosion by acids, but will, on the other hand, raise considerably the price of the alloy.

1397. *Improvements in the Manufacture of Gas, and in the Apparatus connected therewith.* A. Prince, Trafalgar Square, London. A communication. Dated June 4, 1861.

These improvements are based upon the action of a current of steam passing through the gas retorts in producing a larger volume of illuminating gas from a given charge of coal. The aqueous vapour is decomposed by the highly carburetted products of the distillation of coal in such a manner as to form carbonic oxide, and at the same time furnishing an increased amount of hydrogen to the gas, and so preventing a great loss in illuminating effect by diminishing or avoiding the separation of carbon against the heated surfaces of the retort.

There is every probability of a larger yield of gas being obtained by this process, and of the illuminating power in the aggregate being considerably augmented. The poisonous character of carbonic oxide gas appears to be the great disadvantage to which this method of production is liable.

1406. *Improvements in the Manufacture of Insulators for Telegraphic Wires, and in Materials and Machinery for Coating Telegraphic Wires.* H. G. B. Roeber, Silvertown, Essex. Dated June 4, 1861. (Not proceeded with.)

The insulators employed in connection with telegraphic wires and apparatus have hitherto been made of only one non-conducting material; the first part of this invention relates to their manufacture from two or more insulating substances, such as india-rubber, gutta percha, or shellac, employed conjointly with glass, earthenware, or porcelain.

The principle of associating two or more non-conducting materials in the manufacture of telegraphic insulators cannot be said to have originated so recently as June last. Many of the earlier patterns were constructed in this manner; glass and shellac, earthenware and gutta percha, besides other combinations, being employed. It is a common practice in the present day to make the insulators of more than one material.

Royal Institution.—The following Lectures will be delivered in the ensuing week:—Monday, May 5, at Two o'clock, General Monthly Meeting. Tuesday, May 6, at Four o'clock, Mr. C. T. Newton, "On Ancient Art." Thursday, May 8, at Three o'clock, Dr. Lyon Playfair, "On Progress of Chemical Arts, 1851-62." Friday, May 9, at Eight o'clock, Mr. W. Fairbairn, "On Resisting Properties of Iron." Saturday, May 10, at Three o'clock, Professor Anderson, "On Agricultural Chemistry."

CORRESPONDENCE.

Downing v. *Chance.*

To the Editor of the Chemical News.

Sir,—Your version of the "circuit short-hand writer's" notes, which appeared in your last week's Number, necessitates from me a reply, which in fairness I trust you will insert in your next Number.

In my letter which appeared in your Number for April 19, I quoted two versions of what (in the Number for April 5) you made me say, the accuracy of both of which I denied.

In your transcript of the circuit short-hand writer's notes, which are supposed to be "*verbatim*," you give a third version.

Here they are:—

No. 1. "Does not remember the composition of cyanogen, for the compounds of hydrogen are so complicated."

No. 2. "He could not just then refresh his memory as to which two gases cyanogen was composed of at the moment. Some of the compounds of hydrogen are so difficult."

No. 3. "Some of the compounds are so excessively difficult that it would be perfectly impossible, without scientific books, to enter into their composition."

Leaving to others to comment on these three "reports," all said to be taken in the very words used by me, I feel, Sir, that I have a right to ask you, if it is fair—is it English—to hold me up to animadversion for words spoken, three versions of which you have already given?

But you will probably say the circuit short-writer's notes (about whose accuracy there can be no question) states, that when Serjeant Pigott asked the question, "Will you tell me what cyanogen is?" I replied, "It is a compound of nitrogen and hydrogen."

If I had said so, pray how was it that in his next question (see "report") he asks, "Is not it an element, tell me what you say it is?"

Is it not plain from his second question that I never said it is a compound of nitrogen and hydrogen?

If I had so replied, would it not have been arrant nonsense for Serjeant Pigott to ask, "Tell me what you say it is," when I had just told him?—I am, &c.

Alfred Bird.

Birmingham, April 29.

[Our readers will see that the "three versions" are strictly what they profess to be. The first two being condensed accounts, and the third a *verbatim* report, given to prove the perfect accuracy of our former abstract. It is satisfactory to know that Mr. Bird admits there can be no question about the accuracy of the circuit short-hand writer's notes.—Ed. C.N.]

Spectrum Analysis.

To the Editor of the Chemical News.

Sir,—On page 214 of your Number for April 19, Sir D. Brewster is stated to be the first who (in 1822) examined a coloured flame by the prism. A short investigation, however, shows that the first application of the prism to such purposes must be dated at least seventy years previous. The very paper (*Trans. of the Roy. Soc. of Edinburgh*, vol. ix. p. 435) in which Sir David published his examination of yellow sodium light, proposing to use it in his monochromatic lamp, contains a reference to a paper in the "Edinburgh Physical and Literary Essay," (vol. ii. p. 34,) in which we find these previous observations described. To the writer of this paper the remarkable phenomena exhibited by coloured lights when examined by the prism were well known. To show how fully he was acquainted with the

peculiarities of yellow sodium light, it is sufficient to quote the following passage, in which he speaks of the light of burning alcohol mixed with nitre (impure?) or sea-salt:—

"The proportion in which the bright yellow exceeds the other colours in this light is still more extraordinary than in the former; insomuch that the hole seen through the prism appears uniformly of this yellow, and as distinctly terminated as through a plain glass, except that there is adjoining to it on the upper side a very faint stream of green and blue. White bodies illuminated by it appear also through the prism perfectly well-defined; both which are very surprising phenomena to those who have been accustomed to the use of the prism in other heterogeneous lights, where it never fails to throw confusion on the extremities of all visible bodies."

This paper was read before the Medical, afterwards the Philosophical, Society of Edinburgh, on January 3 and February 7, 1752. Its author was Thomas Melvill, M.A., a young and talented experimentalist, who, according to a note prefixed to the printed paper (vol. ii. p. 12), died in December, 1753, at the early age of twenty-seven years. Melvill, it is added, noticing that few, if any, of Sir Isaac Newton's followers had gone one step beyond him in his researches on the spectrum, proposed to apply himself particularly to the further illustration of the theory of light and colours. The published essay, it is said, "is a specimen of what might have been expected from him, and sufficiently shows the uncommon genius of its author."

Another paper by Melvill, on the "*Cause of the Different Refrangibility of the Rays of Light*," is printed in the *Phil. Trans.*, vol. xlviii. (1753), p. 261, having been transmitted to the Royal Society by its author while staying at Geneva. It exhibits equal scientific genius.

It seems, then, only common justice to accord to Thomas Melvill the full honour of first entering with deliberation upon a branch of discovery which now occupies the foremost place in the scientific world. Had his life been prolonged, many beautiful and important facts concerning the spectrum, which we are only just learning, might have been given to the world more than a hundred years ago.

I am glad to find that Professor Roscoe, in the last of his three lectures, briefly noticed the claims of Thomas Melvill, which I am now urging.

In *Phil. Trans.*, vol. lxxv. (1785), p. 190, is a paper relating to the subject by a Rev. Mr. Morgan; it is of very inferior importance.—I am, &c.

W. S. JEVONS.

London, April 23.

Carbon in Iron.

To the Editor of the CHEMICAL NEWS.

SIR,—In a recent Number of your Journal you give an analysis of iron for carbon, taken as new from the *Zeitschrift des Vereins Deutscher Ingenieurs*. Permit me to say that this method is not new. It is due to Mr. Christopher Binks, who, in his paper read at the Society of Arts in 1857, states this very method as his own, and as the best way for determining both the proportion and the quality of the carbonaceous matter contained in steel. The following extract from Mr. Binks' paper shows this:—

"Under the head of 'Some Evidences of the Analysis as to the Real Composition of Steel,' he says (other evidences being adduced), 'The best malleable iron, on the one hand, and by way of comparison with this, the same kind of iron fully converted by the usual process, were taken on trial; the steel was dissolved in a very dilute and pure hydrochloric acid, and after many trials it was found best to place the bar of steel or iron in single voltaic arrangement with platinum, and to effect the solution in the cold with the usual precaution of expelling air from the water employed. In this way, slowly, the steel was dissolved, and the carbonaceous flocculent matter that was left collected, carefully dried and analysed. The iron was treated in the same manner, and the comparatively very small proportion of carbonaceous residue given by it also examined. And these were compared with the residue obtained also from cast-iron. If the acid be strong, and heat be used, and the voltaic arrangement be not used, the results are very different. Gaseous nitrogen, in very minute quantity, is given off along with the hydrogen, some muriate of ammonia is formed in the solution, and but little nitrogen left in the residue.'"

Herr Weil, who announces this as his method of analysis, is not the only one (MM. Fremy, Caron, and others, for example,) who have been anticipated in this paper of Mr. Binks.

Mr. Binks' object in resorting to this, then, new mode of analysis, was to examine into the character of the previously so-called carbonaceous matter found in steel and in some kinds of iron. The result forms one of his numerous proofs of the invariable and inevitable co-operation of nitrogen as well as of carbon in the formation and composition of steel,—results that have, as you are aware, been recently confirmed by the researches of some of the French chemists.—I am, &c. JOHN STEBBING.

Inverness Road, Bayswater.

Chemical Notices from Foreign Sources.

ORGANIC CHEMISTRY.

Benzyl Mercaptan and Bisulphide of Benzyl.—Vogt has prepared (*Annal. der Chem. und Pharm.*, Bd. cxix. s., 142) both of the above bodies in Kolbe's laboratory. Benzyl mercaptan, $C_{12}H_6S_2$, is prepared as follows:—Dilute sulphuric acid is poured on some granulated zinc in a good-sized flask, and when the evolution of hydrogen is brisk some sulpho-chloride of benzyl is added. The flask and its contents are then allowed to stand for twenty-four hours, and the whole is afterwards distilled. When the flask is heated the evolution of hydrogen recommences, and the new body passes over with the gas and steam, condensing in the receiver as a colourless oil. The saline residuum in the flask contains the second body,—the bisulphide of benzyl. Benzyl mercaptan shares with the known mercaptans the property of readily exchanging an atom of hydrogen in the form of sulphuretted hydrogen on coming in contact with a metal; and in possessing a peculiar affinity for mercury. One drop placed on dry oxide of mercury gives rise to a violent reaction. The author goes on to describe the compounds which are formed by the action of this body on various metals.

Products of the Oxidation of Sulphindigotic Acid.—An interesting paper on this subject, by G. and A. Schlieper, will be found in the *Annalen. der Chemie. und Pharmacie*, Bd. cxix. s. 1. The authors describe the formation of isato-sulphuric acid $C_{61}NH_4O_3$, $2SO_3 + 4HO$, and the salts mono- and bibasic formed with the alkalies, alkaline earths, and some metals. Isato-sulphuric acid is prepared by decomposing the baryta salt. The solution is orange red, and very acid. By drying over sulphuric acid a silky crystalline mass is obtained, which remains unchanged in the air, and may be rubbed to a bright yellow powder. The four equivalents of water are lost in the drying. The acid has a strong affinity for bases, and will displace some of the strong mineral acids. It dissolves in strong sulphuric acid without change, and the solution may be heated without producing any blackening. By reducing isato-sulphuric acid with sulphide of ammonium, the authors obtained a new acid, which they have named *Hydrindin schwefel saure*, (and which we may translate sulphydrindic acid), $C_{16}H_6NO_1SO_3, SO_3, HO$. Several other new compounds are described by the authors in this valuable paper.

THE CHEMICAL NEWS.

VOL. V. No. 127.—May 10, 1862.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

A New Method of Clarifying Saccharine Liquids, Juices, and Syrups, and of Revivifying the Animal Charcoal used in the Manufacture of Sugar, by H. LEPLAY *and* J. CUISINIER.

OF late years, most of the improvements in the manufacture of sugar have been directed to the disuse of animal charcoal. Having for many years witnessed the services which animal charcoal has rendered, and still renders, we have directed our researches in a direction entirely different. Our principal object has been to analyse the action exercised by animal charcoal on saccharine liquids at each stage of the manufacture, the duration of this action, and its exhaustion. We have sought to restore, by easy and speedy methods, the absorbing properties it loses by use, and to ascertain the cause of its various absorbing properties, on which chemistry hitherto has thrown but little light. This cause being discovered, we can, so to speak, increase it at will, and thus effect in saccharine liquids, juices, and syrups, a greater degree of purification than can be obtained by the ordinary means.

This study has led us to the discovery of a new method for refining saccharine liquids, and of revivifying animal charcoal, as shown in the following principal results in the manufacture of beet-root sugar:—

1. To completely do away with the use of fresh animal charcoal.

2. To abolish revivification at a high temperature (kilns for revivification, &c.).

3. To greatly reduce the quantity of charcoal used in the course of the work, and thus to effect a notable economy.

4. To obtain sugars of a superior quality, with a larger yield, without changing the apparatus now in use.

5. To reduce considerably the net cost of sugar.

Now, to point out this new method. In the ordinary manufacture, a filter filled with granulated charcoal lasts from twelve to twenty-four hours. The absorbing properties of the charcoal then appear to be exhausted, and have to undergo revivifying processes, the principal being calcination in a closed vessel at a high temperature. The animal charcoal thus revivified does not completely recover its primitive qualities, and its value as an absorbent is reduced one-half, and sometimes more.

In the ordinary method, all the absorbing properties of the animal charcoal are supposed to be exhausted at the same time, and the object of the revivifying process is to restore them equally and simultaneously.

The following is, on the contrary, the fundamental idea of our method:—

1. Granulated charcoal plays a manifold part, and has various absorbing powers, which act independently, and do not become exhausted simultaneously.

2. In the successive revivification of the absorbing properties of animal charcoal, according as they become exhausted, by various means appropriate to the nature of the matters the charcoal has absorbed.

3. In the possibility of augmenting at will the energy of the absorbing properties of the charcoal, and thus to render its refining action on juices and syrups more complete.

4. In obviating any necessity for a temperature higher than that of boiling water or free steam.

By investigating the process of the filtration of juices and syrups, we find that the exhaustion of the absorbing properties of the charcoal can be divided into three periods, which we will examine successively.

The first series of absorbing properties is almost entirely exhausted after a few hours' filtering,—say, under ordinary circumstances, about four hours. These are the properties which affect viscous, nitrogenised, ammoniacal, sapid, and odorous matters, which injure the fluidity of syrups, their crystallisation, the hardness and consistence of the grain, the quantity and quality of the sugar, and which impart to rough sugars the odour and flavour peculiar to the produce of the beet-root. We completely re-establish the primitive absorbing properties by passing a current of steam through the granulated animal charcoal contained in the filter. The absorbing properties of animal charcoal can thus be regenerated indefinitely.

A much longer time is required to exhaust the second series of absorbing properties. They last six or eight times as long as those of the first series. The period of exhaustion varies with the alkalinity of the defecated juices and syrups. Free alkalies, lime, potash, soda, salts of lime, and other saline matters are absorbed by the series in question. These matters especially contribute to colour the juices and syrups during evaporation by destroying the sugar, and when present in large proportion prevent crystallisation. We revive these absorbing properties by pouring a weak solution of hydrochloric acid over the charcoal contained in the filter and by sufficiently prolonged washings in water.

The third series comprise the absorptive properties of charcoal for colouring matters. They last thirty or forty times as long as the first series. Moreover, the presence of colouring matters in these syrups is of no great importance when they are transparent and brilliant and hold no matter in suspension. White sugars can be obtained from coloured syrups, and when, from the tint of the products, it is deemed necessary to revivify the power of absorbing colouring matters, we make use for this purpose of a weak solution of boiling caustic alkalies.

We use these various means of revivification either in the filter itself or in a special apparatus similar to the filter.

The various methods of revivification just described restore the absorbing properties of the animal charcoal to their primitive state, but without augmenting them.

We have sought, in the production of a new fixed product in the charcoal itself, for the solution of the problem of augmenting the absorbing properties of charcoal.

When one equivalent of biphosphate of lime is added to one equivalent of tribasic phosphate of lime, identical with that which enters into the composition of animal charcoal, the two phosphates combine and form a third, which is a phosphate with two equivalents of base. This is explained by the following formula:—

$$PO_5.3CaO + PO_5.CaO.2HO = 2(PO_5 2CaO.HO).$$

This new phosphate is insoluble in water, has no acid action on litmus paper, produces no inverting action on the sugar, and possesses very energetic absorbing properties.

What takes place in a test-glass with tribasic phosphate of lime is produced in the same manner in a filter filled with granulated animal charcoal when a weak solution of biphosphate of lime is poured over it. The same effect is produced with powdered animal charcoal. Charcoals treated in this way possess greater absorbing powers, which can be varied at will, and a smaller quantity of this charcoal suffices to produce a greater degree of refinement in juices and syrups.

Further, we have utilised for clarifying and purifying saccharine liquids the singular property possessed by the phosphate with three equivalents of lime, of precipitating itself in a gelatinous form, and of carrying with it all matters hindering the transparency of syrups, much more effectual than albumen, blood, and other matters used in clarification. In short, our processes are founded on attentive and inductive study of the singular and useful properties of different phosphates of lime, and of their application to the refinement of saccharine liquids, particularly beet-root juice and syrups.

The foregoing processes are in operation at two important sugar-works in the department of the Oise,—one at Fraucières, belonging to MM. Bachoux and Co.; the other at Frogers, belonging to MM. Daniel and Co. The quantity of sugar made at these two works by our processes is now about 300,000 kilogrammes. This manufacture has sufficiently shown the value of our processes and the reality of the advantages they offer. Our processes can be applied with like success to the manufacture of cane sugar as well as sugar refining.

On the Preparation of Protoxide of Nitrogen by the Moist Way, by M. H. Schiff.*

Various substances, such as sulphide of ammonium, sulphide of potassium, moist finely-divided iron, possess the property of changing binoxide of nitrogen into protoxide. Nascent hydrogen has the same property, and we know that greatly diluted nitric acid disengages protoxide of nitrogen in presence of zinc or metallic iron. Moderately concentrated nitric acid, on the contrary, yields binoxide of nitrogen; but if weak sulphuric acid is added to the mixture, the gas disengaged becomes in a short time protoxide of nitrogen free from binoxide. The addition of sulphuric acid, instead of intensifying, as it generally does, the action of the nitric acid, weakens it,—a phenomenon which can be explained only by a reaction of the nascent hydrogen on the binoxide of nitrogen.

Protoxide of nitrogen, sufficiently pure for ordinary purposes, can be readily prepared from zinc and a mixture of 1 part of concentrated sulphuric acid, 1 of concentrated nitric acid, from 9 to 10 parts of water. It is advisable to pass the gas through a tube filled with pumice-stone saturated with ferrous sulphate, to prevent the admixture of a small quantity of binoxide of nitrogen.

* *Annalen der Chemie und Pharmacie*, vol. cxviii. 84.

On the Detection of Bromine, by M. Fresenius.

According to M. Balard, the best vehicle for dissolving bromine just displaced by chlorine is sulphide of carbon, a process long used in France for detecting iodine. M. Fresenius, who has verified this fact with his usual care, insists on the necessity of avoiding excess of chlorine, and of employing sulphide of carbon free from sulphurous and sulphuric acid.

His preference for sulphide of carbon over ether and chloroform is founded on a series of direct experiments with standard solutions containing various proportions of bromides. Solutions containing only $\frac{1}{30000}$th of bromine in the state of bromide of potassium, when treated with the requisite quantity of chlorine, do not communicate the least colour to ether or chloroform, while sulphide of carbon acquires a decided yellow tint.

This vehicle, then, answers best for this purpose. Moreover, being heavier than water, it sinks to the bottom of the liquid with the bromine it has dissolved, and there remains.

If the bromide is accompanied by an iodide, the iodine must be previously eliminated by adding a little hyponitric acid and a drop of sulphide of carbon, which takes away the displaced iodine. After this the separation of the bromide may be proceeded with.—*Zeitschrift für Analytische Chemie*, i. 46.

On the Commercial Analysis of Chrome Ores.

We have received a valuable communication on this subject from Mr. C. L. Oudesluys, of Baltimore, the proprietor of extensive deposits of chrome iron ore near that city. Our correspondent writes:—

"I see in your 'Notice to Correspondents' that you call upon me to furnish my formula. I enclose one received from a chemist here, by which he has obtained results very similar to those obtained by Dr. Genth from a part of the same sample; but Dr. Genth's formula is only known to himself. I had an interview with him yesterday. He has consented to communicate his formula to Edward Franklin,* Esq., a chemist residing in London, for examination, and is willing to abide a test of a sample of fused and pulverised ore he will send to Mr. Franklin, and thinks it will agree with a test of a part of the same sample made by Genth. Dr. Genth thinks that the English formula, as stated by Charles O'Neill, of Manchester, is good (Chemical News, No. 123); but that he should take 7½ grains for fusion instead of only 5 grains, and should fuse an hour or more instead of only half an hour. He thinks Mr. O'Neill loses chromic oxide in his solutions."

The following is the Baltimore chemist's process referred to by Mr. Oudesluys:—

"Half a gramme of the finely pulverised ore is taken and fused in a platinum crucible with three times its weight of bisulphate of potash for one hour; then allowed to cool, and the same amount of a mixture (of equal parts of nitrate of potash and carbonate of soda) put on the top, and again fused for another hour, then

* Qy. Frankland.

allowed to cool, and digested in a porcelain dish for two or three hours, with water on the sand bath. Then filtered, and well washed out with boiling water. The filter is then treated with hydrochloric acid for two or three hours in a warm place until the sesquioxide of iron is dissolved; then filter, and if there is not too much undecomposed chrome remaining, it can be weighed and deducted, or if too much remains it must be again fused as before. The chromic acid solution is warmed, and carbonate of ammonia added to precipitate any alumina, silica, or lime in solution, allowed to stand an hour and filtered, then hydrochloric acid added to the solution until acid, then warmed and sulphurous acid added to reduce the chromic acid to sesquioxide, then ammonia added, and allowed to stand twelve hours, filtered, washed out by decantation, dried, burnt, and weighed."

Mr. Oudesluys remarks upon this:—

"By the above method, a sample of grain ore, of which the enclosed sample is a part, gave 41·04 per cent. oxide of chrome, or 53·07 chromic acid. It then contained 2·71 per cent. of moisture. If dry, as it is now, it should have contained 42·30 oxide or 55·38 of chromic acid. By O'Neill's analysis, as reported by him, a part of this sample analysed dry gave 46·02 acid,—a discrepancy of 9·36 per cent., thus:—

Analysis made in Baltimore, dry . . .	55.38 acid
" " Manchester, by O'Neill.	46·02 "
Difference	9·36

"Baltimore, April 11, 1862."

On the Preparation of Caustic Soda, by M. WOEHLER.

THIS process consists simply in calcining nitrate of soda with peroxide of manganese. No chameleon is formed, as might be supposed, since the nitrate decomposes long before the mixture can reach the temperature necessary for the production of manganic acid.—*Annalen der Chemie und Pharmacie*, cxix, 375.

On the Preparation of Sulphur Soluble in Sulphide of Carbon, by M. L. FAUCHER.

THE works of MM. Ch. Deville, Fordos, Gélis, and Berthelot show that, besides sulphur soluble in sulphide of carbon, there exists a variety of sulphur insoluble in this liquid. Sulphurs obtained by distillation contain insoluble sulphur varying in quantity with the rapidity of the process of cooling from 2 to 3 per cent., as in roll sulphur, to 35 or 40 per cent., as in flower of sulphur.

Thus, to obtain pure soluble sulphur, it is necessary only to digest any kind of sulphur in sulphide of carbon, to filter the solution through cotton, and then to evaporate in a distilling apparatus. But the disagreeable odour and inflammability of sulphide of carbon render its employment both unpleasant and dangerous. Moreover, the sulphur thus obtained has to be freed from all trace of sulphide of carbon by repeated washings in ordinary alcohol.

It is easier and more convenient to obtain perfectly pure soluble sulphur, by boiling for several hours in a water bath, any kind of sulphur with a solution of sulphite of soda, insufficient to dissolve it entirely. Thus, 100 grammes of flower of sulphur, kept boiling for four or five hours in a solution containing 16 grammes of sulphite of soda to 100 grammes of water, would yield about 70 grammes of perfectly pure soluble sulphur.

This result proves, in the first place, that sulphur insoluble in sulphide of carbon dissolves more rapidly than soluble sulphur in sulphite of soda. Thus, by submitting separately two samples of sulphur, of 50 grammes each, one soluble, the other insoluble, to the action of a solution of 50 grammes of sulphite of soda in 500 grammes of water, and weighing the residue after one, two, three, and four hours' boiling, I obtained the following results:—

	S. soluble.	S. insoluble.
After 1 hour . .	47 gr.	24 gr.
" 2 " . .	25 gr.	10 gr.
" 3 " . .	16·75	2·5
" 4 " . .	10·07	0·2*

The difference in the rapidity of absorption of these two sulphurs led me to inquire whether sulphite of soda acting on a mixture of the two sulphurs would not dissolve all the insoluble sulphur before affecting the soluble.

To determine this question, I took three portions, A B, and C, of the same flour of sulphur, each portion weighing 50 grammes. I treated them separately in a boiling-water bath with a solution of 10 grammes of sulphite of soda to 100 grammes of water. After an hour's boiling, I removed the sample A, which was reduced to 42 grammes; two hours' boiling reduced B to 39 grammes; and after three hours, C was reduced to 37·30 grammes. By the aid of sulphide of carbon, I found their composition was,—

Specimen.	A.	B.	C.
Soluble sulphur . .	37·5	35·96	36·48
Insoluble sulphur .	4·5	3·04	0·82
	42·0	39·00	37·30

By noticing that the flour of sulphur employed gave the composition in centièmes,

Soluble sulphur . . .	80·88
Insoluble sulphur . .	19·12
	100·00

and that each of the preceding compositions gives in centièmes,

	A.	B.	C.
Soluble sulphur . .	89·30	92·21	97·79
Insoluble sulphur .	10·70	7·79	2·21
	100·00	100·00	100·00

it will be understood how sulphite of soda, by a sufficiently prolonged action on the flour of sulphur, can separate from it the soluble sulphur in a state of perfect purity.

The tendency of insoluble sulphur to dissolve itself more rapidly than soluble sulphur, is not the only cause of this separation. In fact, I have ascertained that by treating insoluble sulphur with a quantity of sulphite of soda, insufficient to dissolve it completely, that this insoluble sulphur is transformed by a few hours' boiling into soluble sulphur.

Finally, the aqueous liquid of the sulphite of soda exercises a double action on the flour of sulphur. In the first place, it dissolves insoluble more rapidly than soluble sulphur; secondly, it transforms insoluble into soluble sulphur. For these two reasons, sulphite of soda is an excellent agent for obtaining pure sulphur soluble in sulphide of carbon.—*Journal de Pharmacie et de Chemie.*

* In a former number of the *Journal de Pharmacie et de Chemie*, vol. xxiv. p. 343, M. Favre showed that insoluble sulphur, extracted from flower of sulphur, is more rapidly attacked by hypochlorous acid than soluble sulphur.

TECHNICAL CHEMISTRY.

Contributions towards a Knowledge of the Inorganic Constituents of Plants, by Dr. Charles A. Cameron, *M.R.I.A., F.C.S.L, Corresponding Member of the Agricultural Societies of New York, Belgium, &c.*

At a meeting of the Royal Dublin Society, held on Monday evening, April 16, Professor Barker in the chair, the following paper was read:—

One of the most interesting problems in the chemistry of vegetal physiology is, "What are the inorganic constituents of plants?" Until this problem is satisfactorily solved there can be no true theory of agriculture, and the science of phytochemy must remain very defective in a most essential point.

The inorganic constituents of a plant are, as compared with the organic, very small in quantity; indeed, their apparent insignificance at one time led to the belief that they were unessential components of the vegetal mechanism. We now know—thanks to the intelligently directed labours of a host of investigators—that every plant, excepting a few of the very lowest organisation, must contain a certain amount of mineral matter, which varies both in nature and amount in different kinds of plants, and also, but to a far less extent, in different individuals of the same species. The limits of these variations it is the province of the chemist to discover, and until they are ascertained our knowledge of the nature of the food of plants must remain in a very unsatisfactory state.

Early in the present century, De Saussure, Davy, and Dundonald pointed out the natural affinity which certain plants exhibited for particular earthy and saline substances. Davy showed that clover invariably contained a large proportion of sulphate of calcium, and that silica abounded in oats. Since Sir Humphry Davy's time, but more especially within the last twenty years, a great number of chemists have occupied themselves with researches, the object of which was to discover whether or not the chemical composition of the incombustible part of plants is a specific distinction. Other experimenters have, during the same time, endeavoured to prove the possibility of replacing the mineral bodies commonly and abundantly found in certain plants by others which are either rare constituents of those plants or which occur in them but in minute quantities. These researches have not been barren in useful results. On the contrary, we have derived from them a vast amount of information relative to the composition of the vegetal fabric, and to the relationship subsisting between the plant and the soil on which it grows; and the application of the knowledge so obtained to agriculture and horticulture has improved to a great extent many of the processes of those ancient and important arts. But whilst fully admitting the practical utility of those investigations, it is certain that their results shed, after all, but a feeble light on that part of the domain of nature they were intended strongly to illuminate. Indeed, in numerous instances the experiments of different chemists, made with the same object in view, yielded the most discordant results; and until the subject has been more thoroughly investigated, and until the results of the researches of the various explorers prove more harmonious, the problem, "What are the essential, the useful, and the accidental inorganic constituents of plants?" must remain in a state of but partial solution.

Eleven elementary bodies occur so commonly in the ashes of plants that they are usually indicated in books on botany and agricultural chemistry as essential constituents of plants. These bodies are the following:—Potassium, sodium, calcium, magnesium, iron, silicon, phosphorus, sulphur, chlorine, bromine, and iodine. There are six others which occur—some of them frequently, the rest rarely—in plants, and the claim of each of which to be ranked as a normal constituent of at least certain species is supported by at least one chemist. These elements are the following:—Lithium, aluminium, manganese, copper, lead, and fluorine. My object in reading this paper before the Society is to endeavour to prove by the results of my own investigations, as well as from those of others,* that the last named substances, and one at least of the others, are not essential constituents of plants. My experiments extended over a period of five years. They were conducted in the most careful manner, and every precaution against error that I could think of was scrupulously observed. I started not with the object of experimentally establishing a preconceived theory, but simply with the view of elucidating the truth. I trust, therefore, that the following statements may be considered worthy of being added to what the late Professor Johnston termed "the multiplication of results in which confidence can be placed."

Experiments with Potassium.

Potassium, in combination with oxygen and chlorine, appears to be a constant constituent of plants. It exists in great abundance in most species of the orders Umbelliferæ, Compositæ, Gramineæ, Amentaceæ, and Chenopodiaceæ. Some members of these families evince so great a partiality for potash that it is often found to constitute one-half of the weight of their ashes. The fact of potassium being constantly present in every part of plants, favours the supposition that it is an indispensable constituent of the vegetal fabric. The statements of the analyses of the ashes of plants are scattered throughout the pages of so many books and journals, and published in so many languages, that it is all but impossible that every one of them should come under the notice of even the most industrious researcher. I have wandered through the pages of hundreds of books and periodicals in quest of statements of the analyses of the mineral part of plants, and in several thousands of them which I have perused I only noticed in one or two instances the absence of potassium. According to James, the ash of the *Fucus vesiculosus* yields 22·25 per centum of soda and chloride of sodium, but is destitute of potassium. It is more than probable that in James's analysis the amount stated to be soda was made up of the two fixed alkalies, as it is well known that all the fugaceæ contain a large proportion of potassium compounds. Gödechens proved that it contained no less than 30 per centum of oxide and chloride of potassium. My examination of several specimens of the same plant, grown upon the coast of the County of Dublin, showed that potassium was one of its most abundant constituents. In fourteen analyses of different kinds of fucus made by Forchhammer, the alkalies were equal in amount. Indeed, the results of all the analyses of the sea-weeds made by Claubry, Hodges, Johnston, and others, agree in setting down potassium as an invariable component of the ashes of those plants. In two analyses of the red beech, published by Baer, potassium does not appear;

* Chiefly those of Liebig, Daubeny, Way and Ogston, Rammelsberg Weigman, Polstorff, Magnus, Voelcker, Boussingault, the Prince of Salm-Horstmar, Bischof, Hruschauer, Staffel, and Erdmann.

but in the seeds of that tree, examined by Souchay, 18·13 per centum of potash was found. Bottinger and Berthier found potassium in the wood. In twelve analyses of the beech made by me the following results were obtained:—

Substances Examined.	Potash.			Soda.		
	No. 1.	No. 2.	No. 3.	No. 1.	No. 2.	No. 3.
Leaves of the Copper Beech	10·11	6·22	5·38	6·08	10·20	10·00
Wood of ditto	11·30	4·17	1·90	2·06	7·32	8·12
Leaves of the White Beach ..	10·20	8·00	6·83	6·96	6·20	6·12
Wood of ditto	12·60	7·14	6·44	2·51	4·65	2·31

According to Will, Fresenius, and Daubeny, wheat grown near the coast contains more potassium and less sodium than it does when cultivated in situations remote from the sea; but the results of the analyses of the ashes of wheat grown in different localities, made by Way, point to an opposite conclusion.

In the analyses of fossil fuel (the altered remains of plants), the absence of potassium has been frequently noticed. Baer (who appears to have an instinct for ascertaining the absence of this element from plants and their remains) states that it does not occur in the ashes of two varieties of coal. He, however, detected it in the ashes of other kinds. But even admitting that potassium has never been found in the ashes of fossil plants, it would not prove that it was unessential to the living plant. Who can tell the precise nature of the changes which vegetal substances undergo in their transition from the condition of living tissue to that of fossil coal? During the process of its partial decomposition, may not the alkaline chlorides, if present, be separated by the agency of water? To show how little light the analysis of the mineral part of coal is likely to throw on the nature of the normal inorganic constituents of plants, I need only quote the curious results of Richardson's examination of the ashes of coal used at Kelso, in Scotland.

In 100 parts there were the following substances:—

Potash	18·34
Soda	6·87
Magnesia	1·01
Sesquioxide of iron	26·99
Protoxide of cadmium	1·42
Protoxide of zinc	2·03
Protoxide of nickel	1·38
Silicic acid	1·84
Sulphuric acid	21·20
Titanic acid	7·01
Chlorine	9·57

My own investigations, the results of which I shall now proceed to state, lead me to conclude that potassium is essential to plants, but that it may be partly replaced by sodium. My experiments to determine this point were conducted in the following manner:—

I used twelve vessels made of block-tin, each twenty inches in height, and varying in area from sixteen inches to three feet square. These vessels were partly filled with artificial soil composed of the following substances:—

Coarse quartzose sand	25 parts
Alumina	15 ,,
Sulphate of barium	60 ,,

The quartzose sand had been well washed with pure hydrochloric acid, and subsequently with distilled water. The alumina was thoroughly freed from every substance with which it is usually found associated. By repeating many times the process of dissolving it in hydrochloric acid, precipitating it by means of ammonia, and thoroughly washing the precipitate with warm distilled water, the complete separation of potassium was rendered certain. The sulphate of barium was prepared by mixing a dilute solution of chloride of barium with sulphuric acid. The precipitate obtained was boiled in hydrochloric acid, and subsequently repeatedly washed with hot distilled water. Sulphate of barium is not absolutely insoluble in water, but it is so nearly so that I have grown plants in it repeatedly, and have never found them to take up the most minute trace of it.

This artificial soil I found to answer my purpose very well, and no other kind, except in one instance, was employed in any of the experiments described in this paper.

The twelve vessels were divided into three groups, *a*, *b*, and *c*; the vessels of each group were then numbered 1, 2, 3, and 4.

The vessels of group *a* were supplied respectively with a compound of the following substances:—

Chloride of sodium	20
Silicate of sodium	30
Bicarbonate of sodium	2
Sulphate of sodium	2
Nitrate of sodium	2
Phosphate of sodium	2
Tribasic phosphate of calcium	30
Hydrated sulphate of calcium	30
Carbonate of calcium	20
Phosphate of magnesium	26
Sesquiphosphate of iron	14
Carbonate of magnesium	2
Recently-precipitated silicic acid.	30
	210

The vessels of group *b* were supplied with a mixture similar to that placed in the vessels of group *a*, but with the addition of four parts of potash.

In the mixture supplied to the vessels of group *c*, all the ingredients above stated were present, and in the same proportion, except the sodium compounds, which were replaced by equivalent quantities of potassium salts.

The quantity of artificial manure applied, was about 2 per centum of the weight of the soil; a larger proportion would probably have proved injurious.

In vessels No. 1 of the three groups, seeds of barley were sown; in vessels No. 2, seeds of oats; in vessels No. 3, seeds of peas; and in vessels No. 4, seeds of the grass *Poa trivialis*.

An average number of the seeds germinated and produced stems. During the development of the plants, the nitrogen required for their nutrition was supplied by the nitrate of sodium, and by watering them with an exceedingly dilute solution of carbonate of ammonium. Carbon was supplied by the bicarbonate of sodium, and by watering the plants with a solution of carbonic acid gas in water: gaseous carbonic acid gas was also supplied to them. The plants were placed in a good southern aspect, and were protected by large glass shades.

Results:—The barley seeds sown in the soil destitute of potassium, germinated perfectly and produced plants. These were reduced to twelve in number, and their development progressed in the following manner:—By the twentieth day three of the plants had perished. Of the nine remaining, only two were healthy and had attained the height of ten inches. The others were sickly, and varied in height from six to nine inches. On the fortieth day only one plant was alive, and this presented unmistakeable symptoms of approaching death. By the forty-second day every plant had perished.

The oat seed sown germinated, and produced stalks, but, as in the case of the barley, not one of them reached a perfect state of development, and no effort to produce seed, or even to flower was made.

The peas sown were the variety termed "Warwick Gray," which, under ordinary circumstances, are hardy, vigorous plants. In this case, the haulms were very slight, and of a pale colour; in no instance were there more than four small leaves developed; the greatest height attained was only twelve inches; and all the plants perished before the twenty-eighth day.

The *Poa trivialis* seeds produced plants which appeared to thrive better without potassium than any other of the plants experimented upon. Some of the culms perished very soon, but others survived for nearly two months, and in several instances made a feeble attempt at flowering; but in no case was the most minute seed developed.

The barley sown in vessel No. 1 of group *b*, germinated and produced twenty-nine plants. These were reduced to twelve, of which six perished within thirty days, two more within forty, and the four others attained an average height of 23 inches, and produced small ears, with, on an average, eighteen little grains.

The oat seed sown in vessel No. 2, group *b*, yielded almost precisely similar results to those above stated. The larger number of plants perished, and the survivors were stunted, and produced but a small quantity of grain.

The peas throve better than the cereal plants. Of six plants, one attained the height of 2 feet 1 inch; two were under 15 inches, and one under 12 inches, all these produced very small pods, containing in each three seeds. The two others perished without flowering.

The *Poa trivialis* grew up very luxuriantly, and produced seeds, which, however, were under the normal size, and not very numerous.

The barley, oats, peas, and grass to which the potassium (vessels 1, 2, 3, 4, group *c*) salts, but no sodium compounds, had been supplied, germinated perfectly and produced plants, which in due season performed the last function of vegetation—the production of seed.

The plants grown in the vessels of group *b* were calcined, and their ashes submitted to analysis, the results of which are given in the following table:—

Analysis of the Ashes of (whole) Plants, grown in soils containing a small proportion of Potassium compounds.

	Barley.	Oats.	Peas.	Poa trivialis.
Potash and Chloride of Potassium ..	4·76	6·04	5·22	7·16
Soda and Chloride of Sodium ..	11·62	10·14	14·01	20·46
Lime	10·40	8·05	40·90	11·48
Magnesia	10·11	10·75	10·42	4·60
Sesquioxide of Iron	0·96	1·22	00·82	0·40
Sesquioxide of Manganese	0·10	trace	0·16	trace
Silicic Acid	46·61	50·42	9·20	38·20
Sulphuric Acid	4·16	5·12	6·10	5·04
Phosphoric Acid	11·28	8·26	13·17	12·66
Total amount ..	100·00	100·00	100·00	100·20

The centesimal results make up the whole number 100, the loss being distributed over the various substances in proportion to their amounts.

The results of these experiments tend to prove:—

1st. That barley, oats, peas, and *Poa trivialis*, cannot be perfectly developed on a soil destitute of potassium.

2nd. That sodium is capable of replacing potassium in those plants to a limited extent.

3rd. That when sodium predominates over potassium in those plants they are stunted in development, flower with difficulty, and produce very inferior seed.

4th. That there is every reason to infer, from the results of this investigation, that similar researches on other plants would give similar results.

The chief features of interest in this investigation, besides those alluded to above, are the following:—The remarkable instinct which guided the plants in seeking out and appropriating the minute quantity of potassium contained in the soil.† The grass, *Poa trivialis*, which, according to Way and Ogston, naturally contains no sodium, taking up so large a quantity of that element. The peas thriving better than the oats and barley, and in this respect being exceeded by the grass. The plants supplied with every substance considered essential to their nutrition, although attaining to a perfect development, being very inferior to those grown in the field. For this fact I am unable to assign a satisfactory reason.

(To be continued.)

PHARMACY, TOXICOLOGY, &c.

On the Determination of the Presence of Sulphates of Cinchonine and Quinidine in Commercial Sulphate of Quinine, by M. Roger.

This process, which has been modified by various chemists, remains, nevertheless, essentially the same; all the modifications suggested have been based on one chemical fact,—to set the quinine of the sulphate at liberty, and to dissolve it in pure ether. Liebig's process is at present generally employed. It consists, as we know, in treating 1 gramme of sulphate of quinine put into a long bottle, or into a small, easily-stopped burette. To this is added 10 grammes of pure ether, then 2 grammes of caustic ammonia; the vessel is tightly closed and quickly shaken; and, if the sulphate of quinine is pure, a complete solution is instantaneously effected, and the limpid liquid will divide into two layers,—one etherised, containing all the quinine; the other aqueous, containing the sulphate of ammonia. If, on the contrary, the sulphate contains cinchonine or quinidine, a more or less abundant white flaky deposit will be observed at the junction of the two liquids.

I have many times had occasion to use this process, and always with satisfactory results; but then I used ordinary pharmaceutical ether. In one instance, I by chance employed pure instead of ordinary ether, and failed to obtain the same reaction.

In this case the quinine was only partially dissolved in pure ether; some time after, the liquid solidified into a transparent opal jelly, resembling balsam of opodeldoc. After standing for twenty-four hours, the vessel was shaken briskly, and the jelly thrown on a filter became disintegrated; the ammoniacal liquid and the ether saturated with quinine passed quickly, and a granular white substance remained on the filter, containing a large proportion of ether.

At first I thought this substance was cinchonine or quinidine, but then the quantity of it seemed too large for this supposition. After washing the residue several times with ether, then with distilled water, and finally drying it in a hot air stove, I examined it and found that with all the well known quinine reagents it behaved like quinine itself. Having thus positively proved this substance to be quinine, it remained for me to investigate the cause of this phenomenon.

† Less than the one-thousandth part of its weight.

I started with the supposition that the quantity of quinine contained in a gramme of sulphate of quinine is, perhaps, too large to be dissolved in 10 grammes of pure ether, for we have hitherto been ignorant of the degree of solubility of quinine, under the different states in which it is found, but that it might well be soluble in ordinary ether, which usually contains more or less alcohol.

To solve this double problem, I increased the proportion of pure ether; instead of 10 grammes of ether to 1 gramme of sulphate of quinine, I used successively 15, 20, and 25 grammes, and found that only with the largest quantity could I completely dissolve the quinine.

Then it occurred to me to try the effect of previously adding to the ether 1 per cent. of alcohol at 90°; but with this addition the ether gave an imperfect solution, and after a few hours the mixture gelatinised. With 1½ per cent. of alcohol I obtained an almost complete solution, and the mixture remained liquid; and with 2 per cent. the quinine was perfectly dissolved.

The result of these observations is that 10 grammes of pure ether are not sufficient to dissolve completely the quantity of quinine contained in 1 gramme of sulphate of quinine, and that 25 grammes are required to obtain a complete solution; this, then, is the degree of solubility of quinine, recently set at liberty, in pure ether.

That, on the contrary, this same quantity of quinine dissolves very well in ether, with the addition of 2 per cent. of alcohol at 90°, and that ordinary ether acts in the same way only because it always contains more or less considerable quantities of alcohol.

It is, of course, understood that these experiments and observations do not invalidate Liebig's process for recognising sulphates of cinchonine and quinidine—sometimes both together—in suspected sulphate of quinine; it is necessary only, instead of pure ether, to use it with the addition of 2 per cent. of alcohol at 90°. In fact, as we have many times proved, by mixing pure sulphate of quinine with pure sulphates of cinchonine and quinidine in the proportion of 10·6 and 4 per cent. only, these substances remain insoluble, and may easily be estimated at the junction of the two etherised and aqueous liquids. It must, however, be remarked that a portion of the quinidine precipitates to the bottom of the tube.

Finally, may I be allowed to say a few words on this magma—this jelly obtained by employing pure ether? Is it a hydrate of quinine incorporating almost all the ether employed and forcibly retaining it? To me it appears rather as a combination of ether and quinine resembling a hydrate or alcoholate, and might, strictly speaking, be called an etherate of quinine.

As to the sulphate of ammonia formed during this operation, I do not think it is the cause of the formation of this magma.

M. André, who has devoted a short time to these researches, has isolated the quinine of the sulphate by means of ammonia, and after several washings entirely freed the quinine from sulphate of ammonia; nevertheless, when treated by pure ether in the proportions indicated above, the magma was still produced.—*Journal de Pharmacie.*

Royal Institution.—Tuesday, May 13, at Four o'clock, C. T. Newton, Esq., "On Ancient Art." Thursday, May 15, at Three o'clock, Dr. Lyon Playfair, C.B., "On Some of the Chemical Arts." Friday, May 16, at Eight o'clock, J. Scott Russell, Esq., "On the Iron Walls of England." Saturday, May 17, at Three o'clock, Professor Anderson, "On Agricultural Chemistry."

PHYSICAL SCIENCE.

Recent Researches on Metallic Amalgams, and on the Origin of their Chemical Properties, by M. J. REGNAULD.

IN a preceding work I have shown that the chemical properties of amalgamated zinc and cadmium are connected with the thermic phenomena which take place at the moment when these two bodies are united to mercury. My recent investigations tend to generalise these relations. These investigations extend over a considerable number of metals, and prove that, by the fact of amalgamation, some, such as zinc, rise in the scale of positive affinities, while, on the contrary, others, such as cadmium, are lowered.

Among the metals submitted to these comparative experiments, the following, when allied to mercury, become electro-positives: Iron, nickel, cobalt, zinc, tin, antimony, copper, lead, bismuth.

Among these bodies, only zinc, tin, and lead combine with mercury by simple contact and without auxiliary chemical or physical action. I have ascertained that during the amalgamation of tin and lead, and likewise zinc, the temperature is not lowered. Thus then, in all three cases where the temperature can be observed during the reaction, it is found that the positive affinity of the compound augments when the heat of its constitution increases.

Hence, it may be concluded that other amalgamated metals, having identical properties, owe them to the same cause. Let me add that a comparison of their latent heat of fusion, and of the chemical rank they hold, is very favourable to this point of view. It is easy to be convinced of this by examining the table in which the latent heat of fusion of the metals experimented upon are shown. Some are exact, and are the result of calorimetric determinations; others have only approximate vapours, deduced from the relation established by M. Person, between latent heat, the co-efficient of elasticity, and the density of the metals.

	Latent Heat of Fusion.	
Iron	64·171	Calculation.
Nickel	55·397	,,
Cobalt	51·633	,,
Zinc	28·130	Experiment.
Tin	14·252	,,
Antimony	12 455	Calculation.
Copper	33·881	,,
Lead	5·369	Experiment.
Bismuth	12·640	,,

The affinity of the three first metals of the table with mercury differs very slightly from that of zinc; and as their latent heat is decidedly superior to that of zinc, the increase of positive affinity in the amalgams is due to this circumstance.

The electro-chemical properties of the amalgamated metals placed below zinc are also explicable on the same principles. In fact, if on the one hand their latent heat is generally inferior to that of zinc, on the other hand they unite with mercury in virtue of so feeble an affinity that the formation of the alloy lowers the temperature, as is the case with tin and even with lead.

As to the metals analogous to cadmium which disengage heat during amalgamation, and which, according to theory, ought to be lower in the scale of affinities, their place in the group of metals is, through their electro-positive property, far removed from mercury.

Their combination with mercury is the consequence of an energetic affinity, and as their fusing heat seems otherwise to be feeble, the heat produced during the reaction is of remarkable intensity.

Potassium and cadmium, the amalgamation of which is attended with the production of heat to the extent of incandescence, have suggested to me a new bearing of these ideas. Experiments, with results as precise as they are unvarying, have proved that the amalgams of potassium and sodium, formed by these powerful affinities, are negative relatively to pure metals. They present, then, the extreme limits of chemical and thermic phenomena as presented by cadmium. These phenomena will certainly be met with again in various metals of the first sections, the properties of which have been but imperfectly appreciated.

From these researches the following deductions may, in conclusion, be drawn:—

When a metal is amalgamated, its position in the scale of affinities undergoes some modification. The result may be in the contrary direction even for allied metals, for it depends both on the chemical function of the metal and on its latent heat of fusion.

If the temperature is lowered during the combination of the metal with mercury, if, consequently, the constitutional heat of the amalgam is greater than that of the metal, the latter is raised in the order of positive affinities.

When all the thermic phenomena are inverse, when heat is disengaged during the formation of the alloy, the amalgamated metal is negative, in relation to the free metal.—*Comptes-Rendus.*

On the Molecular Dissymmetry of Natural Organic Products, by L. PASTEUR, *Member of the Chemical Society of Paris.*

(Concluded from p. 249.)

X. Here is a very interesting application of the facts which I have just submitted.

Seeing right and left tartaric acids become thus dissimilar solely in virtue of the rotary power of the base, there was room to hope that from this very dissemblance would result chemical forces capable of balancing the mutual affinity of these two acids, and consequently a chemical means of separating the two elements of paratartaric acid. I long sought without success; but I succeeded in it by the aid of two new bases,—isomerics of quinine and cinchonine,—and I obtain very easily, without the smallest loss, by the aid of these latter, quinicine and cinchonicine.

I prepare the paratartrate of cinchonicine by neutralising the base, then adding as much acid as was required for its neutralisation, I crystallised, and the first crystallisations are formed of left tartrate of cinchonicine perfectly pure. All the right tartrate remains in the liquid, because it is more soluble. Then itself is crystallised, and under a different aspect, because it has not the same crystalline form as the right [qy. left?]. One would suppose that he had to deal with the crystallisation of two very distinct salts of unequal solubility.

XI. But the dissemblance of the properties of corresponding right and left bodies, when submitted to dissymmetric forces, appears to me to be of the highest interest in connection with the ideas it suggests on the mysterious cause which presides over the dissymmetric disposition of atoms in natural organic substances. Why this dissymmetry? why such a dissymmetry in preference to its inverse?

Recur with me in thought to the epoch when, having recognised the absolute identity of the physical and chemical properties of corresponding right and left bodies, I had no idea—not even a suspicion—of possible differences between these bodies. It is, indeed, many years since I recognised these identities and these differences.

It was then impossible for me to understand how Nature could make a right body without making at the same time a left body. For the same forces which are in play at the moment of the elaboration of the molecule of right tartaric acid, ought, it seems, to give a left molecule, and there should be only *paratartarics.*

Why even rights or lefts? Why not only non-dissymmetrics, substances of the order of those of inorganic nature?

There are evidently causes for these curious manifestations of the play of molecular forces. To indicate them in a precise manner would be certainly very difficult; but I do not believe I am mistaken in saying that we know one of their essential characters. Is it not necessary and sufficient to admit that, at the moment of the elaboration in the vegetable organism of immediate principles, a dissymmetric force is present? For we have just seen there was but a single case in which the right molecules differed from their left,—the case in which they are submitted to actions of a dissymmetric order.

May these dissymmetric actions, placed perhaps under cosmic influences, reside in light, in electricity, in magnetism, in heat? Are they in relation with the motion of the earth, with the electric currents by which physicists explain the terrestrial magnetic poles? It is not even possible, at the present time, to emit the smallest conjectures in this respect.

But I regard as necessary the conclusion, that dissymmetric forces exist at the moment of elaboration of natural organic products,—forces which would be absent or without effect in the reactions of our laboratories, either on account of the sudden action of these phenomena or on account of some other unknown circumstance.

XII. We come to the last experiment, the interest of which does not yield to any of those which precede, in the proof which it will manifestly afford us of the influence of dissymmetry on the phenomena of life. We have just seen dissymmetry intervene as a modifier of chemical affinities; but it dealt with reactions purely mineral and artificial, and we all know how prudent we should be in the application of results in our laboratories to the phenomena of life. Therefore, I have reserved almost all the views presented in this lecture until I recognised in the most certain manner that molecular dissymmetry offers itself as a modifier of chemical affinities, not only in the reactions of inorganic nature, but in those of a physiological order in fermentations.

This is the remarkable phenomenon to which I allude. It has long been known, through the observation of a manufacturer of chemical products in Germany, that the impure tartrate of lime of the factories, soiled by organic matters, and abandoned under water in the spring, may ferment and yield different products.

That stated, I place in fermentation the common right tartrate of ammonia in the following manner:—I take the very pure crystallised salt, dissolve it, adding to the liquid a very limpid solution of albuminoid matters. One gramme of dry albuminoid matters is enough for 100 grammes of tartrate. Very often the liquid, placed in a stove, spontaneously ferments. I say very often;

I might add, that it always takes place if care is taken to mix with the liquid a very small quantity of one of those liquids in which spontaneous fermentation has been obtained.

Thus far there is nothing remarkable; it is a tartrate which ferments. The fact is known.

But apply this mode of fermentation to the para-tartrate of ammonia, and, in the preceding conditions, it ferments. The same yeast, or leaven, is deposited. All indicates that things go on absolutely, as in the case of the right tartrate. If, however, we follow the steps of the operation with the aid of the polarising apparatus, we very soon perceive marked differences between the two operations. The liquid, at first inactive, possesses a rotary power sensibly to the left, which augments little by little and attains a maximum. Then the fermentation is suspended. There is no longer a trace of right acid in the liquid, which, evaporated and mixed with its volume of alcohol, immediately furnishes a fine crystallisation of left tartrate of ammonia.

We remark at first in this phenomenon two distinct things; as in every fermentation, properly so called, there is a substance which is chemically transformed, and correlatively, there is a development of a body possessing the manners of a mycodermic vegetable. Elsewhere, and it is this which it is important to note at this time, the yeast which causes the right salt to ferment, respects the left salt, in spite of the absolute identity of the physical and chemical properties of the two right and left tartrates of ammonia, whenever they are not subjected to dissymmetric actions.

Observe, then, molecular dissymmetry proper to organic matters intervening in a phenomenon of the physiological considerations and studies the idea of the influence of the molecular dissymmetry of natural organic products, of the great character which establishes, perhaps, the only well-marked line of demarcation which can be placed at the present time between the chemistry of organic nature and the chemistry of inorganic nature.

XIII. Such are, gentlemen, in their totality, the labours with which I have been charged to entertain you.

You have learned, as we advanced, why I have entitled my exposition, "On the Molecular Dissymmetry of Natural Organic Products." It is, in fact, the theory of molecular dissymmetry that we have just established; one of the highest chapters of science, entirely unforeseen, and which offers to physiology entirely new, distant, but certain horizons.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Three Lectures on Spectrum Analysis, by Dr. H. E. Roscoe, *Professor of Chemistry in Owens College, Manchester.*

Lecture II. (*Saturday, April* 5, 1862.)

Ladies and Gentlemen,—In our lecture last Saturday afternoon we investigated the properties of white solar light. We saw that the sunlight which produces upon our eye the impression of whiteness is, in reality, composed of an infinite number of different coloured lights; and we obtained, when we passed this white solar light through a triangular piece of glass, that which we called the solar spectrum—a broad band of variously coloured rays differing from one another in their refrangibility. But we noticed that this solar spectrum is not continuous,—that it is intersected by dark lines which run through the whole length of the spectrum, and which occur always in sunlight. We noticed that these bands occur not only in direct sunlight, but also in reflected sunlight—in the light of the planets, and that these same bands do not occur in starlight. Hence Fraunhofer, as early as the year 1814, stated that these lines observed in the spectra of the sun- and planet-light must in some way have their origin in the sun. We then proceeded to notice the properties of the light given off by artificially heated bodies. We saw that, with the exception of phosphorescence, light is given off only when a body becomes heated; and we divided artificial light as given off from heated substances into two great classes,—namely, that kind of light which is given off when a solid or a liquid is heated, and that kind of light which is given off when a gas is heated. We saw that when a solid or a liquid body becomes luminous it gives off light of every kind between certain limits—that its spectrum is continuous; whereas the light given off by a glowing gas is not of every kind—that such light produces a broken spectrum; and thus we learnt that it was possible to distinguish, by examining the light given off by such glowing gases, between the kinds of gas which were made to glow, but that we could not in the case of liquids or solids decide by the examination of the light what substance was heated; and thus we arrived at a knowledge of the possibility of founding the science of spectrum analysis—a science which will teach us what the chemical nature of a substance is by simply looking at the kind of light given off by its glowing vapour.

I propose in this lecture to notice—for I cannot do more than notice—some of the applications of the principles which we laid down in the last lecture, to the analysis of terrestrial matter; for we find that we obtain, by the application of these principles to the examination of the matter which composes our globe, a knowledge which is as perfectly unlooked for and novel as it is interesting—information concerning the properties and chemical composition of the matter constituting the globe which we inhabit.

We must remember that what we require to do in order to obtain such a knowledge of the constitution of terrestrial matter, is to obtain this terrestrial matter in the condition of a glowing gas. Now, we may divide, for the sake of illustration, the matter composing the globe into three classes,—that matter which is made gaseous and which becomes luminous near the temperature of the coal-gas flame; that matter, in the second place, which is volatile at a much lower temperature than that; and thirdly, that matter which is volatilised, and becomes luminous at a much higher temperature than that of the gas flame.

Thus, for instance, if I place a piece of clean platinum wire in this gas flame for a few moments, we shall observe that it does not impart any colour to the colourless flame. For a moment it does impart a colour, for a reason which I shall have to explain. The platinum itself does not give any colour to the gas flame because it is not volatile at the temperature of the flame, and we do not get any platinum gas. But if I place another substance in this flame; for instance, a piece of common salt, we shall see that this flame is coloured of a peculiar tint, owing to the fact that the sodium is here volatilised, and that it becomes luminous, and gives off its peculiar and characteristic kind of light, namely, yellow. Now, by heating the platinum to a much higher temperature, we can get the peculiar light which it gives off. Thus, for instance, I have here a platinum pole, and by passing an intense electric spark through this, I obtain the platinum, as we shall see in a subsequent part of the lecture, in a state of luminous vapour, and then we find that the platinum also gives out the light which is peculiar and characteristic for platinum alone, and which no other body gives off.

That peculiar chemical substances produce in the flame peculiar colours has long been known, and this fact is used by the chemist as a means of detecting such substances. Thus, for instance, I will here show you a number of such differently coloured flames; here we can produce the luminous vapour of a number of these substances. I can here produce the characteristic yellow flame of sodium. If I bring the salts of potash into this flame I can produce the peculiar colour given by all those salts—a peculiar purple colour. Here I have the peculiar colour which is produced by a very interesting body with which we shall have to do in a subsequent part of the lecture—one of the new alkaline metals discovered by Bunsen, *rubidium;* and this is the flame coloured by the other new alkaline metal, *cæsium*, also discovered by Bunsen. Here we have lithium, which produces this magnificent red colour. Here we have the green produced by barium. All the salts of barium tint the flame of this beautiful green colour. Here we have the red produced by strontium. Here we have the orange produced by calcium, and here I will produce a peculiar blue flame by a substance which differs entirely from these in properties—the non-metallic element selenium. If I bring selenium into the flame, we shall see that this body imparts to the flame a very peculiar and beautiful blue colour. It is extremely volatile, and only lasts for a few seconds. Further on we have the peculiar blue colours communicated to the flame by copper and by boracic acid.

I can show you the same thing in various ways. Here, for instance, I can produce a much larger flame, and show you the colour of the same salts. [A large gas flame was produced from a perforated jet of about three inches in diameter, and urged by a strong current of air. Pumice-stone dipped in solutions of the chloride of sodium, potassium, barium, strontium, calcium, and lithium, were then held in the flame, the colours imparted by those substances being thereby again made evident.]

I will show you one more illustration of this with these papers. These are papers—gun-papers, in fact, which have been soaked in nitric acid, and which have then been steeped in solutions of these various salts. Here you see we shall have rather a quick combustion, but by reflection on the white screen the colour will be shown very well. [The lecturer then burnt gun-paper which had been dipped in solutions of the chlorates of the following substances: sodium, potassium, barium, and strontium.]

As I have said, it has been long known—that these various substances produce certain colours when brought into the flame. But if we now examine more closely what goes on when we have these variously-coloured flames burning before us, and what exact kind of light is given off; that is to say, if we examine the spectra of these differently-coloured flames, we find that we obtain very much more information concerning the matter than we do in this simple way by looking at the flames themselves. We look through a prism, or we employ Kirchhoff and Bunsen's more perfect arrangement, which you see here in the actual instrument before you, or here in the drawing; and we place a bead of the salt, the colour of whose light we wish to examine, in the flames here in front of this slit, as indicated in the drawing. I here bring a bead of chloride of barium into one of these flames placed at one side of the slit. The green light thus produced falls upon the small prism placed over the upper half of the slit, and it is thereby refracted so as to pass into the tube and through the large prism. Into the other flame, placed directly in front of the slit, I bring a bead of chloride of strontium, and the red light which this produces passes directly through the lower half of this slit on to the prism. In this way we obtain two spectra, one in the upper half of the field of the telescope, the other in the lower half; and we are thus enabled to compare very beautifully indeed the spectra which we wish to examine. Suppose, for instance, that we want to know whether a substance really is barium; all we have to do is to place the substance which we know to be barium in the one flame, and to place the substance which we suppose to be barium in the other: if on looking through the telescope we find that these two sets of lines actually coincide—that the lines of the substances which we know to be barium coincide exactly with the lines of the substance which we suppose to be barium,—we then arrive at a very distinct knowledge that the substance is really what we suppose it to be. If we examine with such an instrument as this, which is the latest form of Bunsen and Kirchhoff's apparatus, such a flame as any of those we see burning before us, we observe what is represented very faithfully indeed by these painted diagrams. If we look at the yellow sodium flame, we notice that the sodium spectrum consists of one single bright yellow line, which, when we examine it more carefully with a larger number of prisms, we find is split up into two lines. Now, all the sodium compounds yield this peculiar spectrum; and nothing we know of, besides sodium and its compounds, will yield it. Potassium, which produced the purple flame we saw here, gives us a spectrum consisting of a portion of a continuous spectrum, with a bright line in the red and another in the violet. One of these lines is known as line *alpha* of potassium, and the other as the *beta* of potassium. These lines are not seen in any other substance, and they are seen in every potassium compound from which we can obtain this luminous vapour. Proceeding onwards, we find that we see the spectrum of lithium, consisting of one bright red and one orange line not so bright; and these three lower paintings—strontium, calcium, and barium—represent the spectra of those three alkaline earths. What we notice when we look at these flames through the telescope is exactly what is represented on that diagram.

Now, we may ask ourselves, "This is all very well, but what improvement is this method of analysis upon our ordinary chemical methods? What benefit is it to us that barium gives us these peculiar bands, that strontium yields certain different bands, that calcium produces others again? We know that the reactions of calcium, barium, and strontium are very different, and we can easily detect these substances by the ordinary chemical methods." The answer to this is, that this method is far more delicate than anything which has been hitherto used; that by means of this reaction we can detect such minute quantities that the delicacy of the method is almost past belief. I am sorry to say that I forgot to make an experiment with sodium to show you the delicacy of this process, because I am afraid that we have now so filled the room with that substance that we cannot get the reaction so delicately as I should like. I am afraid the flames will all burn now with the sodium reaction. I want to show you that dust contains sodium,—that we cannot take up a substance which does not contain sodium; and if I heat this platinum wire, we shall see that it contains sodium. The flame becomes distinctly yellow; and if I pass it between my fingers, you will see that my fingers contain sodium, and this you will see by the yellow colour imparted to the flame. I do not know now whether the dust from my coat will show the presence of sodium; it certainly would if the flame were not tinged by the sodium which is already in the atmosphere. I will try my coat. [The skirt of the lecturer's coat was dusted near the flame, and the presence of sodium in the coat-dust was rendered evident.] This is not because it is the coat of a chemist; the dust from a book will show us the presence of sodium. You see that there is sodium in the dust of this book; in fact, every substance contains sodium, and therefore we can understand how Bunsen could recognise the 180 millionth part of a grain of sodium, for this was the small quantity that Bunsen and Kirchhoff found could be easily detected. But not only can we detect such minute traces of substances, but we thus gain important information respecting the distribution of bodies. In illustration of

this, I may mention that lithium—the body which gave us that magnificent red flame, and some of which we have here—was only known to exist in a very few minerals; but Bunsen, on examining the spectra of various substances, saw that this red line, indicative of the presence of lithium, exists almost everywhere. He found it in the ashes of plants; and an experiment, which he is fond of showing, as illustrating the wide diffusion of lithium, is that of holding the end of the ash of a cigar in a colourless gas flame, and showing his friends the occurrence of the red lithium line, when the flame is observed by means of this instrument. We thus see that lithium is contained in the ashes of tobacco, in the ashes of many other land plants, in the oldest formations, in granite, and in the blood of animals. Instead of being, as was formerly supposed, a most sparingly-diffused substance, it is one which occurs almost everywhere; but owing to the small quantity of the substance present, it had been overlooked by our rougher and less exact methods of research. The crowning point of this investigation is, however, that of the discovery of two new alkaline metals by Bunsen. Bunsen, on examining the alkalies contained in the waters of Dürkheim, in Rhenish Bavaria (he had previously separated, by chemical means, all the other bodies from the water, and the substances which were left could only be alkaline substances), saw, on looking at the spectra produced by these, some lines which he had never seen in the spectra of any alkalies before; and he said, "There is a new alkaline metal contained here; this appearance must be produced by some new elementary body;" for no other substance which he knew of, or which he had examined, had ever given these lines before. Now, in a very interesting paper on these two new metals, which he calls rubidium and cæsium,—for reasons I shall explain to you presently,—Bunsen says that from 30 grammes of the mother liquor he obtained only 1·2 milligrammes of the impure salts of these two new alkaline metals. That was all he had to begin with,—about the one-hundredth part of a grain; but still, so sure was he of this method, and so certain was he that his spectrum never failed him, that he set to work at once and evaporated down 50 tons of this water to get some more of this substance. 44 tons yielded him only 105 grammes of the chloride of rubidium, and 70½ grammes of the chloride of cæsium; so that out of 44 tons of water he got only about 200 grains of the mixed chlorides of these two new metals. Here I have a small specimen of the salts of cæsium, kindly sent to me by my friend Professor Bunsen. I have, however, more of the rubidium salts, which now can be obtained in larger quantities from the mineral lepidolite which I have mentioned. Bunsen, then, by the inspection of the spectra of these new alkaline metals determined their presence, and afterwards, having seen these lines, set to work to separate out the metals which caused them.

I will, with your permission, first show you the spectra of potassium and sodium, and afterwards the spectra of the new alkaline metals. Mr. Ladd will be kind enough to show us with the electric light these spectra; but you must not suppose, ladies and gentlemen, that what we get on the screen is exactly what we observe when we look through Bunsen's apparatus, which is adapted specially for analysis; but the results are extremely beautiful and interesting, and, I think, we shall see the distinguishing difference between the salts of rubidium and potassium. I will draw your attention, first of all, to the paintings representing the spectra which we are about to see. Rubidium and cæsium both possess spectra analogous to the spectrum of potassium. The difference in the spectra is but small. The potassium, as I have told you, gives a partially continuous spectrum; rubidium also gives a partially continuous spectrum; and the cæsium likewise gives a partially continuous spectrum; but at either end of all three spectra we find red lines in the least refrangible part, and violet lines in the most refrangible part; the two red rubidium lines are less refrangible than the potassium line, and these red lines [pointing to the diagram] do not exist in the cæsium spectrum. Bunsen has chosen two names for these two metals—cæsium from *cæsius*, a greyish colour, and rubidium from *rubidus*, red, owing to lines of these colours being characteristic of the presence of these two metals.

Here we shall get the sodium spectrum; but I warn you that you will see other lines besides those given by sodium. The bright orange line is due to sodium. The sodium spectrum produces this bright orange line, which is alone seen here when we look at it through the more delicate instrument, and use a finer beam of light and a finer slit. We then see that the sodium spectrum consists solely of two bright yellow lines, which are separated by a very, very slight interval, and each of which is as fine as the finest spider's web. We cannot, unfortunately, exhibit the sodium lines in that way. There you see the splendid spectrum in which the orange band of sodium is very markedly visible. The other lines are not those which we have to attend to at present. It is impossible here, owing to the fact that our carbon points are impure, and contain certain metals mixed with the carbon, to get the pure spectra of the metals; and with this smaller apparatus, unfortunately, we cannot get light enough to throw the spectra on to the screen.

We now have the potassium spectrum, we still see the yellow band of sodium, because, as I have told you, sodium exists everywhere, and it is almost impossible to get the potassium pure; but we shall likewise see two other lines. There is the bright red band at one extreme end of the spectrum, and a violet one at the other end. These two are due to potassium. You notice also that the spectrum is continuous in the centre.

Now, we will take a mixture of potassium and sodium; such a mixture I have here. This mixture contains one part of sodium and twenty parts of potassium, yet the yellow colour of the sodium will cover entirely the purple colour of the potassium; and when we look at the flame with the naked eye we shall see nothing but the yellow flame, which might be produced by pure sodium. [A portion of the mixture was held in the gas flame, to which a bright yellow tint was communicated.] You see this flame is as yellow as if it were pure sodium, yet it consists of one part only of sodium and twenty of potassium. But if we bring this mixture into the apparatus, and if we look at its spectrum, we find that the light of the sodium is kept to its own position. We have the bright yellow line of the sodium which does not interfere with the lines of the potassium, and these come out as distinctly as though no sodium were present at all. By allowing the bright sodium line to appear only where it ought, we see both the potassium lines coming out most beautifully.

Bunsen most eloquently describes, in his Memoir on this subject, the spectra which he sees when he places in the flame a mixture consisting of the chlorides of potassium, sodium, lithium, barium, strontium, and calcium, of each of which substances there is present only the one-thousandth part of a grain. He sees, first of all, the spectra of those substances which are most volatile appearing; the salts of sodium, potassium, and lithium are first seen; their spectra first come out, and these gradually fade away, and the spectra of calcium, strontium, and barium appear in all their vividness. Now, unfortunately, I cannot show you this with the beauty in which it is seen in the instrument when we allow the rays to fall on the retina; but you can see something which is very magnificent indeed. The mixture of all these chlorides together we now place in the carbon cup, and on bringing the upper carbon in contact with the mixture we shall volatilise the compounds, and we shall obtain the super-imposed spectra of all these substances. Mr. Ladd has now placed all the mixed chlorides in the cup, and on making contact we shall have all the

lines appearing. There you see what splendid bands we get now, and you will observe that some of the bands will gradually disappear, the light remaining constant; and others will appear with greater brilliancy, because the more volatile of these salts are driven off. There you notice the bright green bands of the barium. That splendid blue line is produced by strontium. Here we have the sodium; that is the green line of calcium; here we have the bright red line of lithium.

Now, how did Bunsen separate these new metals from one another, and from the old alkaline metals? I must give a moment to this point. In the first place, unless we could examine the spectra of cæsium and rubidium, we probably should never have discovered their existence at all. There is no doubt now that one at least of these newly-discovered substances has been handled by chemists before, but mistaken for potassium, in a certain mineral called lepidolite, which was known to contain lithium, and has now been found to contain a large quantity of rubidium. Rubidium and cæsium are so much like potassium in their chemical characters that, if it were not for this difference in the spectra, we should never have succeeded in separating one from the other, or in detecting any difference between these substances when they were present together.

I can show you that rubidium and potassium are closely analogous. Here we take a solution of chloride of platinum. We know that it produces with potassium compounds an insoluble precipitate, and thus we distinguish these from the sodium compounds with which no such precipitate is produced. We shall at once get, as you will see, a quantity of this bright yellow precipitate of the double chloride of platinum and potassium.

Exactly the same thing we shall see will happen with rubidium. Here I have a solution of the chloride of rubidium. I add a few drops of this solution of chloride of platinum, and immediately we get a precipitate of the double chloride of platinum and rubidium. We cannot in the outward appearance see any difference between the precipitate formed by the rubidium and that formed by the potassium. This is one of the reactions by which we distinguish potassium from sodium, but you see we cannot in this way distinguish potassium from rubidium. We can make a similar experiment with tartaric acid. If we take some chloride of potassium and some chloride of rubidium, and add to each some tartaric acid, we shall obtain with both a white precipitate of the insoluble bitartrate.

But we can distinguish these new metals from potassium, and separate them by a difference of property which is exhibited by these platinum salts. We can distinguish them in this way: Here I have a solution in water of the double chloride of potassium and platinum. I will place some of this in both of these glasses. You will notice that when I add some of the chloride of potassium to this (the potassium bichloride of platinum) we obtain no further precipitate; it is impossible that we should thus obtain a precipitate; but if we add some chloride of rubidium to the potassium bichloride of platinum solution, we shall get at once a yellow precipitate showing that this double chloride of potassium and rubidium is much less soluble than that of potassium and platinum. This is the way in which Bunsen separated rubidium and cæsium from potassium. Bunsen then investigated the salts; and we now have a Memoir, written by himself and Kirchhoff—the second Memoir on Spectrum Analysis—which contains a very elaborate and beautiful description of their researches on this subject. We are now acquainted with the nitrate, with the sulphate, with the carbonate, with the oxalate, with the hydrate, and even with the two new metals themselves; so that we have a chemical history of those two substances, which we must teach in future in all our classes. I must mention also that both rubidium and cæsium form salts which are isomorphous with the potassium salts; they crystallise in the same form, and they possess an analogous composition. Cæsium can be separated from rubidium by the solubility of the carbonate of the former metal in alcohol. The atomic weight of rubidium is 85·36, that of cæsium is 123·35.

We cannot see the end to which the application of this principle may lead. During the first few months it has led to the discovery of these two new metals, and we have not only their spectra examined, but also are acquainted with most of their salts. Another observation which shows us how rich is this field of inquiry, is, that a new elementary substance has probably been discovered by Mr. Crookes. He has not yet succeeded in preparing a large quantity of the body, and thus proving its chemical characteristics distinctly, but he has prepared a substance which seems to differ in its chemical characters from all the other elements, and gives a totally different spectrum, consisting of one bright green line. Much is not known at present about this substance, but there seems very little doubt that it will turn out to be a new chemical element, to be added to the rather large family of elementary bodies.

Now, although Bunsen and Kirchhoff are the real discoverers of this method, because they carried it out with all due scientific accuracy, and placed it on a sure foundation, yet we must not suppose, because they worked it out, the ground was, before them, absolutely untrodden. No great discovery is made all at once. There are always stepping-stones by which such a position is reached, and it is right to know what has been done previously, and to give such credit, as is their due, to the older observers. Now, the first notice we have of the property of these coloured flames was made by Thomas Melvill in the year 1752. He observed the yellow light given off by sodium vapour, but he did not know that it was due to soda, though he observed that a mono-chromatic light was given off. Sir David Brewster, in the year 1822, proposed a mono-chromatic lamp; but the original observation was due to Melvill. Herschel, in the year 1822, observed the spectra of several coloured flames, and in an article on Light in the *Encyclopædia Metropolitana*, in the year 1827, Herschel says,—"The colours thus communicated by different bases to flame afford in many cases a ready and neat way of detecting extremely minute quantities of them." But it is to Fox Talbot, a gentleman well-known to us as one of the first investigators of the beautiful art of photography, that we owe the first valuable suggestions respecting this subject; and it is interesting to remember that Talbot made his experiments in the laboratory of this Institution, under the guidance of Mr. Faraday. Writing in the year 1826, he says:—"The red fire of the theatres, examined in the same way, gave a most beautiful spectrum, with many light lines or maxima of light. In the red these lines were more numerous, and crowded with dark spaces between,"—[these are the strontium lines which you see on the diagrams there]—"besides an exterior ray greatly separated from the rest, and probably the effect of the nitre in the composition." [This really is due to the nitre.] "In the orange was one bright line, one in the yellow, three in the green, a very bright one in the blue, and several that were fainter." The blue line which he mentions was the blue strontium line which you saw, and concerning which I had hoped to speak, but I fear that I must defer that part of my subject. "The bright line in the yellow is caused, without doubt, by the combustion of sulphur." Talbot got wrong there, as did many other early observers. They did not suppose that so small a trace of sodium could produce that yellow light; and even Talbot says that no doubt the yellow line must be caused by the presence of water. He continues:—"If this opinion" (that is to say, the opinion about the formation of these lines) "should be correct, and applicable to the other definite rays, a glance at the prismatic spectrum of a flame might show it to contain substances which it would otherwise require a laborious chemical analysis to detect." That was written in

March, 1826. In February, 1834, he writes:—"Lithia and strontia are two bodies characterised by the fine red tint which they communicate to the flame. The former of these is very rare, and I was indebted to my friend Mr. Faraday for the specimen which I subjected to prismatic analysis. Now, it is difficult to distinguish lithia red from the strontia red by the unassisted eye," (as you have seen here, in fact,) "but the prism displays between them the most marked distinction that can be imagined. The strontia flame exhibits a great number of red rays well separated from each other by dark intervals, not to mention an orange and a very definite bright blue ray. The lithia exhibits one single red ray." "Hence, I hesitate not to say" (says Talbot, writing in 1834) "that optical analysis can distinguish the minutest portions of these two substances from each other with as much certainty, if not more, than any other known method."

Sir David Brewster, in the year 1842, published some interesting observations concerning the spectra of coloured flames. Examining the coincidence of the bright metallic lines and the dark solar lines, he saw—and this is an important observation—that this bright red line in the potassium is a double line; and he noticed, in the year 1842, that this bright red line of potassium is coincident with, or has the same degree of refrangibility as the dark line *A* in the solar spectrum. Now, this was observed also quite lately by Kirchhoff, and entirely independently of Brewster, and they both agree that the dark line *A* is coincident with the red line caused by potassium. This has been lately denied by the French observer, M. Morren; but it seems to me that the researches of two such observers as Brewster and Kirchhoff, made independently of each other, and, especially, at a distance of eighteen years, must be correct.

Professor W. A. Miller, in the year 1845, took up the subject, and investigated the dark absorption bands produced by certain gases, and the bright lines in the spectra of coloured flames. Diagrams of these spectra accompany his Memoir, but they are not sufficiently characteristic to enable us to easily distinguish the particular metal, though in some cases they show lines which Bunsen's diagrams do not exhibit. In his observations he did not refer to the application of these researches for the purpose of detecting the metals producing these bright lines. Here I have some representations of Dr. Miller's lines. This is the barium spectrum; this is the calcium spectrum; and this is the strontium spectrum, as drawn by him in the year 1845. You see they differ from those of Bunsen and Kirchhoff, because Dr. Miller had a continuous spectrum at the same time. His flame was not a non-luminous one, as was the case with the one used by Bunsen and Kirchhoff.

Ladies and gentlemen, I have already taken up the allotted time. I am sorry to say I had a great deal more to say, which I must defer to the next lecture. I may add that I shall be happy to exhibit to those who stay, the spectra of the new metals by means of the electric lamp.

[After the lecture Mr. Ladd exhibited the spectra of rubidium and cæsium. In the rubidium spectrum were seen two bright violet bands, as well as the two characteristic red lines in the ultra-red portion of the spectrum. The violet lines of the cæsium were also seen, and noticed to be less refrangible than those of the rubidium.]

Annual Meeting, Thursday, May 1, 1862.

The Duke of Northumberland, *F.R.S., President, in the Chair.*

The Annual Report of the Committee of Visitors for the year 1861 was read and adopted. The amount of contributions of members and subscribers in 1861 amounted to 3013*l.* 10s., the receipts for subscriptions to lectures were 740*l.* 11s. 6d.; the total income for the year amounted to 4693*l.* 9s. On December 31, 1861, the funded property was 28,655*l.* 17s. 2d.; and the balance at the bankers, 968*l.* 16s. 8d., with six Exchequer bills of 100*l.* each.

A list of books presented accompanies the Report, amounting in number to 524 volumes; making, with those purchased by the managers and patrons, a total of 524 volumes (including periodicals) added to the library in the year. Sixty-three lectures and twenty-one evening discourses were delivered during the year 1861.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to Professor Faraday, for their services to the Institution during the past year.

The following gentlemen were unanimously elected as Officers for the ensuing year:—

President—The Duke of Northumberland, K.G., F.R.S. *Treasurer*—William Pole, Esq., M.A., F.R.S. *Secretary*—Henry Bence Jones, M.A., M.D., F.R.S. *Managers*—The Rev. John Barlow, M.A., F.R.S.; William Bowman, Esq., F.R.S.; Sir Benjamin Collins Brodie, Bart., D.C.L., F.R.S.; Warren De la Rue, Esq., Ph.D., F.R.S.; George Dodd, Esq., F.S.A.; The Earl of Ducie, F.R.S.; John Hall Gladstone, Esq., Ph.D., F.R.S.; William Robert Grove, Esq., M.A., Q.C., F.R.S.; Sir Henry Holland, Bart. M.D., D.C.L., F.R.S.; The Lord Lovaine, M.P.; William Frederick Pollock, Esq., M.A.; Lewis Powell, M.D., F.S.A.; Robert P. Roupell, Esq., M.A., Q.C.; Lieut.-Gen. Edward Sabine, R.A., Pres. of Royal Society; Colonel Philip James Yorke, F.R.S. *Visitors*—Neill Arnott, M.D., F.R.S.; Hon. and Rev. Samuel Best; George J. Bosanquet, Esq.; Archibald Boyd, Esq.; Bernard Edward Brodhurst, Esq.; John Charles Burgoyne, Esq.; George Frederick Chambers, Esq.; Hon. Sir Charles Crompton, Justice of Queen's Bench; Edward Enfield, Esq.; Captain Frederick Gaussen; the Duke of Manchester; John MacDonnell, Esq.; Colonel William Pinney, M.P.; George Stodart, Esq.; Hon. Sir James P. Wilde, Baron of the Exchequer.

CHEMICAL SOCIETY.

Thursday, May 1, 1862.

Dr. A. W. Hofmann, *F.R.S., President, in the Chair.*

Dr. T. Anderson gave a discourse "*On the Chemistry of Opium.*" Since the year 1803, opium had attracted the attention of chemists; of late years the principal point aimed at had been the preparation of morphine in a pure state. In extracting the opium in order to obtain the bases it was better to use only a small quantity of water heated to about 150° F., in which case all the narcotine would be dissolved out, and only woody fibre left; but if a large quantity of water were used, the narcotine was left behind in the insoluble residue. The alkaloids existed in combination with a peculiar organic acid called meconic acid, and another acid had been discovered in opium which was isomeric with lactic acid. The liquid obtained by extraction with water was mixed with a certain quantity of chloride of calcium, and the precipitated meconate of lime separated, after which the solution was concentrated and allowed to stand, when hydrochlorate of morphine and hydrochlorate of codeine crystallised out; these bases could be easily separated by precipitation by ammonia, the codeine being soluble in water. It was generally supposed that the codeine formed a double salt with ammonia, but from Dr. Anderson's experiments this did not appear to be the case. The mother-liquor from which the hydrochlorates had been deposited gave, on the addition of ammonia, a precipitate containing narcotine, papaverine, thebaine, and codeine, together with numerous resinous matters; the presence of the last-mentioned base in the precipitate was remarkable on account of its solubility in water. On dis-

solving this precipitate in alcohol and crystallising, narcotine was deposited, the other bases remaining in solution; and Dr. Anderson imagined that this solution contained a base which had not as yet been noticed. The original mother-liquor from which the ammonia precipitate had been separated still contained bases, which might be extracted by agitating with ether. From the etherial solution meconine could be obtained. The products of decomposition of narcotine under the action of reagents were very interesting. The composition of this alkaloid had been determined by different experimenters, and their results did not altogether agree; for the three formulæ, $C_{44}H_{23}NO_{14}$, $C_{46}H_{25}NO_{14}$, and $C_{48}H_{27}NO_{14}$, had been given, which had been explained by the supposition that there existed three alkaloids agreeing very closely in their properties, and named methylo-, ethylo-, and propylo-narcotine, on account of the decomposition they underwent when passed over soda lime, the three yielding respectively methylamine, ethylamine, and propylamine. By the action of oxydising agents, narcotine might be separated into a basic substance called cotarnine, $C_{24}H_{11}NO_6$, and an indifferent body, which was meconine; three acids being produced at the same time,—namely, opianic acid, $C_{20}H_{10}O_{10}$, hemibanic acid, $C_{20}H_{10}O_{12}$, and a third acid body of the composition $C_{20}H_{10}O_8$. Cotarnine, when acted on by nitric acid, gave an acid containing $C_{16}H_7NO_8$, called apophyllic acid. This acid might be considered as compounded of methylamine and an acid of the formula $C_{14}H_4O_{10}$, and, in fact, when boiled with potash, underwent decomposition, giving off methylamine. On dissolving narcotine in dilute suphuric acid, and exposing it in sealed tubes to a temperature of 300° F., crystals are seen to form, which contain meconine; colouring matter and a humic substance being formed at the same time, while the solution is found to contain cotarnine. When opianic acid is mixed with concentrated sulphuric acid, a purple liquid is produced, which on dilution furnishes a reddish colouring matter which has the properties of alizarin. With regard to the composition of the various substances found in opium, some relation between the different bases might be discovered. Codeine was homologous with morphine, as far as the formula was concerned; but this relation was not borne out by the properties of the two bases, which were very different. The other bases also showed, in some cases, a relation in composition. Several of these alkaloids, when exposed to the action of nitric acid, gave rise to substitution products. The substitution product of codeine could only be prepared by employing a very dilute acid; if stronger acid were used, a resinous acid was obtained, which, when boiled with potash, gave rise to a volatile base. This last reaction was very common among the group of alkaloids. With regard to the variations in the relative proportions of the various bases contained in opium, that obtained from China contained a large amount of narcotine and very little morphine; while the opium from Egypt contained scarcely any meconic acid, its place being taken by sulphuric acid.

Mr. Foster said that from the experiments that he had made in conjunction with Dr. Matthiesson, it appeared that the composition of cotarnine was represented by the formula $C_{24}H_{13}NO_6$, so that cotarnine and meconine together exactly made up narcotine. They had found that the oxidation of meconine furnished an acid containing $C_{20}H_{10}O_{10}$. The action of sodium amalgam on narcotine furnished a basic substance, which appeared to be cotarnine. This reaction was interesting, because it was the reverse of the action of nitric acid, which also gave rise to cotarnine. They represented cotarnine by the rational formula $\begin{bmatrix}(C_{22}H_{10}O_6)''\\ C_2H_3\end{bmatrix}$ N, for the action of nitric acid on this body furnished an acid of the formula $C_{22}H_{12}O_{10}$, and on dissolving cotarnine in hydrochloric acid and heating, a decomposition ensued, chloride of methyl and a body of the formula $C_{22}H_{13}NO_8$, HCl being formed. From their experiments on the decomposition of narcotine by soda lime it appeared that under these circumstances trimethylamine was formed, which had been mistaken for propylamine on account of its platinum salt containing the same per-centage of metal.

Dr. Redwood remarked that he entertained doubts as to the absence of meconic acid in a genuine sample of opium. He had invariably found it to be present in a great number of samples which had come under his notice.

Mr. Morson observed that meconic acid was always present in opium, unless it had been removed by lime or a similar agent.

MISCELLANEOUS.

Chemical Society.—The next Meeting of this Society will be held on Thursday next, the 15th instant.

Recent Improvements in Lucifer Matches.—Of matches prepared with ordinary phosphorus, and which consequently ignite readily upon any friction surface, the "Patent Paraffin Matches" of Messrs. Letchford and Co. are particularly good examples. Instead of the objectionable sulphur coating, melted paraffin is used for impregnating the wood and rendering it more inflammable. Such matches are not likely, therefore, to play havoc with the silver candlesticks and bright metallic surfaces often brought near them in actual service. Their power of remaining uninjured by damp is a special character for which this kind of match is remarkable; in a comparative examination of several different sorts, these only were capable of being ignited after six hours' exposure to a moist atmosphere. On this account they would be particularly suitable for export, and little affected by climate

Action of Bromethylene on Strychnine.—The compounds resulting from the action of bromethylene on strychnine are described by Ménétriès (*Bulletin de St. Petersburg*, t. iv. p. 570). The two bodies were heated in a glass tube placed in a water-bath at 100° for a quarter of an hour. A little spirit of wine is added to facilitate the reaction. A glistening white mass is left in the tube, which is distilled to remove uncombined bromethylene and spirit of wine. A fluid remains in the retort, from which crystals separate on cooling. The crystals are slightly soluble in cold, and very soluble in hot water and alcohol. Their solution gives no precipitate with alkalies. With sulphuric acid and an oxidizer they give the same colour as strychnine. With nitrate of silver they give a precipitate of bromide of silver; but analysis proved that only half of the bromine was precipitated; the whole could only be removed by digesting with moist silver oxide. The analysis of the crystals led to the empirical formula $C_{46}H_{26}N_2O_4B_{22}$. The author examined other compounds of strychnine with nitric acid and chlorine, and those resulting from treatment of the bromine compounds with nitric acid, and describes several new bodies.

ANSWERS TO CORRESPONDENTS.

Downing v. *Chance.*—We can insert no more communications on this subject.

F.R.S.—Your library at Burlington House ought to contain the work. See Vol. vii. p. 338.

M. A. R.—*Querist.*—Received.

Thomas G.—The letter has been forwarded to the person named in it. You will most probably receive a reply by post.

A Manufacturer.—An advertisement in our columns would most likely bring several applications for the situation.

Fessel's Gyroscope.—Can any correspondent kindly refer us to the original paper on this subject?

E. Sherratt.—The mineral occurring in the form of thin crystalline layers between the seams of coal in the Earl Granville's pit, was found by analysis to consist of the carbonates of iron, lime, and magnesia. It is remarkably compact in structure, entirely soluble in acids, and contains the whole of the iron in the lowest state of oxidation.

THE CHEMICAL NEWS.

VOL. V. No. 128.—May 17, 1862.

NOXIOUS AND OFFENSIVE VAPOURS.

THE motion carried by Lord Derby last Friday night, for a Select Committee of the House of Lords to inquire into the injury resulting from noxious vapours evolved in certain manufacturing processes, is of too much importance to a great number of our readers to be allowed to pass without notice. Lord Derby introduced the motion by a remarkably temperate speech, in the course of which he showed that he was well up in the chemistry of Leblanc's process, as well as aware of the extent of our soda trade; and he therefore expressed himself laudably anxious that nothing should be done to seriously interfere with so important a branch of industry. With regard to the evolution of hydrochloric acid gas, against which, we presume, the inquiry is principally directed, and from which Lord Derby himself seems to be a sufferer, that, we think, may be easily disposed of. The escape of this gas, besides being wasteful, is easily prevented. It has been prevented in many manufactories, and might be in all. A condensing tower ought to be erected in every factory, and the subsequent disposal of the acid must be regulated according to circumstances.

It may be urged by some that the remedy at common law is already quite sufficient to prevent a nuisance; but this, however, does not appear to be the fact. In any case, this remedy must be eminently unsatisfactory to manufacturers. We have seen recently that when they have taken every precaution, it leaves them quite at the mercy of wonderful chemists, endowed with marvellous vision, who see *bluish* vapours rise whenever they place on the ground, within sight of a soda factory, a piece of filtering-paper soaked in ammonia. Now, a certificate from a Government Inspector that the works were properly constructed and carried on, would, we imagine, be a satisfactory answer to all actions like that against the Messrs. Chance; and so far the manufacturers would certainly benefit by being placed under inspection.

As regards the pollution of streams by arsenic and other poisons, to which Lord Derby also alluded, there is no doubt that this also may be easily prevented; and it would probably save a good deal of litigation if the works where such a result was likely to happen were also placed under inspection.

But there are some cases in which it is difficult to see how the escape of some noxious vapour—a rather comprehensive expression—is to be entirely prevented; and it behoves manufacturers and smelters to watch well the proceedings of the Committee, and to scan warily the measure which may be introduced into the House of Lords founded on the report, to see that practical impossibilities are not required of them.

It is almost to be regretted that Lord Ravensworth did not succeed in adding the word "offensive" to the original motion; for although there might be more difficulty in completely destroying the nauseous stinks emitted from factories where animal matter is burnt or dealt with, there is no doubt that the use of very simple means would go far towards mitigating the nuisance. The motion, as it stands, however, leaves ample room for the sanitarians to urge the claims of health; and as the Committee proceeds with the inquiry, the objects may perhaps be a little extended.

We have warned manufacturers to be on their guard, and we may conclude with a word to many ingenious readers who may possibly be provided with schemes to abate the evils of which Lord Derby complains. To all such the Committee will give a patient hearing, and we advise them to lose no time in bringing forward their plans.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Sulphide of Arsenic in Commercial Sulphide of Antimony, by R. REYNOLDS, *F.C.S.*

A FEW months since I received a small quantity of powdered black antimony for analysis, being informed that several calves had died after its administration, the animals had been dead some weeks, that the only sources of evidence were the medicine itself, and such facts as had been observed with respect to symptoms. After reporting the analysis, the following outline of the case was supplied to me; but as it occurred at a distance, and I was not directly in communication with the owner of the calves, it is not quite so full as it might otherwise have been.

To each of twenty-four yearling calves was given one ounce of powdered black antimony, in a horn of urine. This was at 9 a.m., and the same evening one calf died, the whole of them being seriously ill. At varying intervals up to ten days, further deaths occurred, until ten animals had succumbed. The others ultimately recovered. The symptoms were great cramp, constipation, and falling off of the hair. In two cases post-mortem examinations were made, and great irritation of the mucous coats of the stomach was found.

Analysis.—The powder was boiled with water, but neither arsenic nor any other matter was dissolved. To determine the sulphide of arsenic present, the process of Wackenroder was adopted (Fresenius). Twenty grammes are deflagrated with forty grammes of nitrate of potash and twenty grammes of carbonate of soda, by projecting the mixture gradually into a red-hot Hessian crucible. The resulting mass is lixiviated with water, filtered, the filtrate acidulated by hydrochloric acid and sulphurous acid added, after which sulphuretted hydrogen is passed through it. The moist precipitate is digested with carbonate of ammonia, filtered, acidulated, and precipitated as tersulphide of arsenic. This method is not rigidly accurate, as traces of either sulphide of antimony or of free sulphur may contribute to give too high results.

I give below the results obtained with four distinct specimens of sulphide of antimony:—

No. 1	contained	sulphide of arsenic	1·33	per cent.
2	,,	,,	·27	,,
3	,,	,,	1·16	,,
4	,,	,,	·25	,,

Thus it appears that No. 1 (that given to the calves) contained far more arsenic than did some other specimens, amounting to 5·84 grains of tersulphide in an avoirdupois ounce, being the dose which proved fatal.

The dose of black antimony may be considered unreasonably large, but I know of no grounds for supposing that the antimony produced the mischief. Large doses of this substance are constantly given to animals. Pereira speaks of doses of two to four ounces being given to horses, and of half an ounce taken by a man for several days without bad effect.

It appears, then, that the arsenic present was the cause of mischief. The dose of pure sulphide of arsenic has not been determined, and it has often been regarded as comparatively inert. This is doubtless correct as long as it retains its insoluble condition. *Medicina non agunt nisi soluta.* But, singularly enough, in selecting urine as a vehicle for dosing the calves, their owner was providing for the solution and prompt absorption of the sulphide of arsenic. When it was dissolved by the ammoniacal urine, its poisonous properties would probably be just as great as those of a similar quantity of arsenious acid in solution.

Gmelin gives a process for purifying commercial black antimony from arsenic by digesting the powder with twice its weight of aqueous ammonia for forty-eight hours with stirring, then filtering, and washing the product, which is almost entirely freed from arsenic. This method may become of some practical importance. One of the principal makers of emetic tartar informs me that the black antimony lately in the market has been much more contaminated by arsenic than was formerly the case, and that it causes much trouble with the mother-liquors.

Impurity may exist to a much greater extent than that now recorded. Thus, Gmelin quotes Serullas to show that all German and French black antimony, excepting that of Montluçon, contains from 1·6 to 5·0 per cent. of arsenic.

It is evident that those who may unwittingly vend the contaminated samples incur the risk of being held responsible for results.

So long as sulphide of arsenic is present, and its noxious or innocuous quality depends upon the nature of the fluids with which it meets in the stomach, there will be a very disagreeable uncertainty about the use of the crude drug.—*Pharmaceutical Journal.*

TECHNICAL CHEMISTRY.

On the Preparation of Nitrate by means of Carbonate of Potash and Nitrate of Soda, by M. GUIDO SCHNITZER.*

DURING the last few years, great quantities of nitrate of potash have been made by decomposing carbonate of potash by nitrate of soda. By mixing the solutions of these two salts, nitrate of potash is formed which crystallises; after several crystallisations, the mother liquors are evaporated, and carbonate of soda is obtained sufficiently pure for use in the arts. The author has adopted the following modifications of this process:—First, change carbonate of potash into caustic potash by means of lime; make one atom of potash act on one atom of nitrate of soda, and evaporate the liquid to 40° Baumé. This liquid, when cold, furnishes a first crystallisation of nitrate of potash. Repeat these evaporations and crystalisations several times. Drain, and wash the crystals of nitrate of potash in water acidulated with a little hydrochloric acid, and the caustic soda separated from the crystals is, according to the author, transformed into Seignette salt.† The chief advantage of this process is the larger quantity of nitrate of potash it produces, this salt being much less soluble in a solution of caustic soda than in a solution of carbonate of soda. Moreover, the concentration can be carried to a greater extent, because caustic soda is uncrystallisable.—*Répertoire de Chemie*, iv. 42.

* *Gewerblatt aus Wurtemberg*, 1861, p. 15.

† For large quantities of Seignette salt prepared in this way there would be no use. It would be better to pass a current of carbonic acid into the liquid to transform the soda into carbonate.—S. K.

On Peruvian Guano, by M. LIEBIG.

ALMOST all the guano now so extensively used in Europe is obtained from the New World. This natural manure, composed essentially of urate and oxalate of ammonia, phosphate and oxalate of lime, and a peculiar base, guanine, is, it is well known, the produce of certain sea birds.

M. Liebig, whose works have rendered so much service to agriculture, has recently published a Memoir, in the *Annalen der Chemie und Pharmacie*, vol. cxix. p. 11, 1861, which to us appears to throw great light on the cause of the fertilising properties of guano, and on the means of appreciating them by the aid of chemical analysis.

The manifest action of Peruvian guano on the soil has not hitherto, he says, been satisfactorily explained. The good effects of this manure are generally attributed to the large proportion of nitrogenised matters which it contains,—matters consisting chiefly of ammoniacal salts and uric acid. Many observations, however, have proved that a field manured with guano yielded an abundant crop, while the addition, to a part of the same soil, at the same time, and for the same crop, of a quantity of ammoniacal salts corresponding exactly, in richness with nitrogen, to the guano employed, had scarcely any effect on the crop.

If, in the first instance, guano owes its fertilising properties to the nitrogen it contains, it is difficult to comprehend why, in the second instance, the same quantity of nitrogen, added to the soil in its most active form, should have no influence on the crop. The cause of the energetic action of guano must then be sought in its other constituents. If from these we except uric acid, the influence of which on vegetation is almost completely unknown, there remain only earthy phosphates and alkalies, which, existing simultaneously with ammoniacal salts, might communicate to guano its active properties.

According to M. Liebig, various reasons prevent the acceptance of this conclusion. Phosphate of lime is, with ammoniacal salts, the predominating element of Peruvian guano, which contains 32 to 36 per cent. of it. A quantity of phosphate (powdered bones), four, six, or even eight times larger than that contained in guano, is very far from producing the effect of the natural manure; its fertilising action is frequently increased by the addition of ammoniacal salts, but the results fall far short

of those produced by guano containing phosphates in the same proportion. The principal difference to be remarked between these two manures consists in the length of time each takes to manifest its action,—a point which has hitherto remained unexplained. The influence of guano is apparent the first year, often even after a few weeks, and goes on diminishing year by year; the powdered bones, on the contrary, act feebly during the first season, but afterwards their effect increases.

These facts once established, we will adduce M. Liebig's experiments, which tend to prove that guano owes its rapid action to the presence of oxalic acid.

Various kinds of guano contain very varying quantities of oxalic acid,—another proof that the composition of guano is not constant. Certain analyses, not indeed sufficiently numerous to lead to a positive conclusion, seem to prove that the quantity of oxalic acid contained in a sample of guano is in inverse proportion to the weight of uric acid it contains; that is to say, that the guanos rich in uric acid are generally poor in oxalic acid.

If Peruvian guano is moistened with either cold or boiling water, and filtered immediately after, the liquid furnishes, by evaporation, abundant crystals of neutral oxalate of ammonia, the mother-water containing a certain quantity of phosphate and sulphate of ammonia.

If guano is moistened with cold water and left alone for some time, the results are very different. The proportion of oxalic acid contained in the solution goes on diminishing, and the filtered liquid contains phosphoric instead of oxalic acid. After remaining in contact for twenty-four hours, the quantity of phosphoric acid becomes so considerable that the filtered liquid, boiled with sulphate of magnesia, yields, without the addition of ammonia, an abundant crystalline precipitate of phosphate of magnesia and ammonio-magnesian phosphate.

It is easy to explain why phosphoric acid, under these circumstances, should become soluble; in fact, it is evident that the oxalate of ammonia, dissolved by the addition of water to the guano, is gradually transformed in presence of phosphate of lime, and that the result of the reciprocal action of these two salts is insoluble oxalate of lime and soluble phosphate of ammonia.

It will be seen from this that the phosphoric acid of guano is not dissolved, because the manure contains at the same time oxalic acid; for by distributing all the fixed bases of the guano between phosphoric acid, sulphuric acid, and chlorine, there remain for the phosphoric acid only two equivalents of lime and magnesia, forming with it a salt partially soluble in neutral salts of ammonia. The presence of oxalic in the aqueous solution of guano is a sufficient reason for the absence of lime.

The following fact seems to contradict the explanation given above:—Recently-precipitated phosphate of lime, with two or three equivalents of base, is scarcely at all modified by long contact with oxalate of ammonia; traces only of phosphoric acid are found in solution. But it must be observed that guano always contains a body which facilitates decomposition, sulphate of ammonia. This salt renders the phosphate of lime partly soluble, but it does not pass into the solution in this form, the lime being immediately precipitated by the oxalic acid, the action of the sulphate of ammonia being prolonged, and the decomposition of the phosphate of lime goes on. By adding a small quantity of sulphate of ammonia or a few drops of hydrochlorate of the same base to a mixture of oxalate of ammonia and phosphate of lime, the phosphate is very rapidly changed into oxalate.

In guano moistened with water the transformation of oxalate of ammonia to the state of phosphate goes on rapidly to a certain point, after which the action slackens, and the decomposition is incomplete even after eight days. A little oxalic acid always remains in the liquid, easily recognisable by the fact that the precipitate which it gives with a salt of lime does not entirely disappear in acetic acid. The persistence of the oxalic acid is perhaps due to the fact that the phosphate of lime, not yet decomposed, is masked in a thick coating of oxalate of lime, which considerably retards the action of the oxalate of ammonia.

By acidulating with sulphuric acid the water serving to moisten the guano, so as to render the mixture freely acid, the decomposition becomes so much accelerated that it is finished in a few hours. No trace of oxalic acid is then found in the liquid, but in its stead an equivalent quantity of phosphoric acid.

Acetic acid and water charged with carbonic acid act on guano in the same manner as sulphuric acid.

M. Liebig has obtained the following results from a specimen of guano remarkable for the small quantity of oxalic acid and the large quantity of uric acid (18 per cent.) it contained. Besides water, potash, soda, and ammonia, he found in the aqueous extract of 100 parts of guano:—

Phosphoric acid . .	2·857
Oxalic acid . .	4·202
Sulphuric acid . .	3·371

After having effected the transformation of the phosphate of lime by a little sulphuric acid, M. Liebig found that 4·2 per cent. of oxalic acid contained in this guano had been replaced by 3 per cent. of phosphoric acid; that is to say, that by this method about half of the whole of the phosphoric acid of the guano had become soluble.

In other kinds of guano the same method has rendered soluble a weight of phosphoric acid corresponding to 10 or 12 per cent. of the weight of the guano; in other words, the whole of the phosphoric acid contained in the guano.

M. Liebig devotes the latter part of his Memoir to examining the practical conclusions to be drawn from these facts. When, he says, a field manured with guano receives too little rain to moisten the manure mixed with the arable land, all conditions are united to favour the solution of a certain quantity of the phosphoric acid, combined with the lime, and, consequently, to increase the fertilising action of the ammonia. The guano then acts in the same manner as acid phosphate of lime.

Heavy and continuous rains, while washing the soil, disturb this decomposition; and it is to be desired, for the interest of science, that agriculturists would give some attention to the influence of guano, under these various circumstances, on the fertilisation of the soil.

It is hardly necessary to remark that the action of guano can be made certain—at least, so far as it depends on the reaction of oxalic acid on phosphate of lime—by moistening the manure before using it with very diluted sulphuric acid, and then letting it stand for twenty-four hours. The moistened mass should have an acid reaction.

The addition of water, to increase its weight, is the most common falsification of guano; the greatest inconvenience arising from this fraud is, that it favours the decomposition above mentioned. The evaporation of ammonia proceeding from the phosphate of ammonia, formed under the influence of water, explains the loss of nitrogen observed in guano which has been kept for any length of time.

It is now evident that the agricultural value of guano

has been but imperfectly appreciated, when the ammonia, phosphoric acid, and phosphate of lime which it contains, have been estimated without taking account of the oxalic acid, to which, as M. Liebig has shown, its fertilising action is chiefly due.

This account would be incomplete without a description of the simple process by which M. Liebig estimates the oxalic acid in guano. The following is an extract from his letter on the subject:—

"To estimate the oxalic acid, I boil the guano with nitric acid; I then wash it; afterwards I add hydrochloric acid to the residue, which dissolves the remaining oxalate of lime and phosphate, leaving the uric acid. I neutralise the acid liquid with ammonia, which precipitates the phosphate and oxalate; I then add acetic acid, which dissolves the phosphate of lime, and throw the oxalate on a filter, wash it, &c."

Guanos can thus, in a very little time, be tested before being used.—*Condensed from the Annalen der Chemie und Pharmacie*, cxix. 11.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

Weekly Evening Meeting, March 21, 1862.

The Rev. John Barlow, *M.A. F.R.S., Vice-President, in the Chair.*

A Lecture, by F. A. Abel, Esq., F.R.S., *on Some of the Causes, Effects, and Military Applications of Explosions.*

The contrast presented by lectures delivered on consecutive Friday evenings in this theatre is often very remarkable, and there certainly can be none greater than that which will be afforded by the eloquent discourse to which we all listened with such pleasure on last Friday, and the very matter-of-fact story which I am about to tell this evening. An imperfect sketch of the causes, effects, and some of the applications of one large class of explosions, is all that I have to offer to the members of the Royal Institution and their friends on this occasion; but, as one who is now rarely called upon to lecture, and whose time is almost exclusively devoted to pursuits of a purely technical character, I feel that I may claim some amount of indulgence on the part of my audience; and, for the very elementary character of much of my discourse, I trust that the desire which I have to render as clear as possible the description which I shall give of some of the military applications of explosions, will be a sufficient apology.

The phenomena which we are in the habit of classing under the term explosions, are all due, I need scarcely say, to a sudden and considerable expansion of matter. The successful effort, for example, of confined particles of air to escape from the bonds within which they have been compressed, and to re-assume the position which they originally occupied relatively to one another, is always accompanied by the production of sound, the violence of which is regulated by the extent and suddenness of the expansion and the amount of resistance to be overcome, and, consequently, by the violence with which the particles of air dash against and impart vibration to the surrounding atmosphere, or other particles of matter with which they come into collision; as is the case when I compress air within this bladder, until a point is reached at which the cohesive force, holding the particles of the bladder together, is overcome by the pressure exerted from within, and we have a sound produced. [This was illustrated by air being forced by a syringe into a small india-rubber balloon until the pellicle burst with a slight report.] What we call explosions may also be produced by a sudden or very rapid conversion of a solid or liquid into a gas or vapour,—by the sudden change of state (as we commonly call it) of matter, brought about by the action of heat, which is therefore one great—in fact, the most important—source of explosions.

Such explosions as are due solely to physical agencies, afford for consideration a number of points of interest; but it is impossible to treat the whole subject of explosions generally in one lecture. I must therefore ask you to allow me to confine myself to the consideration of those explosions which are brought about directly or indirectly by chemical agency.

When chemical action produces, or is followed by, an explosion, we know that the main cause of that explosive effect or result, of which I have just endeavoured to point out the general nature, is the development of heat consequent upon the disappearance of chemical activity; and we also know that the amount of heat—if I may use this term in speaking of such an agency as heat—corresponds to the energy of the chemical action; just in proportion, therefore, as we have chemical energy exhibited, we have heat developed. There is another cause to which we may refer the production of explosions, and that is the alteration in the state of matter resulting simply from chemical change. Solids may, as you all know, be, under these circumstances, converted into vapours or gases; such changes may be effected very suddenly, and quite independently of any heat developed; and the sudden expansion thus brought about would naturally produce the effect of an explosion. We may readily conceive that, if a small quantity of powder, such as the charge used in a small arm or rifle (weighing about seventy-five grains), were suddenly converted, independently of the action of heat, into a considerable volume of gas, an explosive effect would be produced. But, accompanying this change of state, we have, in the actual case of the explosion, very intense heat developed, resulting from the chemical transformation; and that heat, by its expansive effect, contributes far more to the production of the violent explosion than the mere alteration of state resulting from the chemical change. This may readily be rendered evident by comparing the volume of gas which, by theory, would be produced by ignition of the powder, taking into consideration the expansive action of the resulting heat, with the comparatively small volume which the gas would occupy at common temperatures. [The difference in volume of the charge of gunpowder and other gases produced under the above circumstances was illustrated by means of cubes.]

Now, there are several classes of chemical action by which explosions may be brought about. Firstly, we are acquainted with a few instances in which explosions result from chemical combination, as in the case of some elementary bodies which possess a tendency to enter into combination with great energy, and consequently with explosive violence. There are numerous instances of combination of a very energetic character, especially between compound bodies, but very few of them, indeed, produce explosive results, simply because the combination proceeds in comparatively a gradual manner. As I have said before, it just depends upon the rapidity of action we can establish between bodies,—upon the intensity of chemical affinity exhibited by bodies when they are brought together,—whether we produce a sufficiently sudden expansion of matter to bring about an explosion. In a case of feeble chemical action, such as that of an acid upon a weak base, heat is developed, but slowly and to comparatively a slight extent, because of the very gradual nature of the combination. Thus, if we dissolve this oxide of zinc, which is a weak base, in acid, the heat developed will only gradually melt the very fusible material with which this vessel is coated. This is an example of feeble chemical action. If we proceed a step further, taking, for example, such a substance as this

oxide of phosphorus, which possesses great chemical affinity for water, and we allow it to combine with a small quantity of that liquid, we observe that it will do so much more energetically than the oxide of zinc did with the acid, producing almost an explosive result. We have a proportion of the water suddenly converted into vapour by the heat developed, which is sufficiently intense to ignite a highly combustible material, as we find if we allow gun-cotton to come into contact with the oxide of phosphorus which we are combining with water. If two very active elementary bodies, such as bromine on the one hand, and potassium on the other, are brought together, we find that a still more violently rapid combination takes place. No substance existing naturally in the form of a vapour or gas is produced by their combination; it is simply here the intense heat, suddenly generated by bringing these two substances together, which suffices to produce a powerfully explosive result, by instantaneously generating a quantity of vapour. And, lastly, if we take a mixture of two gases, such as hydrogen and oxygen, or hydrogen and chlorine, and confine them as we have in this glass vessel, we know that the mere momentary contact of some portion of the mixture with a body raised to a high temperature, or the passage of an electric spark through it, produces instantaneous combination throughout the whole volume of gas. We will fire these mixed gases in this vessel by the passage of the electric spark through them. A violent explosion takes place, and the vessel is shattered into innumerable minute fragments, in consequence of the enormous force suddenly exerted by the intensely heated product of combination of the gases.

These are one or two instances in which combination produces explosion. We can produce a much greater variety of examples in which explosion is the result of the instantaneous or very rapid decomposition of a chemical compound. We are acquainted with several classes of compounds remarkably unstable in their character, these are to be found particularly among the bodies which we term organic; there are, however, a few inorganic compounds which are also remarkable for their instability; such are the combinations of hydrogen and nitrogen, with chlorine, bromine, and iodine. This, for instance, is the iodide of nitrogen, with which you are well acquainted: observe its great tendency to explode. [A small quantity was exploded by being touched with a piece of folded paper.] If it is sufficiently dry, a very slight touch will cause the explosion. It is not quite dry, or I might show that the explosion may also be produced simply by allowing the substance to fall upon water; but I have sufficiently attained my object, which was to show you the highly explosive nature of this substance. The combinations of mercury and of silver with carbon, nitrogen, and oxygen, which we know as the fulminates, are remarkably explosive in their character. When these fulminates are perfectly dry, a very slight blow or a very small amount of friction is sufficient to bring about their decomposition. Thus, here is a small quantity of fulminate of mercury; you will observe that a very slight application of heat to this is sufficient to cause it to undergo decomposition. It inflames with a dull sort of sound, which could, of course, be rendered more violent if the particles were confined. Here is some fulminate of silver, which is much more explosive in its character. We will take a much smaller portion of this than of the other fulminate, and place it upon the copper, and submit it to the action of the heat. [This was done.] You see it explodes much more readily and violently, and we perforate the copper instantly; while in the case of the mercury compound, the copper was hardly indented. As I have said, the explosive characters are exhibited by various organic compounds—bodies not of natural occurrence, but produced from non-explosive organic substances by the action of an acid remarkable for the amount of oxygen which it contains, and for the tendency which it has to impart that oxygen to other substances—nitric acid. This nitric acid, as many of you know, may be made to produce changes in organic substances, resulting in the oxidation of a proportion of hydrogen-atoms in the organic structure, and their removal in the form of water; a corresponding proportion of the partially de-oxidised acid (nitrous acid) passing into the space created by the abstraction of the hydrogen from the group; and thus we can produce, for example, from cotton, from cellulose, or lignine, highly explosive substances. Here is one of these bodies—the well-known gun-cotton [holding it to the spirit-lamp and so firing it.] If we confine a small quantity of it in a little vessel of this description (a small glass globe), and fire it, which we can readily do by applying this hot iron to the exterior, we shatter the vessel; so that you see that the substance which appeared but feeble in its explosive power when burnt in the open air, may readily be made to produce powerful effects. There is also an explosive substance produced by a similar action of nitric acid upon the sweet principle of manna, beet-root, parsnips, or onions, known as mannite. Again, we are enabled by the mere contact of nitric acid, in its most concentrated form, at a low temperature, with glycerine, to produce a substance of a highly explosive character known as nitroglycerine or glonoïne. It is only necessary to moisten a small portion of filtering-paper with a little of this substance, and then to expose this to a moderately violent blow, in order to show its explosive nature. [A piece of filtering-paper moistened with the compound of glycerine and acid was placed on an anvil and struck with a hammer.]

Here is an explosive substance belonging to this class, of very recent origin, a member of a most interesting family, one of the derivatives of that remarkable substance, aniline, to which we are indebted for those beautiful new colours, Mauve and Magenta, of whose history we shortly expect an interesting account from Dr. Hofmann. It is curious that this body, aniline, which has become of such importance in connection with arts and manufactures, should also exhibit what may be called "warlike tendencies." By the action of nitrous acid at a low temperature upon aniline, the explosive substance to which I refer is produced. This singular body rejoices, I am happy to say, in the comparatively simple name of *nitrate of diazobenzol;* compared with the names which are possessed by some of the members of this family, this is certainly not a hard one. [A small quantity was exploded on copper foil, which was shattered.] You see that it appears quite equal in its explosive power to fulminate of silver. I cannot help devoting a few moments to a comparison of its explosive properties with those of fulminate of silver. The silver compound explodes with a slight touch given by a hard substance; whereas this new compound can actually be rubbed between hard surfaces without exploding; at least, I have found it so on frequent occasions, although I have finally exploded it in that way. Some time is necessary, however, for producing, by friction, the requisite heat for its decomposition. The slightest touch, you observe, explodes fulminate of silver, and it does not admit of being rubbed. The *nitrate of diazobenzol*, when compared with the fulminate in this way, appears to be the less explosive substance; and yet, when directly exposed to heat, it is certainly the most sensitive of the two. If we expose the fulminate of silver in this little tin tube to the heat of boiling water, I think we shall find that it is not affected. [The experiment was performed with the result anticipated.] But if I heat, in the same manner, a little of this fulminate of aniline—if I may use the term—(although there are one or two compounds, also derivatives of aniline, still more recently discovered by Dr. Hofmann, which may claim the title,) if I similarly heat this *nitrate of diazobenzol*, you will find that it will undergo decomposition as soon as the tube has reached the temperature of the water. There we have a very

violent explosion; the tube is shattered, and has been thrown out of the water-bath by the force of the explosion. The fulminate of silver has not exploded, but we had better get rid of it. I shall be able to explode it by exposing it to this source of heat [the flame of the spirit lamp], for it explodes at more than double the temperature of boiling water, or about the temperature of melting tin, so that as soon as the tube begins to melt, the fulminate of silver will explode. [The explosion shortly ensued.] We see, therefore, that this fulminate of silver appears far less sensitive, when exposed direct to heat, and more sensitive when submitted to friction, than the derivative of aniline.

Having glanced at the nature of explosive chemical compounds, we will pass on to explosions which result from the reaction upon each other of substances in a state of mechanical mixture,—not chemically combined. We know that if we mix together some substances which differ widely in their chemical properties, we may, under favourable circumstances, bring about the ignition or explosion of the mixture. We have a number of substances which may be applied to the preparation of such mixtures. For instance, here are, on the one hand, substances all rich in oxygen, oxides of manganese and lead, and combinations of metals with nitric acid or the corresponding acid, chloric acid. These substances have a tendency to part with their oxygen, and, therefore, bodies which, on the other hand, are easily oxidisable, such as sulphur or phosphorus (which we can employ with comparative safety in the amorphous state), or the sulphides of arsenic and of antimony, when brought into a state of mixture with these sources of oxygen, will produce materials endowed with explosive properties. Thus, for example, we have here a mixture of binoxide of lead with sulphur, which ignites most readily. We will just apply heat to this for a very short time, and we shall have chemical action established. You see the mixture burns violently, the heat produced has actually broken the tube. Here we have a compound of a high oxide of manganese. We mix this together with iron in a very fine state of division. It is enabled to oxidise that iron very rapidly on the application of heat, and produces an effect bordering upon an explosion in its character. Here we have the particles of iron burning beautifully in the oxygen imparted to them from this oxide of manganese. Then, again, if we take the same substance, together with sulphur, and mix them in this mortar, we can do so in safety, but, on applying a small amount of pressure, the mixture will ignite. You see how beautifully it burns when I have mixed the substances with a sufficient degree of intimacy. And if we take these compounds, many of which are familiar to you, nitric and chloric acids, in combination with various metals, such as potassium, sodium, or barium, we impart to them explosive characters by simply mingling them with an oxidisable substance such as resin or sulphur. We have there chlorate of baryta mixed with a small quantity of resin. Observe how violently it burns, with a bright green-coloured flame. Here are mixtures of chlorate of potash with charcoal,—and in effecting their ignition, I wish to direct your attention to the circumstance that the two mixtures have different degrees of rapidity of burning, as this affords me an opportunity of pointing out to you the very great influence which may be exerted upon the properties of an explosive mixture, quite independent of the chemical properties of the substance, by the degree of intimacy with which its constituents are mixed together. The more perfect the mixture, the more rapid and complete will be the transformation suffered by the components, and, consequently, the more powerful will be the explosion. I need hardly observe that the characters of these mixtures are regulated also by the chemical and physical properties of their constituents; for instance, some bodies are more readily oxidisable than others, and partly, sometimes, because they are more easily convertible into vapour. A comparison of the facility with which sulphur and charcoal are oxidised, will furnish an illustration of this. Sulphur, as you know, is melted and vaporised at a comparatively low temperature, while charcoal is not at all similarly affected; and, therefore, though the latter possesses a most powerful tendency to combine with oxygen, sulphur may be readily oxidised or inflamed at a very much lower temperature. I bring forward this example because it bears particularly upon the subject of gunpowder, in which the high degree of inflammability of sulphur is turned to account to promote the very rapid oxidation of the charcoal. I have here, in an atmosphere of oxygen, the two substances, carbon and sulphur, in a fine state of division. I shall place over the mouth of this vessel which confines the oxygen, a piece of heated iron gauze, which will not be hot enough to ignite the charcoal when I puff it upwards so as to bring it into contact with the metal. But while that charcoal still remains suspended in the oxygen, I will throw up some sulphur, which will inflame directly it reaches the iron, and will ignite the particles of the charcoal, combustion being thus brought about almost instantaneously throughout the mixture of oxygen and carbon. [The oxygen was contained in a loosely-covered glass jar, into the lower end of which two tubes were fixed communicating with the receptacles containing the charcoal and the sulphur. These two substances were respectively blown, at an interval of about two seconds, to the top of the jar, and so thrown into contact with the hot piece of iron. No change was apparent when the charcoal was blown up, but immediately upon the arrival of the sulphur to the top, combustion ensued throughout the whole mass, producing for an instant a very dazzling body of light.]

The substances which furnish the oxygen in explosive mixtures exhibit equally important differences in their properties, which also assist us in regulating the characters of explosive mixtures. For the purpose of illustrating this, I beg to direct your attention to a very simple experiment with the two well-known substances, nitrate of potash and chlorate of potash. Here are mixtures of these two bodies with amorphous phosphorus (which we employ as a substance very easily oxidised, although its use requires very great care, as it is liable to oxidation in such mixtures on a very slight impulse). This is the mixture with nitrate of potash; you see, it simply burns brilliantly when heat is applied. And here is the phosphorus mixed with chlorate of potash, which ignites immediately with a violent explosion, and the copper on which it rested is shattered.

These few experiments may suffice to show you that these mixtures are susceptible of very great modification in their explosive characters. Not only are some of them decomposed with the greatest facility on exposure to the very slightest disturbing influence, but when once the decomposition of one small particle has occurred, it may proceed with uncontrollable rapidity throughout the whole mass.

A very slight examination into the effects of fulminate of mercury and of gunpowder, employed under the same circumstances, will illustrate the difference in the effects produced by substances which explode with different rapidity. Let me first compare the rate of combustion of those two substances. Here is a small train of fulminate of mercury, and here is a similar train of gunpowder. You observe that the flame travels much more rapidly along the train of fulminate than along the gunpowder. This difference will prepare you to believe that the effect of the two when confined in vessels must be different. Here are the fragments produced by the explosion, in a small shell, of 100 grains of fulminate of mercury; the number (amounting to one-seventh of the weight of the shell) were, however, so small that they could not be recovered after the explosion. Here are the fragments of a shell of the same size exploded by 765 grains of gunpowder. The difference between the size and number of the fragments in the two

instances is very striking. In the case of the fulminate of mercury the explosive effect is exerted almost instantaneously in all directions, and the shell is therefore shattered into a very large number of fragments, the force of the explosion being almost entirely spent upon the bursting of the shell; while in the case of gunpowder, the explosion being comparatively gradual in its nature, the force developed is only partly spent upon the fracture of the shell, and is still in course of development when this result is produced; hence, not only are the resulting fragments much fewer and larger, but a considerable projectile force is exerted upon them after their production, and they are consequently scattered to a much greater distance than those produced by the employment of fulminate of mercury.

When gunpowder is confined, by means of a shot or shell, in the barrel of a gun, the explosion of the first particles has the effect of overcoming the inertia of the projectile, and, the action proceeding gradually, as compared, for instance, to that of the fulminate of mercury, the shot is projected with comparatively small strain upon the gun; but in employing the fulminate of mercury, in a corresponding experiment, it would be found that the enormous force, which reaches its maximum suddenly, would, almost simultaneously with the first movement of the shell, also discover the weakest parts of the gun; in all probability, therefore, the cannon would be burst, while we should not project our shot or shell to any great distance. In quarrying, where it is desired to detach large masses of rock or stone, without producing any great destructive effect, we employ a very slow-burning powder, the explosion of which exerts a force like that obtained with a wedge; if we were to employ fulminate of mercury in this instance, the particles of rock in the immediate vicinity of the charge would become shattered by the sudden and violent explosion, but the desired result would not be produced. A very simple experiment with these Lilliputian cannon, made of sheet copper, will afford some kind of practical comparison between the effects of sudden and gradual explosion. Here we have a gun charged with ten grains of gunpowder, the charge being merely confined by means of a wad. The wad is projected, you observe, to a considerable distance, as I fire the gun, but no apparent destructive effect is produced upon the weapon. The amount of gas resulting from the explosion of the ten grains of gunpowder is represented by this cube. This second gun is loaded with a charge of two grains of fulminate of mercury; and this small cube represents approximately the gas produced by its explosion. You observe that the wad is projected to a little distance, but the gun is burst open along about half its length. And on firing this gun, loaded with only half a grain of fulminate of silver, we have the breech also torn open, and that part of the gun upon which the fulminate rested is perforated. These results will, I think, render evident to you the very great value of the gradual explosive effect of gunpowder.

The action of gunpowder, gradual though it appears to be when compared with the action of a fulminate, may, in particular conditions and under certain circumstances, be much too rapid. Recent investigations of the effects of gunpowder have shown that the power we possess of modifying its action so as to render it more gradual, is exceedingly important. As an illustration of this, I may state that in long guns, and in cannon of large calibre, the charge of the powder used for the projection of the shot has been clearly shown to be completely ignited before the projectile is moved to any great distance along the bore of the gun; hence we find that, whenever explosions do occur in guns, in consequence, for instance, of inferior casting or metal, or an excessive charge of powder, the fracture of the gun is almost always confined to the part reaching from the trunnions to the breech. The American Dahlgren gun, of which this is a model, exhibits this great thickness at the breech end; this form has been adopted to enable the weapon to resist the comparatively enormous strain exerted on that part by the heavy charges employed. Where cast-iron cannon are still used, it will always be especially necessary, if we employ a rapidly-burning powder, to make the gun comparatively very thick from the breech towards the trunnions; and the production of strong cast-iron guns of this form is attended with very considerable difficulties; but if we use a slower powder, we can employ a cannon of more uniform thickness, as the strain exerted by the exploding powder is distributed much more uniformly throughout the greater part, if not the whole, of the length of the gun. Again, in rifled guns, in which, in consequence of the accurate fit of the projectile, the friction between it and the bore of the gun is very great, in some of which also, as in Sir William Armstrong's gun, the projectile has to be expanded, by the explosion, into the grooves of the cannon, a gradually progressive action of the explosion is obviously of very great importance. In mortars and very short guns, on the other hand, where we have a very small space for the projection of the shell, it is necessary to employ a very rapidly-burning powder. It has been constantly observed, that in firing mortars with the description of powder used for cannon, a portion of the charge has been thrown out unexploded.

Now, a modification of the rapidity of combustion of gunpowder is very easily effected, and in a variety of ways, though there is only one really correct mode of proceeding. I will just point out to you by what different methods we can modify the explosion of powder. Firstly, we can do so by altering its composition. If you look at the composition of a number of specimens of gunpowders, as given upon this diagram, you will find that they differ considerably from each other in the proportions of their ingredients. By increasing the proportion of charcoal beyond that indicated by theory as the smallest quantity which will combine with the whole of the oxygen in the saltpetre, we decrease the rapidity of burning of the powder, simply because we give the saltpetre a larger amount of carbon to oxidise, which it will do less energetically; the oxidation will, therefore, take place less rapidly, and less heat will be developed. Then, again, we can modify the rate of combustion of the powder by altering the method of manufacture—that is, by rendering the mixture of ingredients more or less perfect. Here are two trains of gunpowder, the components of one of which are much more intimately mixed than those of the other; the difference in their rate of burning is very evident. And, lastly, we may modify the action of powder—and this is really the correct way of going to work—without interfering at all with the proportions of ingredients as indicated by theory, or with the intimacy of mixture essential to their perfect action; by simply modifying the state of division of the material; that is to say, by employing various sizes of grains of gunpowder, and also by submitting it to different degrees of compression. A comparison of the rate of burning of two or three different samples of powder of the same composition, but varying in size of grain, will show you that this modification in the rapidity of action of gunpowder may be effected with very great ease and nicety without interfering in any way with the ultimate amount of force exerted by the powder. Here we have ordinary small grain gunpowder, here the powder used generally as cannon powder, and here we have a much larger grained gunpowder; the fragments are, at least, four times as large as those of common cannon powder. Large grains, or rather lumps, such as even these (showing some as large as walnuts) have been tried, and, by employing them judiciously, it is found that they propel shot to an equal distance, and with even greater uniformity, than ordinary cannon powder. You see how leisurely these large grains burn. A good indication of their gradual combustion is afforded by noticing the marks or trails which they leave upon the dish in which we have burned them; this other,

in comparison, burns very quickly; and, lastly, you will see, when I light this fine-grain powder, that it burns with a rapidity almost like that of fulminate of mercury, when compared with this particular gunpowder itself.

Time will not permit me to do more than allude to the important and beautiful uses to which the power of controlling the rapidity of combustion of gunpowder and similar mixtures have been applied by the artillerist and pyrotechnist. By combining the application of uniform and accurately regulated pressure with modifications in the composition of gunpowder, and by confining the material within a case or receptacle, so that, when ignited, it can only burn in one direction, the valuable arrangements known as fuzes and time-fuzes are obtained, by which charges of powder in shells may be ignited at any period, within certain limits, determined upon previously to the loading of the cannon. By mechanical arrangements, which regulate the amount of compressed powder to be burnt before the flame reaches the charge in a shell, the time of explosion may be adjusted with great nicety, subject, however, to slight variations, which depend, as Dr. Frankland has recently shown us, upon fluctuations in the density of the atmosphere. The production of rockets, signals, and numerous pyrotechnic arrangements is based upon the principles applied in these fuzes.

Although the gradual action of gunpowder is, as we have seen, of the greatest importance in most applications, there are certain instances in which more rapidly combustible substances, or more rapidly explosive bodies, undoubtedly might be employed with advantage. This is particularly the case, for example, in mining operations, where it is mainly desired to produce great destructive effects by the explosion. This circumstance has frequently led to the trial, and even occasionally to the use, for a brief period, in actual practice, of bodies more readily and rapidly explosive than gunpowder. Only one explosive compound, gun-cotton, has been put to the practical test, but trials have been made of a variety of mixtures, in all of which chlorate of potash is employed instead of saltpetre, in admixture with very oxidisable substances. The preparation of any of these substances upon a large scale has, however, always been sooner or later attended by disastrous results, which in many instances have not been simply due to carelessness. Examples of these mixtures are Callow's Mining Powder, containing orpiment, or sulphide of arsenic, mixed with chlorate of potash, and German or white gunpowder, which consists of prussiate of potash, chlorate of potash, and sugar. These substances will explode readily by exposure to friction or to blows, as you observe when I strike a little of this upon an anvil. You will admit that the blow by which I am enabled to fire this white gunpowder is not very great. It was, indeed, not greater than it might frequently be accidentally exposed to in its manufacture.

We have some of these mixtures which are so prone to change as to be ignited instantaneously by contact with powerful chemical agents,—such as acids, for example. Chlorate of potash is most readily acted upon by sulphuric acid, which not only decomposes the salt, but also transforms the chloric acid into very unstable compounds, and the heat resulting from the chemical changes thus established by the acid in a small portion of the mixture of chlorate of potash with an oxidisable body, such as sugar, or sulphide of antimony, is sufficient to ignite it, and thus the whole is almost instantaneously exploded. Again, friction will ignite some of these mixtures very readily. Here is one of them mixed with a few particles of sand; I will try to ignite this by friction. [The mixture was rubbed, but did not at first explode.] It is no argument, that because, when we try to ignite this substance by friction, it does not happen to do so very readily, therefore it is one which may be safely dealt with. This substance may appear to you to be endowed with far more stable properties than I led you to believe. I should like to show you that it is by no means so. [The mixture was again rubbed, and then exploded.] We are acquainted with some substances much more easily acted upon by friction,—such as mixtures of chlorate of potash and amorphous phosphorus. Very slight friction in that case will produce an explosion; pressure alone is almost sufficient to effect it. You see, I have really had nothing more to do, to produce the explosion in this instance, than to press the mixture firmly. This mixture may, indeed, be ignited even by the prick of a pin. We have it here arranged in the ingenious manner in which Sir William Armstrong has recently applied it to important purposes. It is enclosed in shellac varnish in a small cavity of wood. I place this pin with its point resting upon the mixture, and if I can see the head, I will explode the composition by a very slight blow with this thin rod upon the pin's head. [The explosion was produced.]

We can readily understand how easily the manufacture of mixtures such as those we have just noticed may be attended by accident, if we devote one moment to the consideration of the ease with which accidents are constantly brought about in gunpowder works, even when the greatest precautions are apparently taken against them. Such accidents are generally involved in mystery, in consequence of the great amount of destruction resulting from them, and of the loss of life of any persons in or near the place of accident, who alone might be able to explain its cause; but, whenever it has been possible to investigate them successfully, they have almost invariably been found to result from a want of strict attention on the part of the workmen to the precautionary measures laid down for ensuring their own safety. A serious explosion recently occurred at the Government works at Waltham Abbey, (of the effects of which I have here a diagram,) which could be more clearly accounted for than is usual with such occurrences. In this case the gunpowder was about to be removed from the bed of one of the mills after it had been completely incorporated. I may remind you, that the ingredients, after having been mixed, are ground up and stirred together, or incorporated, under very heavy *runners* or millstones. The mill was not in motion in this instance, and the greater part of the gunpowder had been removed from the bed. You observe that there are several mills in one continuous building; each one surrounded on three sides by massive walls, the compartment enclosing each mill being so arranged that the roof and one side are capable of being blown away very easily indeed by the force of an explosion, so that, as the rush of gas can readily make its escape in one direction, there is less destruction resulting from an explosion than would be otherwise produced. After the removal of most of the composition from the mills, in the particular instance to which I refer, it was the business of the men engaged upon this operation to slightly lift the millstones (which they could do by means of a crow-bar), and to take the remaining powder, amounting, perhaps, to half-a-pound or so, upon which the runners rested. It is the practice to place a piece of leather upon that part of the mill-bed to which the end of the crow-bar would be applied, and upon which the millstone would be shifted, so that, in the case of the latter slipping when raised by the crow-bar, it might not come down immediately upon the hard bed and any particles of powder resting on it. This precaution had been neglected by the workmen, as it appeared afterwards, with very great impunity, and, on this occasion, an explosion occurred, evidently in consequence of this, and, therefore, of the ignition of some particles of gunpowder by accidental exposure to sudden and powerful friction. One man in the building was sadly burnt, and subsequently died, and others were badly injured. But the explosion was communicated from this one mill to others in the same building on the other side, in a manner which is worth a moment's notice. In the incorporation of gunpowder a small quantity of dust is always unavoidably produced, notwith-

standing that the mixture is kept constantly damp while under the mills; small particles of the powder therefore continually attach themselves to the walls, and although these are swept carefully from time to time, it is impossible to prevent small portions from remaining on them. It was imagined that the individual mills were so perfectly separate and isolated from each other by the plan of building, that an explosion from one could not communicate to the other, particularly as an arrangement existed whereby an explosion in one mill would instantly cause a mass of water to fall upon the powder in the other mills; but there was a small shaft running through the wall from one mill to another, by which this descent of water was ensured, and this shaft passed through very small openings in the walls, closed by tight little doors, so that there were only one or two little crevices communicating from one mill to the other. Well, these were sufficient to allow the explosion to pass from one mill to the others, and to bring about the explosion of the powder upon the mill-beds before the water could reach it. The powder-dust had formed a train upon the walls, and the flame of the first explosion, reaching this, was led to the openings just spoken of, and thus passed, as you may notice, to two mills on the one side, from which the charge had not yet been removed, and to one mill on the other.

Some years ago I had an opportunity of visiting, with some military friends, a powder manufactory in Belgium, where, undoubtedly, the greatest attention seemed to be devoted to the utter neglect of any precautions. (Laughter.) The localities in which all the various operations of the most dangerous character were conducted, instead of being separated in the ordinary way by thick walls or mounds of earth, were arranged one on the top of another. It happened to be a very wet day on which we inspected these works, and we were taken into the various rooms with our muddy and sandy boots. We recoiled with something like horror at the thought of going into the mill in that condition. However, we could but follow the proprietor, who told us that all was perfectly safe. About two years afterwards I went to the same neighbourhood, and once more visited the mills, or, rather, the spot where they had stood; for they had not long before sustained complete destruction. Although the manager firmly believed that they were destroyed, not by accident, but by *malveillance*, I think you will concur with me in the opinion that a total neglect of all precautions against the possible ignition of powder by friction must, sooner or later, have led to such a result.

But to return for a short time to the more explosive compounds and mixtures. Although their sensitive character does not admit of their application upon any large scale, they have a variety of very important uses, when employed in, comparatively speaking, very small quantities, and especially as affording means for the instantaneous ignition of gunpowder. I need only allude very briefly to a few of their applications in this direction. For example, we have the well-known percussion caps, charged with fulminate of mercury mixed with chlorate of potash, which has the double effect of retarding the explosion usefully, and of greatly increasing the volume of flame resulting from it. Then we have applications of the mixture of chlorate of potash and sulphide of antimony to the production of matches for firing cannon by percussion or friction, and to the ignition of signals by the aid of sulphuric acid. We have also arrangements based on the latter principle, such as were adopted by the Russians against our ships in the late war, where a glass vessel containing a small quantity of sulphuric acid was broken in the midst of some of the mixture just named, by the pressure of a rod which was forced against it when a ship came into contact with the submarine mine. We have the same kind of detonating composition applied in a variety of fuzes which ignite by percussion or by concussion. The sudden check which a shell, containing such a fuze, may receive in its flight, will be sufficient to ignite the mixture, placed upon some movable portion of the arrangement, by the blow to which it is subjected, thus causing the gunpowder in the shell to explode. We have one of these arrangements shown in this diagram; the concussion to which it would be exposed, when in a shell, upon the firing of the gun, would flatten a small leaden cap; this ball, coated with the mixture, being then free to move, would come into violent contact with some portion of this chamber of the fuze, as soon as the shell were checked in its flight, and the composition would be ignited, inflaming a little quick match, which, in turn, would ignite the charge of powder in the shell. Then we have, lastly, the ingenious application made by Sir William Armstrong of the phosphorus-composition to which I directed your attention just now. The fuze-arrangements to which I refer exhibit a degree of delicacy of action which we have never before attained in contrivances of this class; and you will easily understand this if you will bear in mind how very readily I was enabled to ignite a small amount of the phosphorus-mixture by the insertion of a pin into it. In the fuze-arrangement shown in this diagram, a needle, which is fixed into a plug of metal, is readily detached, by the concussion on the discharge of the gun, from the place in which it has been held secure, and it immediately flies with sufficient violence against the explosive mixture contained in this cavity, to cause its ignition. The resulting flame lights this quick-match, and the fire is thus led to the fuze-composition, the period during which this burns, before firing the charge in the shell, being regulated by an index-arrangement. We will just fire one of these fuzes; the concussion produced by striking it violently on its base will detach the needle; there, you see, the time-fuze is now burning, and presently the fire will reach the small charge of powder in the base of the fuze. Here we have another arrangement, more delicate still in its action, in which the fuze is set, but not fired, by the explosion of the gun. The concussion of the explosion breaks some thin wires which hold the needle in its position of safety, and the little hammer, to which the needle is attached, flies to the greatest distance from the detonating composition in the fuze. Directly this is checked in its course, even very slightly, the needle flies forward and ignites the composition. In the shell of paper fixed to the end of this stick I have one of these fuzes, and the wires are cut. I will try whether by simply allowing the little hammer containing the pin's point to fall with its own weight upon the composition, we cannot explode it. You see, directly I bring the shell in such a position that the hammer can fall suddenly, it is exploded. You will, I am sure, agree with me that these are interesting illustrations of the beautiful manner in which these highly explosive materials may be brought into use by the exercise of proper care and the application of mechanical ingenuity.

I must just briefly allude to one other application of explosions of this class, namely, to the ignition of gunpowder by means of the modifications of electricity known as electro-magnetism and magneto-electricity. The particular mixtures applied to this purpose possess other important properties besides being easily ignited. They must also possess the power of conducting electricity to some extent, in order that they may afford to feeble currents of electricity the requisite assistance for overcoming the amount of resistance offered to their passage by interruptions in the metallic circuit. Among the mixtures which have recently been applied in this direction, we have one so highly explosive, and possessing such good electric conducting properties, that its ignition is effected by the passage through it of very feeble electro-magnetic or magneto-electric currents. We have arranged a dozen fuzes in one circuit up in the air, and I will try to fire these by the current from this induction-coil machine. The resistance offered by the interruptions in the circuit is very great in

this instance, but the current is assisted across the spaces by the sensitive and conducting composition, which at the same time is raised to the temperature necessary for its ignition. To show you the very sensitive nature of the fuzes prepared with this mixture, we will fire them by the current induced from small permanent magnets, and we have here a little portable magneto-electric instrument contrived for military purposes by Mr. Wheatstone, which is ready for use at any time, and by which a number of mines may be fired at once through several miles of wire. The current is developed by turning this handle, and by a depression of this key we can at once fire the fuzes.

I cannot more fitly conclude this very imperfect sketch which I have given you of the application of explosions, than by just pointing out to you the variety of work which is performed by one single explosion of gunpowder, in the instance of an Armstrong gun loaded with a shell. Upon firing the gun, not only is the shell propelled, but, by being forced into a narrow portion of the rifled barrel, its external shape becomes moulded to the proper form to enable it to receive a rotatory motion before it leaves the gun. We have a third important operation performed by the same explosion. A small quantity of a lubricating material, enclosed in a little metal case, is placed between the powder and the projectile, and, when the gun is fired, this lubricating material is thrown against the sides of the barrel by the flattening of the little case, and thus the gun is kept from becoming foul. A fourth operation, performed by the explosion of the charge, is one of those which I have just described to you, namely, the ignition of the time-fuze by which the shell is exploded at a fixed period after its discharge from the gun, producing, by its explosion those great destructive effects of which you all have heard. And, lastly, as a fifth result of the one explosion, we have the fracture of the small wires of this percussion fuze, which is enclosed in the shell, so that in the event of its striking or coming into contact, however slight, with some object at any period short of the burning down of the time-fuze, it may be at once exploded. We have, therefore, five distinct operations performed by the explosion of a charge of gunpowder before the projectile leaves the gun. I will not ask you, at this late period of the evening, to follow the shell in its flight; but I think this remarkable instance of the variety of work which may be performed by a single explosion, affords one of the most interesting illustrations which we could possibly select, not only of the very great progress which has been made within the last few years in the application of explosions, but also of the comparatively great control which may be exercised over the operations of these destructive agents, —these wonderful examples of the concentration of latent force.

NOTICES OF PATENTS.

1407. *An Improved Composition for Building, to be Used in Substitution for Brick and Stone, and an Improved Method of Constructing Walls and Roofs for Houses, &c.* S. Standfast, Hackney, Middlesex. Dated June 4, 1861.

A composition is made of the following ingredients, which are to be thoroughly incorporated with water and moulded into shapes, so as to form blocks suitable for building purposes :—Cement or lime; sand, gravel, or burnt clay; cow-hair, vegetable fibre, and iron dust or borings are intimately mixed, with the addition of hoop iron or wire as strengthening materials; these are filled into the mould while moist, and allowed to set and harden. In forming slabs to be afterwards rendered available in the construction of walls and roofing, it is preferred to line the moulds with plaster or a finer sort of material than that specified above as forming the body or interior part of the improved structure; and for chimneys to employ common glazed earthenware pipes imbedded in a mass of this material.

The binding qualities of the oxide of iron produced in the process of rusting receive a very ingenious application in the present instance, and would be capable, no doubt, of even more extended employment. Bands of hoop-iron have long been made to serve a useful purpose as stays in brick-work, and must materially strengthen any structure in which they are used. There may be some fear, however, of their causing ferruginous discoloration when employed as a backing to ordinary plaster in walls, partitions, &c.

1410. *Treatment of Fatty and Oily Matters.* H. L. Buff, Osnabruch, Hanover. Dated June 4, 1861. (Not proceeded with.)

This invention consists in the treatment of oily and fatty matters for the purpose of obtaining solid fatty acids, and a secondary product which the inventor terms "artificial caoutchouc."

1411. *Obtaining Products from Sea-Weeds.* E. C. Stanford, Worthing, Sussex. Dated June 4, 1861.

For the purposes of this invention, in place of burning sea-weeds in the open air to obtain ashes therefrom as hitherto practised, the sea-weeds are subjected in close vessels to the action of destructive distillation, by which means not only are the ashes obtained in a more favourable condition for the separation of potash salts and iodine, but the volatile products which are usually lost in the process of burning are condensed and retained for further use. It is believed that the process of destructive distillation may be most conveniently carried on by the use of retorts or vessels heated externally, and connected with suitable condensers, but other well-known arrangements for destructive distillation, amongst other systems by the help of super-heated steam, may be employed. The residual ashes thus obtained, and the condensed matters, are then treated by the ordinary processes for separating and purifying the several products contained therein; and the gases evolved during the distillation may also be collected and employed for the purposes of light and heat.

By the employment of the admirable process suggested by Mr. Stanford, it will be possible to economise much ammonia and other nitrogenous and inflammable matters, which have hitherto been wasted in the preparation of kelp.

1611. *Treatment of Sea-Weeds.* J. S. Macardle, Galway. Dated June 22, 1861.

The sea-weed being freshly gathered on the coast, the first of the preliminary processes consists in washing it with warm water for the purpose of removing all saline matters, which greatly retard the decomposition of the weed in the next stage of operations, when by piling up the mass of washed sea-weed into a heap, a kind of fermentation sets in, accompanied by the usual effects of heating and great reduction in the bulk of the materials. The next step consists in charring the mass of organic matter by exposure to a gentle heat, and in recovering from the carbonised product the alkaline and saline matters therein contained. This is performed by the ordinary process of lixiviation, the last washings being employed in the extraction of the soluble matter from a second portion of ash; and the liquors so obtained are concentrated by evaporation until they yield crystals of the alkaline salts. In order to collect the ammonia, which exists to the amount of about one and a-half per cent in sea-weed, the inventor mixes the weed with lime, potash, or other caustic alkali, and subjects the mass to distillation. The alkali here employed need not be wasted, for it will be ultimately recovered with the inorganic salts at the end of the process. For the condensation of the liberated ammonia the patentee employs either sulphuric or hydrochloric acid.

This specification has much in common with the patent claim of prior date, No. 1411, last described. The inter-

mediate step of inducing the fermentation of the sea-weeds is likely to be serviceable in lessening the bulk of the matters afterwards to be distilled.

Grants of Provisional Protection for Six Months.

973. Henry Joseph Simlick, Baker's Arms Gardens, Wellington Row, Bethnal Green, London, "Improvements in the manufacture of vesuvians or cigar-lights.

1005. Thomas Cobley and James Wright, Bridge Street, Blackfriars, London, "Improvements in the method of, and the apparatus for, treating auriferous and argentiferous minerals and ores for the purpose of extracting and separating the gold and silver from the other metals, minerals, and substances combined therewith, also in the method of treating the various residues resulting therefrom, and in the utilisation, application, and use of the said residues when so treated."—Petitions recorded April 8, 1862.

1042. John Garnett, Windermere, Westmoreland, "Improvements in apparatus for washing photographic pictures."

3163. John Dale, Manchester, "Improvements in the manufacture of glue or size."—Petition recorded December 17, 1861.

742. William Gossage, Widnes, Lancashire, "Improvements in the manufacture of soda and potash."—Petition recorded March 17, 1862.

780. William Clark, Chancery Lane, London, "Improvements in the manufacture of soap."—A communication from Mr. Antoine Auguste Mollard, Boulevart St. Martin, Paris.—Petition recorded March 20, 1862.

851. Edward Henry Cradock Monckton, Thurloe Place, South Kensington, London, "Improvements in the manufacture of effervescing liquids."

857. Samuel Anderson Emery, Arundel Street, Coventry Street, Westminster, "Improvements in the manufacture of soap."

1030. Henry Deacon, Appleton House, Appleton, Lancashire, "Improvements in the manufacture of caustic soda."

2893. Pierre Andre, Jean François Pline-Faurie, and Joseph Pierre Richard, Bordeaux, France, "Improvements in machinery or apparatus for preparing or manufacturing fuel."—Petition recorded November 18, 1861.

35. Henry Davis Pochin, Salford, Lancashire, "Improvements in the manufacture of resin, soap, or size."—Petition recorded January 4, 1862.

235. William Clark, Chancery Lane, London, "Improvements in the disintegration and bleaching of textile materials for the manufacture of paper."—A communication from Jean Baptiste Michel Auguste Siry, Boulevart St. Martin, Paris.—Petition recorded January 29, 1862.

289. Thomas Mossom Meekins, LL.D., Chancery Lane, London, "The production of a projectile and explosive force to be used in instruments of war, for an electric-gas-gun and electric-gas-shell, for a method of using the recoil of weapons for the purpose of increasing the pressure of elastic fluids, for the production of a projectile force for a method of rapidly loading weapons at the breech, and of a motive force to be used in an electric-gas engine or other engines."—Petition recorded February 4, 1862.

349. William Clark, Chancery Lane, London, "Improvements in refining cast iron, wrought, and other malleable iron, and in the cementation of iron."—A communication from Messrs. Louis Joseph Frederic Marguerette and Alfred Lalouël de Sourdeval, Boulevart St. Martin, Paris.—Petitions recorded February 10, 1862.

471. William Henry Ross, Liverpool, "Improvements in the manufacture of sugar."—A communication from Edward Beanes, Havannah, Cuba.—Petitions recorded February 22, 1862.

578. Thomas Tillam, Church Street, Deptford Green, Kent, "An improved method of purifying gas."—Petition recorded March 3, 1862.

627. William Noy Wilkins, St. John's-wood, London, "Improvements in the manufacture of pigments for oil and water colours."

643. William John Bennett, Millbank-street, Westminster, "An improved solution or preparation to be used with Portland and other cements for the production of artificial stone, or for building purposes."

677. John Edwin Grisdale, Oxford Street, London, "Improvements in photographic cameras, and in the mode of fixing the lens therein."

720. Henry Young Darracott Scott, Brompton Barracks, near Chatham, Kent, "Improvements in the manufacture of cement."

722. John Avery, Mark Lane, London, "Improvements in purifying coal."—A communication from John Reed, New York, U.S.

724. James Robey, Hereford Road North, London, "Improvements in manufacturing and refining sugar, and in apparatus employed therein."

760. Richard Archibald Brooman, Fleet Street, London, "Improvements in the manufacture of barytes and barytic products, and the application of these substances in the manufacture of sugar and other uses."—A communication received from Alfred Delaune, Paris.—Petitions recorded March 18, 1862.

Notices to Proceed.

3032. John Lyon Field, Upper Marsh, Lambeth, Surrey, "Improvements in the manufacture of mould candles."

3082. John Fordred, Brighton, Sussex, "Improvements in treating linseed oil."—Petitions recorded 9th December, 1861.

158. Alfred Joseph Martin, High Street, Bow, Middlesex, "Improvements in the treatment of fusil oil, and for various applications of the same to useful purposes."—Petition recorded January 21, 1862.

742. William Gossage, Widnes, Lancashire, "Improvements in the manufacture of soda and potash."—Petition recorded March 17, 1862.

3163. John Dale, Manchester, "Improvements in the manufacture of glue or size."

3195. Vasco D'Almeida, Nottingham Street, Marylebone, London, "An improved mode of obtaining colouring matter applicable for dyeing skins, silk, wool, and other fibrous materials."—A communication from Edward Smith, Place Cruzquebrada, Lisbon.

2917. Francis Puls, Francis Terrace, Hackney Wick, London, "Improvements in treating fatty and oily matters."—Petitions received November 20, 1861.

2952. Jean Baptiste Hulard and Louis Guillaume Poupel, Paris, "An improved process for hardening stones and plaster of Paris, and making them impervious to water."

2964. Phineas Cowan, Barnes, Surrey, "A mode of utilising the waste heat of furnaces used in reburning animal charcoal."—Petitions recorded November 25, 1861.

2994. Michael Henry, Fleet Street, London, "Improvements in the manufacture of soap, and the preparation of materials for the purpose."—A communication from Louis Marie Théophile Riot, Boulevart St. Martin, Paris.

35. Henry Davis Pochin, Salford, Lancashire, "Improvements in the manufacture of rosin, soap, or size."—Petition recorded January 4, 1862.

Chemical Society.—Arrangements have been made for the delivery of a Lecture, on Thursday, June 5, "On Oxide of Ethylene," by M. A. Wurtz.

Royal Institution.—Tuesday, May 20, at Four o'clock, C. T. Newton, Esq., "On Ancient Art." Thursday, May 22, at Three o'clock, Dr. Lyon Playfair, C.B., "On Progress of the Chemical Arts." Friday, May 23, at Eight o'clock, Warrington W. Smyth, Esq., "On Coal as One of the Great Materials of British Industry." Saturday, May 24, at Three o'clock, Professor Anderson, "On Agricultural Chemistry."

CORRESPONDENCE.

Adulteration of Milk.

To the Editor of the CHEMICAL NEWS.

SIR,—Having seen a paper "On Adulteration of Milk in Dublin," in the CHEMICAL NEWS of May 3, I beg to point out some errors in the calculations made by the authors.

They state that the mean quantity of water added is 3·5 in 17 adulterated samples, and that the maximum quantity is 5·6 per cent; and they compare the Dublin milk favourably with that of London, which, according to the *Lancet*, contained in adulterated samples 10 to 50 per cent. of water. Now, the authors of this paper estimate the quantity of water added as being the difference between the quantity of water found by their analyses and the normal proportion in pure milk, viz., 87·35. Calculating in this way, it would be difficult to show how more than 12·65 of water could be added. It seems to me, that as the addition of an equal weight of water would reduce the fixed constituents by one-half, the true method of ascertaining the quantity of water added would be to say, as the normal proportion of fixed constituents is to 100 parts of pure milk, so is the quantity found by analysis to the quantity of milk in the sample. The difference between the result obtained and 100 will be the water.

We thus get the following results, considering the normal fixed constituents at 12·65 per cent., and adding the total organic matter and the ash in the samples analysed:—

	Milk.	Water.
Rathmines Road	79·01	20·99
Cullenswood Avenue	76·35	23·65
North Strand	82·19	17·81
Pill Lane	84·98	15·02
Townsend Street	55·95	44·05
Frances Street	64·53	35·47
Brabazon Street	75·86	24·14
N. King Street	72·77	27·23
Barrack Street	61·67	38·33
Charles Street	65·61	34·39
Ranelagh	76·28	23·72
Harrington Street	81·12	18·88
Great Britain Street	80·10	19·90
Hanover Quay	57·93	42·07
Lower Kevin Street	73·10	26·84
Bride Street	67·59	32·41
Church Street	64·23	35·77

Giving a mean of 28·27 per cent. of water added—a very different result from that given, namely, 3·5 per cent.

Probably the quantity of water added is less than that given above, as in several of the samples the deficiency in butter shows that the milk had been skimmed,—a fraud not alluded to by the authors. The total fixed constituents would thus be diminished, as well as by the addition of water.—I am, &c. EDWARD DAVIES, F.C.S.

Chemical Works, Weston, near Runcorn.

Sulphate of Copper.

To the Editor of the CHEMICAL NEWS.

SIR,—We beg to call your attention to an error affecting ourselves in your article of the 3rd inst. "On Chemical Products in the International Exhibition," in which you notice our crystals of sulphate of copper, writing our name "*Buck and Co.*" Will you kindly correct this, and at the same time allow us to state that the crystals exhibited were not made expressly for the Exhibition, but are simply a sample of our ordinary make by an improved patent process, from which crystals are produced in a perfect and, as you say, "gigantic" form.—We are, &c.

JOHN T. BOUCK AND CO.

Chemical Works, Newton Heath, Manchester.

MISCELLANEOUS.

INTERNATIONAL EXHIBITION OF 1862.

LIST OF JURORS IN CLASSES AND SECTIONS.

CLASS I.—MINING, QUARRYING, METALLURGY, AND MINERAL PRODUCTS.

1. Samuel Blackwell, F.G.S., Dudley, mining engineer; 2. J. A. C. das Neves Cabral, Portugal, Inspector of Mines; 3. Charles Combes, France, Member of the Institute, Inspector-General, and Director of the School of Mines; 4. Devaux, Belgium, Member of the Department of Science of the Royal Academy of Belgium, Inspector-General of Mines; 5. Lieut.-General Alex. Gerngross, Russia, Director of Mining Department; 6. Sir W. Logan, Canada, Director of the Geological Survey of Canada; 7. Francisco Luxan, Spain, senator; 8. Sir Roderick Murchison, F.R.S., F.G.S., Chairman, London, Director-General of the Geological Survey and of the Government School of Mines, 16, Belgrave Square, S.W.; 9. C. Overweg, Zollverein, landowner, Letmathe; 10. J. Percy, M.D., F.R.S., F.G.S., London, Professor of Metallurgy to the Government School of Mines, 2, Craven Hill, W.; 11. Arcangelo Scacchi, Italy, senator, Professor of Mineralogy; 12. Warington W. Smyth, M.A., F.R.S., F.G.S., London, Professor of Mining to the Government School of Mines, 27, Victoria Street, S.W.; 13. Thomas Sopwith, F.R.S., F.G.S., Newcastle, mining engineer, 43, Cleveland Square, W.; 14. K. Styffe, Sweden, Director of the Royal Polytechnic Institution, Stockholm; 15. Peter Tunner, Austria, Director of the Imperial Mining School in Leoben; 16. H. Hussey Vivian, M.P., F.G.S., Swansea, mine owner, 5, Upper Belgrave Street, S.W.; 17. Nicholas Wood, F.G.S., Newcastle, mining engineer.

CLASS II.—CHEMICAL SUBSTANCES AND PRODUCTS AND PHARMACEUTICAL PROCESSES.

SECTION A.—*Chemical Products.*

1. Frederick Anthon, Chem. D., Austria, Professor of Chemistry, Prague; 2. Balard, *Chairman*, France, Professor of the College of France and of the Faculty of Science; 3. E. H. Von Baumhauer, M.D., Netherlands, Professor of Chemistry in the University of Amsterdam, and Member of the Academy, 56, Brompton Crescent, S.W.; 4. A Bernays, Ph.D., India, Professor of Chemistry, St. Thomas's Hospital; 5. Chandelon, Belgium, Professor of Chemistry, University of Liége, Member of the Royal Academy of Medicine; 6. E. Frankland, Ph.D., F.R.S., Foreign Secretary to the Chemical Society, 42, Park Road, St. John's Park, Haverstock Hill, N.W.; 7. Professor G. Forchammer, Denmark, Secretary to the Royal Society of Science, Copenhagen; 8. Wm. Gossage, Warrington, chemical manufacturer, 2, Oxford Court, Cannon Street, E.C.; 9. T. Graham, F.R.S., London Master of the Mint, Vice-President of the Chemical Society, 4, Gordon Square, W.C.; 10. A. W. Hofmann, London, F.R.S., Ph.D., President of the Chemical Society; Professor of Chemistry, Government School of Mines, 9, Fitzroy Square, W.; 11. N. Kunheim, Ph.D., Zollverein, manufacturer, Berlin; 12. A. V. Lourengo, Portugal, Professor of Chemistry at the Polytechnic of Lisbon; 13. Dr. A. Müller, Sweden, Professor of Chemistry at the Royal Agricultural Academy, Stockholm; 14. Raffaele, Piria, Italy, Member of the Italian Parliament, late Minister of Public Instruction, Naples, Professor of Chemistry; 15. James Young, F.R.S.E., F.C.S., Edinburgh, chemical manufacturer, 25, South Street, Thurloe Square.

SECTION B.—*Medical and Pharmaceutical Products and Processes.*

1. Dr. Wurtz, France, Professor of the Faculty of Medicine; 2. Von Fehling, M.D., Zollvercin, Professor of Chemistry at Stuttgart; 3. Daniel Hanbury, F.L.S.,

London, pharmaceutical chemist, 1, Plough Court, Lombard Street, E.C.; 4. Salvatore de Luca, Italy, Professor of Chemistry; 5. T. N. R. Morson, F.L.S., London, pharmaceutical chemist, 38, Queen's Square, Bloomsbury, W.C.; 6. J. M. Neligan, M.D., Dublin, Great Western Hotel, W.; 7. Theos. Redwood, M.D., London, Secretary to the Chemical Society, and Professor of Pharmacy to the Pharmaceutical Society, 19, Montague Street, Russell Square, W.C.; 8. A Schroetter, Ph.D., Austria, General Secretary of the Imperial Academy of Science, Professor of Chemistry, Vienna; 9. Robert Warington, F.C.S., London, Vice-President of the Chemical Society, Apothecaries' Hall, E.C.

Class IV.—Animal and Vegetable Substances used in Manufactures.

Section A.—*Oils, Fats, and Wax, and their Products.*

1. J. Ben Heath, Italy, Italian Consul General; 2. P. J. Kirchhoff, M.D., Netherlands, Professor of Chemistry in the University of Gröningen; 3. S. Marcoran, Ionian Islands; 4. Emmanuel Mavrogordato, Greece, merchant, 56, Westbourne Terrace; 5. T. J. Miller, M.P., London, spermaceti manufacturer, Dorset Wharf, Millbank, S.W.; 6. W. A. Miller, F.R.S., London, Professor of Chemistry, King's College, London, King's College, W.C.; 7. A. Payen, France, Member of the Institute, Professor to the Conservatory of Arts and Manufactures, and Professor to the Central School of Arts and Manufactures; 8. Emil Seybel, Austria, Member of the Chamber of Commerce, Vienna; 9. Stas, Belgium, Member of the Department of Science of the Royal Academy of Belgium; 10. Dr. Stein, Zollverein, Professor, Dresden; 11. T. Thompson, M.D., India, Superintendent of the Botanical Gardens, Calcutta, 5, York Gate, Regent's Park, N. W.; 12. W. W. Williams, London, soap maker, 31, Westbourne Park, W.; 13. George Wilson, F.R.S., London, manager of Price's Patent Candle Works, Wandsworth Common, S.

Section B.—*Other Animal Substances used in Manufacture.*

1. Capt. C. Bagot, South Australia, 8, Inverness Terrace, W.; 2. Bella, France, Director of the Agricultural Institution of Grignon; 3. Samuel Birchall, New South Wales, wool merchant, Gresham Club, E.C.; 4. George Busk, F.R.S., London, Secretary of the Linnean Society, 15, Harley Street, W.; 5. Robert Czilchert, Austria, landowner; 6. Sir Frederick J. Halliday, K.C.B., India, late Lieut.-Governor of Bengal, 28, Cleveland Square, W.; 7. J. G. Homere, Greece, merchant, 4, The Terrace, Kensington Gardens Square, W.; 8. J. Jowitt, Queensland, wool merchant (James Scott and Co.), 11. King William Street, E.C.; 9. Antonio Marchetti, Italy, Member of the Italian Parliament; 10. J. J. Mechi, London, Alderman, 4, Leadenhall Street, E.C.; 11. P. L. Sclater, F.R.S., London, Secretary of the Zoological Society, 11, Hanover Square, W.; 12. L. Scholler, Zollverein, Privy Councillor of Commerce, Duren; 13. J. Stebut, Russia, Professor of Agriculture in the Agricultural College, Gorigoretsk, 58, Brompton Square, S. W.

Section C.—*Vegetable Substances used in Manufactures, &c.*

1. T. Archer, F.R.S.E., Edinburgh, Director of Industrial Museum, Edinburgh; 2. Barral, France, Member of the Imperial Society of Agriculture; 3. Robert Fauntleroy, London, hardwood merchant, 99, Bunhill Row, Finsbury, E.C.; 4. J. D. Hooker, M.D., F.R.S., V.P.L.S., F.G.S., London, Director of the Royal Botanic Gardens, Royal Gardens, Kew, S.W.; 5. J. B. Hurlbert, LL.D., Canada; 6. Sir Robert Kane, F.R.S., M.R.I.A., Dublin, Director of Museum of Irish Industry; 7. J. Miers, F.R.S., F.L.S., Brazil, Temple Lodge, Hammersmith, W.; 8. Filippo Parlatore, Italy, Professor of Botany, Royal Museum of Natural History, Florence; 9. W. Henry Peat, South American Republics, 11, Mincing Lane, E.C.; 10. George Peterson, Russia, Member of the Scientific Committee of Crown Lands, and of the Council of Manufactures, 56, Brompton Square, S.W.: 11. R. Riddell, India, late uperintending surgeon, Nizam's army, Deccan, , The Grove, Clapham, S.; 12. W. W. Saunders, F.R.S., Tasmania, Vice-President of the Linnean Society; 13. Chev. de Schwarz, Austria, Chairman, Imperial Councillor, Director of the Austrian Consulate General, Paris, 6, Onslow Crescent, S.W.; 14. M. A. Sevastopoulo, Greece, merchant, 18, Leinster Gardens, W.; 15. Dr. Thiel, Zollverein.

Section D.—*Perfumery.*

1. E. Moll, France, Professor at the Conservatory of Arts and Manufactures; 2. W. Odling, M.D., F.R.S., London, Professor of Practical Chemistry, Guy's Hospital, Guy's Hospital, S.E.; 3. Sept. Piesse, London, distiller of perfumes, 2, New Bond Street, W.; 4. Eugene Rimmel, London, perfumer, 96, Strand, W.

Class XIII. — Philosophical Instruments and Processes depending on their Use.

1. Sir David Brewster, K.H., F.R.S., Edinburgh Principal of Edinburgh University; 2. Charles Brooke, F.R.S., London, Surgeon to Westminster Hospital, 16, Fitzroy Square, W.; 3. Dr. Dove, Chairman, Zollverein, Professor of Natural Philosophy and Principal of the Academy of Sciences, Berlin; 4. J. P. Gassiott, F.R.S., London, wine merchant, Clapham Common, S.; 5. James Glaisher, F.R.S., London, Superintendent of the Meteorological and Magnetical Department, Greenwich Observatory, 1, Dartmouth Place, Blackheath, S.E.; 6. Colonel Sir H. James, R.E., F.R.S., Southampton, Superintendent of Ordnance Survey; 7. G. Karsten, Denmark, Professor, Kiel; 8. Edouard, Kraft, Austria, Member of the Council of Civil Engineers, Vienna; 9. Mathieu, France, Member of the Institute, and of the Bureau of Longitude, Examiner at the Polytechnic; 10. Carlo Matteucci, Italy, Senator; 11. Major-General Sabine, R.A., F.R.S., London, President of the Royal Society, 13, Ashley Place, Westminster, S.W.; 12. Wm. Thomson, F.R.S., LL.D., Professor of Natural Philosophy, University of Glasgow; 13. C. Wheatstone, F.R.S., London, Professor of Experimental Philosophy, King's College, 7, Chester Terrace, Regent's Park, N.W.

Class XIV.—Photography and Photographic Apparatus.

1. H. W. Diamond, M.D., London, Twickenham House, Twickenham, S.W.; 2. A. F. J. Claudet, F.R.S., London, photographer, 11, Gloucester Road, Regent's Park, N.W.; 3. Baron Gross, Chairman, France, Senator; 4. Lord Henry Lennox, M.P., London, 51, Portland Place, W.; 5. C. T. Thompson, London, official photographer, Science and Art Department, South Kensington Museum, W.; 6. J. Tyndall, Ph.D., F R.S., London, Professor of Physics, Royal Institution, Albemarle-street, W.

Class XXIX.—Educational Works and Appliances.

Section D.—*Specimens and Illustrations of Natural History and Physical Science.*

1. Cloquet, France, Member of the Institute, Professor of Medicine; 2. Rev. B. M. Cowie, B.D. London, one of Her Majesty's Inspectors of Schools, 62, Queen's Gardens, W.; 3. J. E. Gray, Ph.D., F.R.S., Keeper of Zoological Collection, British Museum, W.C.; 4. N. S. Maskelyne, London, Keeper of Mineralogy, British Museum, W.C.

HOUSE OF LORDS.

Friday, May 9.

NOXIOUS VAPOURS.

The Earl of Derby, in moving for the appointment of a Select Committee to inquire into the injury resulting from noxious vapours evolved from certain manufacturing processes, and into the state of the law relating thereto, said, in the absence of any more exciting topics of discussion, he hoped he might be excused for bringing before their Lordships, as shortly as he could, a subject certainly so far removed from the range of party, or what was commonly called politics, that his only fear was that he should not be able to attract their Lordships' attention to it in a degree

commensurate with the real importance of the question. Some time back their Lordships had conferred a great advantage upon the metropolis by passing a stringent measure against what was called the Smoke Nuisance. Their Lordships had also introduced a stringent measure with regard to polluting rivers so as to affect the produce of salmon. But this could not be placed in competition with matters which affect health, or the productive power of land. With regard to the poisoning of the air, and the streams and rivers of the country, the noble Earl stated he had been informed by Dr. Lyon Playfair—than whom no more eminent name need be mentioned—that, on being called on to investigate a case where the water was alleged to have been poisoned by the refuse which had been turned into it from a manufactory where arsenic and lead were largely employed, he had found that though the water did not contain a sufficient quantity of poison to be injurious to health, yet, upon an analysis of one pound of mud, there were from ten to twelve grains of arsenic. The noble Earl next referred to the increase of alkali works, such as alum, soda, potash, and pearlash, and remarked how largely they entered into various manufactures. In Newcastle-upon-Tyne there is one of these manufactories which employs not less than 1000 persons, and covers under one roof sixteen acres of land. Those of their Lordships who had travelled by the London and North-Western Railway probably recollected near Warrington a most beautiful specimen of brickwork, in the shape of a column 131 yards in height. It was erected by Mr. Muspratt in order to meet the complaint that was made of the injury caused by his works. However, the great height of the tower only had the effect of carrying the vapours further off. The manufacture of soda was carried on by the decomposition of common salt, by means of sulphuric acid; and in most establishments the manufacture of sulphuric acid was also carried on. That was produced by the combustion of nitre and sulphur, and resulted in the most offensive and most noxious vapours. Fortunately, however, it happened that it was the interest of the manufacturers to condense the whole of those vapours, and most of the works in which both soda and sulphuric acid were manufactured, caused no inconvenience in the neighbourhood. Unfortunately, it was not sufficiently the interest of manufacturers to condense the muriatic acid gas caused by the manufacture of soda. The works of Mr. Muspratt were begun in 1831 or 1832. For some years there were great complaints in the neighbourhood of the injury done to crops, fences, and trees; and at last the evil became intolerable. In 1846, the late Mr. Lee, the proprietor of a large proportion of the land in the neighbourhood, obtained a considerable amount of damages from Mr. Muspratt, and another gentleman obtained no less than 1300*l.* damages. The manufacture was nevertheless continued; but in 1851, Mr. Lee, finding that the timber on his estate was either killed or greatly damaged, brought fresh actions against Mr. Muspratt, which were compromised by the latter paying 2000*l.* and costs, and giving a promise, which was punctually performed, to pull down and destroy the whole of his works. There then sprung up at St. Helen's a considerable number of other works, against which, in the same manner, the neighbouring proprietors were compelled to bring their actions. In 1839, Sir John Gerard recovered 1000*l.* damages; in 1846, 2000*l.*; and in 1852, he brought an action for more than 4000*l.*, which was suspended by reason of his dangerous illness and subsequent death. In the meantime, additional manufactories had sprung up, and the difficulty of tracing injury home to any particular person was proportionately increased. The consequence of these manufactures was that for four or five miles round St. Helen's, in the direction in which the wind usually blew, there was scarcely a living tree. (Hear.) The landed property had deteriorated to the extent of 200,000*l.* His (the Earl of Derby's) house was five or six miles from St. Helen's, and on the side of his park next that town there had of late been a very considerable decay in the older timber, and to a certain extent he attributed it to the poisonous fumes of the manufactures in question. Now, so far as most of such manufactures were concerned, a perfect remedy might be applied, as the vapours might be condensed in the simplest possible manner. Water would absorb 480 times its own bulk of muriatic acid gas, and consequently a constant flow of water down the chimney would absorb the gas as it rose. Some of the manufacturers already took this precaution, but very many neglected to do so, and in many cases where the precaution was taken the water was allowed to run into brooks, which thereby became poisoned. The water, however, might not only be rendered innocuous, but might be turned into a source of absolute profit. The present law did not afford a sufficient remedy. For the purposes of prevention, the law of England was absolutely silent, except as to a very partial provision made by the Public Health Act, which gave a certain power of preventing the erection of nuisances such as he had referred to. There were, therefore, only two courses open—one to individuals, and the other to the public—for remedying the great injury done by these works, viz., an indictment for a nuisance, and an action for damages. In bringing an action for damages one should prove the injury, and the extent of the injury, and also that it was done by the specific work; and although some persons might succeed in getting large damages, still they did not succeed in putting down the nuisance. The noble Earl concluded by saying it was not intended to restrict the operations of trade; the aim of his motion was to inquire how far legislation might be introduced which, without injury to manufactures, might protect the community against the noxious vapours arising from them. (Cheers.) After some discussion the motion, as originally framed, was agreed to.

Improved Lucifer Matches.—We have recently had occasion to notice some of the many patented improvements which have of late been introduced into the manufacture of lucifer matches. One of the most novel and important inventions under this head is the "patent special safety match" of Messrs. Bryant and May, of Fairfield Works, Bow. The protection afforded by the use of this match is based upon the circumstance that it will only ignite by being rubbed upon the prepared surface of the box; no ordinary kind of friction being capable of inflaming the combustible materials with which the wooden splints are tipped. The match does not itself contain any phosphorus, but is coated merely with oxydising substances, such as chlorate of potash in conjunction with binoxide of lead or manganese, and the ingredients are in this manner so divided that it is necessary to employ the special friction surface, prepared with amorphous phosphorus, in order to secure the inflammation of the match. The security against accidental conflagration is thereby reduced to a minimum; the splints have, indeed, so little of the dangerous character of the ordinary match that the makers announce that their manufacture enjoys the exclusive privilege of being sanctioned and admitted within the building of the International Exhibition. The matches were at first coated with sulphur in the usual manner; but this practice appears to have been discarded almost immediately in favour of the employment of some kind of fatty matter for impregnating the wood. No phosphorus being employed in the match composition, they are, of course, quite destitute of the unpleasant odour and poisonous character of this substance. The dangerous practice of carrying loose matches about the house, and the common habit among servants of striking them upon the wall to the disfigurement of the paper-hangings, will be altogether avoided by the introduction of Bryant and May's patent special safety match.

THE CHEMICAL NEWS.

VOL. V. No. 129.—May 24, 1862.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Occurrence of Berberine in Xanthorrhiza Apiifolia, by J. DYSON PERRINS, *F.C.S.*

HAVING lately observed the presence of berberine in *Hydrastis canadensis*, it appeared to be worth while to ascertain whether this alkaloid also occurred in *Xanthorrhiza apiifolia*, another North American plant, which belongs to the same natural order, Ranunculaceæ. Professor Bentley having announced his early intention of describing the botanical characteristics, properties, uses, &c., of *Xanthorrhiza apiifolia*, any account here is rendered unnecessary. Suffice it to say, that the root is of a fine yellow colour, and popularly called in the United States, "Yellow root." I was indebted to the courtesy of Daniel Hanbury, Esq., for a specimen of this root; but finding a large quantity would be required for my purpose, and failing to meet with it in this country, I obtained a supply from a correspondent in New York.

The mode of treatment adopted was extremely simple, viz.—Exhausting the bruised root with boiling water, and evaporating the liquid to a soft extract, which was digested in boiling alcohol so long as anything was dissolved; the greater part of the alcohol was distilled off, and a little dilute nitric acid added to the residue; after standing for a day or two, fine crystals had formed, which were purified from resin and other matter by repeated crystallisations from water, with the cautious use of animal charcoal, always adding a few drops of dilute nitric acid to the liquid, which I prefer to hydrochloric acid; the pure salt finally consisted of fine acicular yellow crystals.

All the known reactions of berberine were manifested; but in order to place the matter beyond dispute, the following analyses were made. The nitrate, as above described, was crystallised from water to get rid of any possible traces of free acid, dried at a temperature of 212° Fahr., and burned with chromate of lead:—

No. 1. 5·002 grains gave 11·032 grains carbonic acid, and 2·058 grains water.

No. 2. 5·213 grains gave 11·504 grains carbonic acid, and 2·149 grains water.

	No. 1.	No 2.	Fleitmann.*
Carbon, per cent. .	60·15	60·18	60·15
Hydrogen	4·57	4·58	4·75

Some of the platinum salt was also prepared, dried at 212° Fahr., and burned with lead chromate.

6·132 grains gave 9·981 grains carbonic acid, and 1·882 grains water.

10·243 grains burned with soda lime, gave 4·300 grains ammonio-chloride of platinum. 8·516 grains gave, on ignition, 1·544 grains platinum, which figures correspond with the following per-centages:—

		Fleitmann.*
Carbon . .	44·39	44·44
Hydrogen . .	3·41	3·42
Nitrogen . .	2.59	2·52
Platinum . .	18·13	18·11

These results prove satisfactorily that berberine occurs in *Xanthorrhiza apiifolia*, thus adding another source for this very beautiful alkaloid. The amount present is not large; seven pounds avoirdupois yielded only fifty-two grains of the pure nitrate.—*Pharmaceutical Journal.*

* Fleitmann on "Berberine," *Chemical Gazette*, 1847, vol. v. p. 129, &c.

On the Estimation of Phosphoric Acid in Presence of Oxide of Iron and Earthy Bases, by M. A. GIRARD.

THE estimation of phosphoric acid, so easy when the acid is simply united to alkaline or metallic bases, presents almost insurmountable difficulties when, in addition, oxide of iron, alumina, lime, and magnesia are present. Chemists who have long occupied themselves with this question, have tried to resolve it by two different methods. Some, as Berzelius, MM. Rose, Otto, Fresenius, and Wackenroder, propose to precipitate the phosphoric acid, in the state of insoluble phosphates, in alkaline, and especially ammoniacal solutions; but experience has shown that, in consequence of the great analogy between the properties of phosphates and the corresponding oxides, none of these processes could furnish absolutely exact results. Others, on the contrary, by taking advantage of the insolubility of certain phosphates in nitric acid, have sought to collect on one side all the phosphoric acid, and on the other the nitric liquid containing all the bases. Such are the processes founded on the employment of phospho-molybdate of ammonia (Schonnenschein), phosphate of tin (Reynoso), and phosphate of bismuth (Chancel). Though more exact than the preceding, there are, nevertheless, two drawbacks to these processes: firstly, they require long and complicated manipulation; secondly, part of the peroxide of iron, and even of the alumina, are precipitated with the insoluble phosphate, thus increasing its weight. In M. Chancel's process, thanks to an ingenious artifice, it is possible to escape this inconvenience by reducing the iron to a minimum; but, in order to obtain this result, the necessity of passing, first, a set current of sulphuretted hydrogen, and then a current of carbonic acid, increases the difficulty of an analysis already very delicate.

From the moment that the oxide of iron showed an energetic tendency to precipitate itself with the insoluble phosphate, even in nitric acid, it seemed to me that there was more chance of success in circumventing the difficulty than in seeking to do away with it. I have arrived at this conclusion by considering, on the one hand, the insolubility of phosphate of tin in nitric acid, and, on the other, the perfect solubility of the same phosphate in hydro-sulphate of ammonia.

M. Reynoso's process, which served me as a starting-

point, consists in collecting the mixture of stannic acid and phosphate of tin furnished by a known weight of tin, and in weighing the calcined precipitate. This process would, in fact, be extremely simple if the stannic acid did not, unfortunately, carry with it almost all the oxide of iron, and a portion of alumina, at the same time as the phosphoric acid.

I found that this evil was easily remedied by treating the precipitate with hydrosulphate of ammonia, which, by redissolving all the phosphate of tin, admits of the direct estimation of the phosphoric acid in the state of ammoniaco-magnesian phosphate, leaving insoluble oxide of iron and alumina, which is then added to the liquid of the bases.

Supposing the complicated case in which oxide of iron, alumina, lime, magnesia, &c., are present with the phosphoric acid, the operation should be conducted in the following manner:—The matter, free from chlorides, is dissolved in nitric acid, and a certain weight of pure tin is thrown into the solution (a weight equal to four or five times the presumed weight of the phosphoric acid is found by experience to be sufficient); this tin, while passing to the state of stannic acid under the influence of nitric acid, takes with it all the phosphoric acid, as well as a large portion of the iron and alumina; this is first washed by decantation, then on a filter, and the nitric solution free from tin, containing all the bases, minus a portion of iron and alum, is set aside. This done, the precipitate is redissolved in a small quantity of aqua regia, and, disregarding for the moment the disintegrated matter on the filter, or small portions of phosphate of tin remaining insoluble, the solution is to be supersaturated with ammonia, and then we add excess of hydrosulphate of ammonia.* A black precipitate of sulphide of iron and alumina is immediately produced; the mixture is to stand at rest for an hour or two, then to be filtered, taking care to wash the precipitate with hydrosulphate of ammonia, to take away all trace of tin. Then, all that remains to be done is to add sulphate of magnesia to the filtered liquid in order to obtain, more rapidly than under ordinary circumstances, the characteristic precipitate of ammonio-magnesian phosphate. The filter containing the sulphide of iron and alum is redissolved in nitric acid, and the filtered solution is added to the original liquid of the bases, which are to be separated in the usual way.

This process is very simple and rapid; it separates in a few hours all the phosphoric acid contained in the most complicated mixtures, and then effects with exactitude the separation of the bases. Numerous experiments have, moreover, shown me that phosphoric acid is estimated in this way with unusual precision.

As an illustration of the results this method gives, I add the numbers furnished by two synthetic analyses, performed in the laboratory of the *Ecole Polytechnique*, one by myself, and one by M. A. Charpentier, under my direction:—

	I.		II.	
	Used.	Found.	Used.	Found.
Phosphoric acid .	0·445	0·443	0·190	0·191
Oxide of iron .	0·100	0·102	0·180	0·174
Alum. . .	0·125	0·126	0·300	0·294
Lime . . .	0·250	0·248	0·080	0·078
Magnesia . .	0·080	0·081	0·250	0·247
	1·000	1·000	1·000	1·988

—*Comptes-Rendus.*

* It is also well to put the hydrosulphate in contact with the precipitate of stannic acid and phosphate of tin, but the solution then occupies more time.

TECHNICAL CHEMISTRY.

Contributions towards a Knowledge of the Inorganic Constituents of Plants, by Dr. CHARLES A. CAMERON, *M.R.I.A., F.C.S.I., Corresponding Member of the Agricultural Societies of New York, Belgium, &c.*

(*Continued from page* 258.)

Experiments with Sodium.

Sodium compounds are found in large proportions in the ashes of members of the families *Cruciferæ*, *Liliaceæ*, and *Algæ*. It occurs in greater or less proportion in most plants; but it is very doubtful whether or not it is essential to any. Sprengel found 38 per cent. of soda in the ashes of bean seeds, Richon 19 per cent., and Levi nearly 12 per cent. Daubeny could only find in the same substance 7 per cent. of sodium, as chloride and oxide, in one specimen, 2·25 per cent. in another, and 1·45 per cent. in a third. Way and Ogston found 13·61 per cent. in the straw of the bean, and 2·8 in the seeds; and lastly, Boussingault, so remarkable for the accuracy of his analyses, found no sodium in the seeds of French beans (haricots), whilst Richardson obtained 21·20 per cent. of soda and chloride of sodium in the pods of the kidney bean. The analyses of the ashes of the pea, made by different chemists, exhibit a great variation in the amount of sodium. According to Sprengel, soda constitutes 20 per cent. of the pods and an inappreciable amount of the straw. Boussingault found 2·50 per cent. of soda in the pods, and Hartwig 13 per cent. of soda and chloride of sodium in the straw. According to Erdmann, there is 1·50 per cent. in the straw and none in the pods. Rammelsberg found 19·82 per cent. in the straw and none in the pods. Richardson obtained 19 per cent. from the pods. Spinach and asparagus are very rich in soda. The ashes of the former yield, according to Richardson, 42·89 per cent. of soda and chloride of sodium, and those of the latter 47·15 per cent. In no part of the horse-chesnut, at any stage of its growth, could Wolff detect sodium, and Staffel (the Author of the Jena Prize Essay, "Whether the Quantity of the Inorganic Constituents of the same Plant and of the same Organ varies in the different periods of vegetation"), who sought specially for sodium in the wood, the leaves, and other parts of the horse-chesnut and of the walnut, failed to detect it in those trees. Of three specimens of *Armeria maritima*, analysed by Voelcker, the first contained 28·48 per cent. of soda and chloride of sodium; the second, 18·44 of chloride of sodium; and the third, neither soda nor chloride of sodium. In several analyses of the potato, made by different chemists, the proportion of soda is high, and in others very low. Waltz found 9 per cent. in the ashes of the tubers of early red potatoes; and the mineral part of the haulms of "Axbridge kidneys," examined by Thomas, contained 16 per cent. of soda. In a complete analysis of the potato, recently published by Clare Calvert, the presence of soda is not stated. J. W. Hardy, of Virginia, has analysed the potato and the yam, or sweet potato (*Convolvulus batatas*). In the ashes of the tubers of the former he found 0·5 per cent., and in those of the tops 0·7 per cent. of soda; the ashes of the tuberous root of the yam yielded 0·8 per cent., and those of the tops 0·6 per cent. of soda. In all of Hardy's analyses the proportion of soda to potash was, on the average, as 1 is to 70. I have analysed potatoes which were grown at Skerries, in the county of Dublin, within a few yards of the sea, and on a soil containing nearly 2 per cent. of soda and chloride of sodium, yet I found

only a mere trace of soda in the tubers, and a very small proportion of that substance in the tops. The centesimal composition of the (mineral part of the) tubers is shown in the following Table:—

Potash	38·18
Soda	a trace
Lime	2·06
Magnesia	3·17
Sesquioxide of iron	1·06
Sulphuric acid	7·00
Phosphoric acid	10·27
Carbonic acid	18·30
Silicic acid	0·38
Chlorine	1·58
	82·00

Way analysed the matter extracted from the soil by a solution of carbonic acid gas in water, and found it to be rich in soda; yet the plants grown upon the soil containing so much available soda, took but little of it up, —their ashes, in some instances, yielding abundance of potash, but no soda.

According to Dickie, Voelcker, and others, the sea-pink (*Armeria maritima*), scurvy grass (*Cochearia officinalis*), and sea-side plantain (*Plantago maritima*), contain sodium when grown near the sea-shore, and potassium when developed in mountainous situations, or cultivated in the garden.

The investigations to which I have referred, as well as many others which the limited period of time to which the reading of this paper is restricted prevents my bringing under your notice, appear to show:—

1st. That soda is frequently either altogether absent from many important plants, or exists in them in but an insignificant amount.

2nd. That when it is abundant in plants it accumulates, in most instances, in those parts which are not concerned in the perpetuation of the species, and which are also of least importance as food for animals.

3rd. That it has not been proved that the replacement of soda in plants by potash has proved injurious to them.

All the experiments which have been made to elucidate this subject render highly probable the supposition that sodium is not an essential element in vegetable bodies; still it must be admitted that it has not been conclusively proved that this substance is a useless constituent of plants; neither, on the other hand, has it been satisfactorily shown that it performs any important function in the vegetal economy. As a contribution—I trust not altogether valueless—towards the solution of these questions, I offer the results of some experiments performed by myself, and the details of which I shall now proceed to state.

Spinach and asparagus, plants which naturally contain a large proportion of sodium compounds, were selected as the most suitable for the purpose of those experiments. The seeds of these plants were sown in soils and supplied with manures containing no sodium (as in the case of those placed in the vessels of group *c*).

The seeds germinated and produced an average number of plants, which, in due season, flowered and fructified.

During the period of their growth the plants were supplied with carbonic acid gas, and with a solution of that compound in distilled water; they were also occasionally watered with a solution of carbonate of ammonium, containing 1 part of the carbonate in 250 parts of water. The vessels in which the plants were grown were covered with glass shades. The plants were placed in a good southern aspect, and altogether were developed under conditions of growth, if we except the absence of sodium, of a very favourable character.

When the plants had fully matured their seed they were removed from the vessel, thoroughly washed to free them from traces of adhering soil, and burned. The ashes of each kind of plant were carefully examined for sodium with the ordinary chemical means, but not the slightest trace of that substance could be discovered, although in the case of the spinach 286 grains weight were operated upon, and of the asparagus 206.

The ordinary processes of analysis having failed to detect the presence of sodium in these ashes, recourse was had to Bunsen and Kirchhoff's spectrum analyser. The ashes of the two plants were respectively ignited in the flame of a Bunsen's burner, and a spectrum having been produced by the spectroscope, the intense yellow band which appeared therein at once revealed the existence of sodium in the ashes. By repeating this experiment on the ashes of the plants grown in the vessels of group *c*—the barley, oats, peas, and grass—I found that sodium was present in every instance. The soils on which the plants grew were also examined by photo-chemical means, and were found to contain sodium. The presence of this substance in compounds from which it seemed to have been entirely excluded is not unaccountable. Bunsen and Kirchhoff have proved chloride of sodium to be constantly present in the atmosphere, and an abundant constituent of the dust which accumulates on shelves and which we brush from our clothes. I have found it in distilled water which had been prepared in the most careful manner; and I have no doubt that it existed in the soil on which the plants I experimented upon were grown, and in the artificial manures supplied to them before the sowing of the seeds (for they were not examined by the aid of the spectrum analyser till after the analysis of the plants). From one or more, or all of these sources the plants derived the sodium found in them. Now, the question here presents itself, Was the minute trace of sodium found in those artificially-grown plants essential to their healthy development? I think this question must be answered in the negative, as it is inconceivable that any substance found in bodies —mineral, vegetal, or animal—in quantities so minute as to be inappreciable by ordinary chemical means, could be useful, much less essential, constituents. In not one of the plants grown without the intentional application of sodium compounds could the amount of sodium have been so much as the $\frac{1}{20000}$th part of its ashes, or the $\frac{1}{500000}$th part of the whole plant. The proportion may, indeed, have been far less than even this minute quantity, as the spectrum analyser merely indicates the presence, but does not accurately tell us the quantities, of bodies.

The fact of the universal diffusiveness of the greater number of the elements of matter is one of the most wonderful discoveries of this age of scientific marvels, and there is little doubt that in the progress of mental enlightenment nearly all the simple substances will be found as widely disseminated in nature as the so-called universal substance, oxygen. We are now in possession of an instrument of research, the delicacy of which is such that it almost approaches to the infinite. With an apparatus which enables the eye to detect the presence of the 180,000,000th part of a grain of common salt,* it

* Bunsen and Kirchhoff state that by means of the spectrum analyser they can detect the 1-3,000,000th part of a milligramme of chloride of sodium. Miller doubts the accuracy of this statement, but admits that the delicacy of the photo-chemical mode of testing is exceedingly great.

should not surprise us if sodium were found in every substance submitted to the searching scrutiny of such an agent. By its means, or by those of a still more delicate instrument, it is provable that almost every one of the elementary bodies would be found in plants and animals, but subserving as unimportant functions in the economy of those beings, as silver does in the ocean, or carburetted hydrogen in the atmosphere.

It is impossible to accurately determine by experiments conducted in the field, what are, and what are not, the indispensable elements of vegetal nutriment. No matter how completely impoverished the soil selected for the experiment may appear to be, it is certain to contain a notable proportion of all the substances found in the ashes of plants. Field experiments are, however, when accurately conducted, varied and repeated, the safest to rely upon in solving such problems as the following:—Is the turnip benefited more by the application of phosphate of calcium than by that of sulphate of ammonium? Does the application of common salt to meadow or pasture land increase the yield or improve the quality of the herbage? It is from the results of experiments in the field, and from those of the analyses of plants which were grown in ordinary soils, that nearly all our knowledge of the nature of the food of plants has been derived. Such experiments as those performed by me should not, therefore, supersede, in the slightest degree, trials in the field, for it is only when the theoretical deductions of the chemist agree with the practical experience of the husbandman, that we may be certain of having arrived at the truth. The following experiments may be regarded as intermediate between a trial in the field and what have been rather contemptuously termed "flower-pot experiments":—

About 4 cwt. of a loamy soil of medium fertility was deprived of its organic constituents by burning them. The clay was then placed in barrels, the latter filled with water, and the soil well stirred up. After the subsidence of the earthy particles the clear liquid was drawn off, and a fresh supply of water added. This process was repeated about twenty times. At each washing, except the last three, two pints of hydrochloric acid, mixed with four gallons of water, were poured over the clay a few minutes before the barrels were filled with water. The soil was by these means deprived of all or the greater portion of its soluble constituents; and, until exposed for a long time to the action of the atmosphere, by which its rocky portions would be partially disintegrated and rendered available as food for plants, it would be almost, if not quite infertile. Such a soil, I believe, would be admirably adapted for the testing of the relative values and suitability of the different kinds of manures; nor was I mistaken.

The soil was spread to the depth of six inches on a bed of powdered bricks; and, having been manured with a compound containing all the substances, except sodium, known to be useful to vegetation, it was sown with the seeds of asparagus, radishes, and onions. The seeds germinated and produced plants which were in every respect equal to those cultivated in the ordinary manner. Some of the plants were burned, and their ashes, on being submitted to analysis, yielded the following results:—

AMOUNT OF SODA AND POTASH IN 100 PARTS.

	Soda.	Potash.
Asparagus . . .	3·06	30·40
Radishes . . .	1·16	28·14
Onions	2·06	29·16

Asparagus, many varieties of the onion, and, indeed, most species of Asphodeleæ, are indigenous to the sea-coast and salt marshes; and although Fourcroy and Vauquelin found calcium salts to preponderate in the onions examined by them, still I believe it will be found that all the species of Asphodeleæ abound in sodium—a fact which renders the more remarkable the results of the experiments last detailed, which prove that these so-called soda-loving plants thrive equally well when restricted to an almost pure potash diet.

The sodium found in these plants was derived from their seeds, the air, the soil, and the manure—from the last especially as I could not, unless by an immense expenditure of time and trouble, render chemically pure so large a quantity of matter as that employed, nor could the soil be deprived, merely by washing, of all traces of sodium.

Taken as a whole, the facts relative to the presence of sodium in plants which I have brought under your notice, are adverse to the assumption that this element is an indispensable component of vegetal substances, and even render doubtful its direct utility to plants. Indirectly it is probably useful to vegetation, as a solution of common salt possesses the property of dissolving the earthy phosphates. The application of five or six cwt. of salt per acre, to certain crops, has been found in numerous instances beneficial; probably owing to the large quantity of earthy phosphates which its solution in water dissolved, and rendered thereby available as food for the plants. It may also prove serviceable when applied to soils deficient in available potassium, but in such cases I have no doubt greater benefit would follow the application of salts of potassium.

Sodium is invariably found in animals, but it does not enter, like phosphorus, sulphur, and some other of the elements, intimately into the composition of the nerves and muscles; it does not, so to speak, participate in the life of the animal; it forms, however, a large proportion of the mineral part of the blood and of the other animal fluids, and probably exists in a mineral state in every part of the body, except the enamel of the teeth. An adult consumes on an average sixteen pounds of common salt annually, and supposing him to be ten stones weight, four ounces of that will be chloride of sodium. As the food of animals is derived directly, or indirectly, from the vegetal kingdom, it may be urged against my view of the unimportant functions performed by sodium in the vegetal economy, that as chloride of sodium is so largely required by animals, Nature would have furnished a sufficient supply of it, as well as of the other elements of animal nutrition, through the agency of the vegetal kingdom. This objection is obviated by the simple fact that, if we except water, common salt is the only article of food for which we have an appetite in its mineral state—every other substance we derive from plants or animals.

On some Points in Connection with the Exhaustion of Soils, by J. B. Lawes, *F.R.S., F.C.S., and Dr.* J. H. Gilbert, *F.R.S., F.C.S.**

The question of the exhaustion of soils was one of peculiar interest at the present time, not only on account of the great attention now paid to the waste of manuring matters, by the discharge of the sewage of towns into our rivers, but also from the fact that Baron Liebig has recently urged that our soils are suffering progressive exhaustion from this cause, and predicted certain, though

* Abstract communicated by the Authors. (Manchester, September 9, 1861. Section B).

it may be distant, ruin to the nation, if our present modes of procedure be persevered in.

The question was one of chemical facts, and the authors had intended to treat it much more comprehensively than they were able to do on the present occasion. They proposed, by way of illustration, to bring forward one special case of progressive exhaustion, occurring in the course of their own investigations, and then to contrast the conditions of that result with those of ordinary agriculture.

They had grown wheat for eighteen years consecutively on the same land, respectively without manure, with farm-yard manure, and with different constituents of manure, and they had determined the amounts of the different mineral constituents taken off in the crop from the respective plots. Numerous tables of the results were exhibited. The variations in the composition of the ash of both grain and straw, dependent on variations of season, and consequent character of development and maturity, were first pointed out. The general result was, that, with an unfavourable season, there was a slight though appreciable decrease in the per-centage of lime and potash, and increase in that of magnesia; and, again, an increase in the per-centage of phosphoric acid and silica; and, especially in the case of the straw ash, a decrease in that of sulphuric acid. Turning to the bearing of the results on the main subject of inquiry, it appeared that when ammonia salts were used alone, year after year, on the same land, the composition of the ash, both of grain and of straw, showed an appreciable decline in the amount of phosphoric acid, and that of the straw a considerable reduction in the per-centage of silica.

When ammonia salts alone were used, the amount of mineral constituents in the crop of a given area of land was very much increased,—much more so than when a liberal supply of mineral constituents alone was used. But, in neither of these cases was there anything like the amount of mineral constituents obtained in the crop that there was when the ammonia salts and mineral manures were used together, or when farm-yard manure was employed. The greatest deficiency indicated was in the silica and the phosphoric acid, and next in order came potash and magnesia. The exhaustion here apparent was, however, not to be wondered at when it is considered that in these experiments, in which both corn and straw were annually removed without the usual periodical return of farm-yard manure, there had been on the average annually taken from the land by the use of ammonia-salts, about twice as much phosphoric acid, about five times as much potash, and about twenty-five times as much silica as would be removed under a system of ordinary rotation with home manuring, and selling only corn and meat; in fact, in sixteen years there had been taken from the land as much phosphoric acid as would require thirty-two years, as much potash as would require eighty-two years, and as much silica as would require 400 years of such ordinary practice to remove.

Again, the authors estimated that in the experiments of the Rev. Mr. Smith, of Lois Weedon, on the growth of wheat year after year on the same land, without manure, there had been an annual extraction from each acre of land of about three and a-half times as much phosphoric acid, about seven times as much potash, and about thirty-seven times as much silica, as there would be in the ordinary course of practice; yet, after some fifteen years, the crops at Lois Weedon were said to be not at all failing.

The authors did not recommend such exhaustive practice as that quoted, either from their own or the Rev. Mr. Smith's experiments; but the instances given showed the capabilities of certain soils, and in one case the conditions under which the point of comparative exhaustion had been reached. It was, of course, impossible to state the limits of the capability of soils generally, so infinitely varied was their composition; but it would be useful to give an illustration on this point. Reckoning the soil to be one foot deep, it was estimated that it would require of ordinary rotation, with home manuring and selling only corn and meat, about 1000 years to exhaust as much phosphoric acid, about 2000 years to exhaust as much potash, and about 6000 years to exhaust as much silica, as, according to the average results of 42 analyses,† relating to 14 soils of very various descriptions, had been found to be soluble in dilute hydrochloric acid. Many soils had, doubtless, a composition inferior to that here supposed. In a large proportion, however, the amounts of the above constituents assumed to be soluble in dilute hydrochloric acid would probably be available for plants long before the expiration of the periods mentioned; whilst, in a large proportion, there would still be further stores eventually available within a greater or a less depth from the surface.

But the exhaustion of mineral constituents by the sale of corn and meat alone was, in reality, not so great in the ordinary practice of this country as had been assumed for the purpose of the above illustrations. Where there was no purchase of cattle food or of artificial or town manures, the sales of corn and meat would, on the average, be much less than were taken in the authors' estimates; and where such materials were purchased with any degree of judgment in the selection, there would always be much more phosphoric acid (otherwise, the most easily-exhausted constituent) so brought upon the land than would be obtained from it in the increase of produce yielded; in fact, under such conditions, in many soils potash was more likely to become deficient. Again, by no means the whole of the mineral constituents sent from the farm in the form of corn and meat will reach the sewers of our towns, and thence our rivers, a not inconsiderable portion finding its way back to the land in some form; in addition to which, imported corn, meat, and other materials will contribute something to the restoration of our own cultivated land. It is at the same time certain, that so much of the refuse matters of our towns as becomes diluted with water in the degree recognised under the present sewerage system, will be applicable as manure, on the large scale, only to succulent crops, and especially to grass land; and so far as this is the case, they will, of course, not directly contribute to the restoration to the land under tillage of the mineral constituents sent from it in its produce of corn and meat. When other descriptions of produce than corn and meat, such as roots, hay, or straw, are largely sold, compensation is generally made by the return to the land of stable or town manures of some kind. If this be not done, the loss of mineral constituents may, indeed, be very considerable.

In conclusion, whilst the authors insisted upon the importance of applying to agricultural purposes as much as possible of the valuable manuring matters of our towns, they, at the same time, believed that modern practices, taken as a whole, did not tend to exhaustion in anything like the degree that had been supposed by some.

† The accuracy of some of these analyses is, however, admittedly open to question. (See report by Magnus, *Ann. d. Landwirthschaft*, xiv. 2.)

PHARMACY, TOXICOLOGY, &c.

On the Estimation of the Purity of Chloroform, by M. Hardy.

According to M. Heintz, chloroform is not attackable by alkaline metals, even at the boiling point. M. Hardy proposes to make use of this reaction in estimating the purity of chloroform. If a small piece of sodium thrown into chloroform remains unattacked, the chloroform is pure.

When this anæsthetic agent contains alcohol, or other substances susceptible of alteration, a disengagement of gas takes place. These gases are generally composed of hydrogen, marsh gas, and oxide of carbon; if the foreign substance be wood-spirit, the gases are always formed of hydrogen and oxide of carbon. This reaction takes place in the cold, and is almost instantaneous. After all disengagement of gas has ceased, the chloride separates, and the chloroform, freed from foreign matters, is pure. —*Répertoire de Chemie.*

INTERNATIONAL EXHIBITION.

Chemical Substances and Products, and Pharmaceutical Processes.

In commencing our Reports on the Chemical Contents of the International Exhibition, it is hardly possible to avoid some notice of the building itself. Public opinion is almost unanimous on its merits, and we can only add one more testimony to the unredeemed ugliness of the exterior, at all events in its present condition. What it may be made if the edifice should remain, and receive all the decorations which the architect has designed, it is hard to say and may be premature to speculate upon.

On passing to the interior, but little more satisfaction is experienced. The visitor who had the good fortune to witness the Exhibition of 1851 immediately misses the magnificent *coup d'œil* which struck him on entering that building. But perhaps it is too early to express an opinion on this matter. The nave is at present in much confusion, and in a few weeks its appearance may be greatly improved. Now, however, it seems much overcrowded,—the impression received at almost every part of the building except the picture galleries.

The visitor who goes especially to inspect the Chemical Department had better prepare himself by a diligent study of some ground-plan of the building, or it may be a long time before he discovers the south-east passage of the Eastern Annex, in which the articles included in Class 2 are located. When he arrives there, however, we have no doubt he will experience a lively satisfaction. As compared with the Exhibition of 1851, the display of chemical products indicates a decided advance in their manufacture on the part of our countrymen. In that year, when the classification was by no means so complete as on the present occasion, the number of exhibitors was only 126. Chemical and Pharmaceutical Products were then classed together. In the present Exhibition, Chemical Substances and Products form one class, and Medical and Pharmaceutical Processes form a sub-class; and the number of exhibitors in these two divisions amounts to 205,

But it is not only in the number of exhibitors that the new Exhibition excels the old. One of the first things that strikes the visitor's eye as he enters the department is the magnificent case, No. 581, of Messrs. Perkin and Sons. As the originators of a branch of industry entirely new since 1851, the Messrs. Perkins certainly deserved a prominent place in the present Exhibition. They have here a collection illustrating their manufacture of the mauve colour, from the rough coal-tar to the pure dye, and dyed specimens of all kinds, in skeins and fabrics, with the various mordants used in fixing the colour. The completeness of the collection will give it an interest to all intelligent visitors, beyond the beauty of display, and will make them readily appreciate the happy intelligence which succeeded in extracting "a thing of beauty" from the vilest refuse, and the skill which has adapted it to the most agreeable forms of decoration.

But the effect of mauve becomes sombre on long contemplation, and we turn for relief to Case 600, for which, too, we may obliquely thank Mr. Perkin. It is the brilliant display of the Magenta colour, by Messrs. Simpson, Maule, and Co., with its gorgeous crown of crystals of the acetate of rosaniline. The remarkable brilliancy of the green shade of these crystals, when seen by reflected light—only equalled by the colour observed in the plumage of some birds and on the bodies and wings of a few insects—is, we fear, not seen to the best advantage in the present situation. The crown would look better, we think, under a shade at the top of the case, and then the larger and very perfect crystals of the same material, now in a dish beside it, would be seen much better. The specimen of yellow dye in the same case we should be glad to know more about; and it is to be regretted that the exhibitors have not, like the Messrs. Perkin, shown specimens dyed with the different shades and colours. Like the Messrs. Perkin, they have judiciously displayed materials illustrating the manufacture in all its stages, from the coal-tar naphtha, nitrobenzol, aniline, arsenic acid, dry or not, up to the salts which possess and confer the colour. These two cases will be sure to receive the highest commendation, and will be looked at with much interest by those of our subscribers who have read the articles on the aniline colours which we have from time to time published.

The first thing which will attract the attention of the pharmaceutical visitor is the oaken structure, No. 664, contributed by the Pharmaceutical Society. Of this, as a piece of cabinet-making, it is impossible to speak in too high praise; but there our commendation must nearly end. It is worthy of notice that the leading members of the Society are conspicuous by their absence of participation in this display of drugs, and are represented by cases of their own standing in proximity, and somewhat damaging contrast, to the display of the Society. The collection comprises almost everything (except chemicals) which is to be found in a druggist's shop, including Aqua Sodæ Effervescens, Syrupus Simplex, and Spiritus Vini Gallici, and only needs a bottle of Aqua Pura to make it quite complete. Standing on the shelves they do, we are bound to say that the bottles, although their contents may be of the best, have a very mean appearance, and they are further disfigured by the dog-latin labels common in druggists' shops. There may be good reasons for a chemist mystifying his customers, but why on earth should not a Covent Garden herbalist call his goods by intelligible names? What, we should like to know, have some very fine English camomiles done that in an English exhibition they should be ticketed, *Flor. Cham. Dup. Ang.!* and why, if Messrs. Butler and M'Culloch must indulge in this canine latinity, did they not secure the services of some one who could, at all events, spell it correctly, and not label red poppy flowers, of a particularly good colour, *Flores Rheœdos?*

These things only disfigure a collection of very well-dried plants and degrade the whole display. They would be beneath notice on any occasion except this, when critical visitors from foreign countries jealously scan everything of British production, and are too happy to go away sneering and saying, "They do these things better in the Sandwich Islands." There are some other things—specimens of Persian and Egyptian opiums, for example—without any labels at all; and there are some ordinary looking samples of sarsaparilla and some large pieces of cinchona bark, only curious from their size, on the shelves below, which altogether make up a display which we may allow to pass with the safe criticism that it might have been made a great deal better if a little more trouble had been taken.

This, perhaps, is the place to make a remark on the absence of one important firm from the Exhibition. The Apothecaries' Company has nothing to exhibit. Now, as one of the largest manufacturers of pharmaceutical products in London, having a large share in the supply of Government stores, and a great trade with the medical profession and general public, we think they certainly ought to have contributed to a national Exhibition, and we have reason to believe that their products would have been among the best. But the Company seems as anxious to suppress a knowledge of the commercial side of its functions as M. Jourdain was to conceal the exact commercial position of his father.

The case of Messrs. Allen and Hanbury, No. 644, stands opposite the collection of the Pharmaceutical Society. We are at a loss to know why these gentlemen, as well as some others, did not add their whole collection to that of the Society, seeing they are among the leading members. This case also contains some well-preserved English medicinal plants. There are also some barks of rarer plants, and a very large specimen of red cinchona bark. There is also a specimen of the new hæmostatic agent, *Pidang kao*, or Penawaur jambi. When we were boys, some felt off a beaver hat, or a cobweb, was found to be an efficient hæmostatic agent in the case of a cut finger; and cobwebs were sometimes made into pills and administered for internal hæmorrhages. After that a large woolly leaf called Matico, which stanched blood when applied to a wound, was imported, and they made a tincture and infusion of it, which were supposed to be endowed with hæmostatic virtues. Now, a strange-looking plant, with long, fine hairs, which have the same effect, is brought to England, and we suppose a tincture and infusion of these latter will be made, and be found to produce wonderful effects. The inductive method, as applied to medicines, is sometimes a curious study.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Three Lectures on Spectrum Analysis, by Dr. H. E. Roscoe, *Professor of Chemistry in Owens College, Manchester.*

Lecture III. (*Saturday, April* 12, 1862.)

Ladies and Gentlemen,—In our last lecture we examined the mode of obtaining the spectra of such substances as were volatile at the temperature of the non-luminous gas flame. We placed in the flame certain bodies, such as sodium and lithium, and we saw that these bodies thus placed in the flame became volatile, and that their luminous vapour gave spectra which were characteristic of the bodies themselves. I drew your attention to this diagram (showing the spectra of the alkalies and the alkaline earths). This, however is only one, and a very small part of the number of applications of this method of spectrum analysis. If we place platinum in the flame, we see that this platinum imparts to the flame no peculiar colour, and this is because the platinum is not volatile at the temperature of the flame, and, therefore, does not give the luminous vapour which is characteristic of the metal. In order to obtain, then, for this and other metals, and by far the larger number of them, the spectra which are peculiar to them, we must make use of another method, and we must employ a higher temperature. This higher temperature is best obtained by means of the electric spark; and it is to this that I would, for the first few minutes of this day, direct your attention.

Wollaston, who discovered the dark lines in the solar spectrum, was also the first to observe the bright lines in the spectrum of the electric spark; but Mr. Faraday seems to have been the first to declare that the electric spark consisted solely of the material particles of the poles and of the medium through which it passes. He speaks, in his experimental researches, of the electric spark as "produced by a current propagated along and by ponderable matter, and heated in the same manner, and according to the same laws, as a voltaic current heats and volatilises a metallic wire." Mr. Wheatstone, however, in the year 1835, was the first who pointed out that the spectra produced by the sparks from poles of different metals were dissimilar; and he concluded that the electric spark results from the volatilisation or ignition, and not the combustion, of the ponderable matter of the poles itself, for he observed the same phenomena *in vacuo* and in hydrogen gas; and in 1835 he writes concerning these differences observable in the spectra produced by the poles of the different metals:—"These differences are so obvious that any one metal may be instantly distinguished from another by the appearance of its spark; and we have here a mode of discriminating metallic bodies more readily even than a chemical examination, and which may hereafter be employed for useful purposes." I would refer you to this diagram, which is a copy of that drawn by Wheatstone in 1835, showing the bright lines in the various metals. Here we are supposed to have the different colours, and each vertical division is a different metal. Here we have tin, bismuth, cadmium, zinc, and mercury, and here the sodium spectrum, giving one bright yellow band. Here, you see, for each metal there are distinct bands. But it was chiefly through the experiments and researches of the Swedish philosopher, Angström, that we gained a more intimate knowledge of the structure of the electric spark. In the year 1855, Angström investigated the matter very thoroughly, and pointed out the important fact, that when we look through the proper instrument at the spectrum of this bright electric spark, we see not only bright lines produced by the metal itself, but also bright lines which are produced by the incandescence of the air through which the spark passes; for not only are the particles of copper or zinc, or whatever metal may be employed as electrodes, heated to luminosity, but the air is also rendered incandescent, and we get in this way two spectra superimposed,—one derived from the metal of the poles, and one derived from the gas or the atmosphere through which the spark passes. This is a very important point, and it was first clearly established by Angström.

Now, this method of examining the spectra of the metals by means of the electric spark, is one which can be brought into very general use. In order to illustrate to you that we do get different coloured sparks when we have different metals in contact with the pole, I will here introduce a small quantity of the chloride of strontium placed on this

bead, and we shall see that the spark is in this case coloured bright red, owing to the strontium being volatilised and giving its peculiar colour, which will be indicated in the instrument by its peculiar spectrum. In this way we can detect the presence of any other metal, and in this way Kirchhoff has obtained the spectra of a great number of metals. I cannot, I am sorry to say, show you the same thing that we see when we look through this instrument; but I can show you the diagrams of his lines, and some of the very beautiful spectra which we obtain when we use the electric lamp. I will first show you Kirchhoff's diagram, that you may see what these electric lines are really like. I am able here to throw on the screen a diagram of Kirchhoff's metallic lines. What I want especially to point out are the lines which we see here. You remember that the lines above denote the dark lines in the solar spectrum. The lines below, having the chemical symbols of the metals attached to each of them, are the metallic lines. This, Hg, to which I now point, indicates *hydrargyrum* (mercury), and here is another line given of the metal mercury. Here we notice two lines between these lines *D*,—two lines given by the glowing vapour of zinc and nickel. Here are a number of lines all connected together by a horizontal division; they are all given by one metal,—viz., iron. All these bright lines are produced by the metal iron, and are obtained in the way I have described, by the help of the electric spark passing from iron poles. I may show you on another slide lines illustrating the same thing. You will observe here also lines below the solar spectrum. Here we see the lines of magnesium, iron, copper, cadmium, lead and nickel, and a number of other metals. You will notice, too, that these lines are all quite fine lines, and this represents what we really see when we look through an instrument, as perfect as Kirchhoff's was, at the spark which I exhibited a short time ago.

Now, we can throw on the screen the spectra of some of these metals produced by the electric lamp. We will take, for instance, the spectrum of silver first. You will notice that here we do not get fine bright lines, but we get a series of very magnificent, bright bands. Here we get the bright bands due to silver. These now, if we examine them with an apparatus like Kirchhoff's, resolve themselves into an infinite number of beautiful fine lines, which are more characteristic than these bands are of the presence of silver. The necessity of taking, here, a wide slit, and the fact that our arrangement is not optically perfect, produces these bright bands, which in other cases would be given as bright lines perfectly fine. Now we will take another spectrum. We will next take the copper and then the mercury spectrum. Here we get the bright bands characteristic of these metals.

In this way, Kirchhoff and Bunsen have been able to distinguish with the greatest ease between the rarest metals. They have distinguished between such metals as erbium and terbium, and between didymium, lanthanum, and cerium—metals which are extremely analogous and difficult to separate from each other by ordinary chemical means; but by examining their spectra we get at once a distinctive test for these three very analogous metals, and a decisive answer as to which of them is present.

We may ask ourselves, can a great many mixed metals be thus detected? This is a question much more easily asked than answered. This subject, as I have said, is a very new one. It is in its infancy, and it is a subject which nobody, except Bunsen and Kirchhoff, can be said to have investigated with any degree of exactitude; and we do not possess sufficient information to enable us to say how far, when we have a number of metals present together, the lines may all be satisfactorily distinguished.

A very interesting point respecting the alteration which the spectrum of certain metals or vapours undergo on increasing the temperature, was first observed by Dr. Tyndall. When working this electric lamp he observed in the lithium spectrum a bright blue band appear when the temperature is raised. The other lines did not change, but a bright band appeared. This is also observed when we bring into this intense electric spark some lithium compound. My friend Professor Clifton and I confirmed this observation, and we noticed that this blue line of the lithium, when looked at with one prism, appears to coincide exactly with the ordinary strontium line δ. We could see no difference in refrangibility between the two. But when we came to examine them more minutely, and took a larger magnifying power and a greater number of prisms, we then found that these lines were not coincident. They were not identical in position; but the difference was very slight, not greater than the distance between the two sodium lines. Here I have a diagram which will illustrate this. Here we see the lithium spectrum with the one bright line which appears only at a high temperature. It is the effect of the high temperature, but how produced of course we do not know. It appears, when we use one prism, to coincide exactly with this line, *delta*, of the strontium; but when we employ a larger number of prisms, we find that they are not identical in refrangibility. We noticed also that three other bright violet bands came out in the strontium spectrum at the high temperature, which were not seen at the low temperature.

Another very important and very interesting observation was likewise made by us with this more perfect instrument to which I have referred. We found that the broad bands which are characteristic of the spectra of the alkaline earth metals at a low temperature, and which you see so well in these diagrams—the broad bands in the strontium, the broad bands in the calcium—disappear. The broad bands in the calcium which you notice here, and which we saw when we looked at the calcium spectrum produced by placing a bead of chloride of calcium in this intense electric spark, entirely disappeared; and in their place we got bright lines which were not coincident with the broad bands, which differed in refrangibility from the broad bands. Now, this is a very interesting observation, and, if we may allow ourselves at this incomplete stage of the inquiry to speculate on the cause of this alteration from the bright broad bands to the bright narrow lines, we should say that this change is probably due to the decomposition of the difficultly reducible oxide in the flame. When we place the calcium bead in the flame and see the broad band, it probably is not the metal itself which gives us the spectrum, but it is a compound—probably the oxide—which is not decomposed at the temperature of the coal-gas flame; but when we raise its temperature, as we do by passing the electric spark, it does decompose, and we then get the true spectrum of the metal. This is a mere speculation; but it may be the reason of the change, for we know how chemical compounds are split up at a high temperature. We do not see anything of the kind in the case of the metals whose oxides can be easily decomposed. The metals of the alkalies, for instance, do not give us these phenomena at all. We observe it simply in the case of barium, strontium, and calcium.

There is another class of bodies besides these whose spectra we looked at with the electric spark, which also give, when ignited, a peculiar spectrum; and these substances which I refer to are those which are gaseous at the ordinary temperature. Among these are nitrogen, hydrogen, and chlorine. These spectra have been drawn, and the spectra of these bodies have been investigated with great care by a German philosopher, Professor Plücker; and he for this purpose employed a very beautiful arrangement, which I can illustrate to you here. He filled some tubes with the gas that he wished to examine, and he then, by a peculiar arrangement, pumped out all but the very minutest trace of hydrogen, or of nitrogen, or of oxygen, or of whatever gas he wished to examine, and he thus got

the vacuum of the gas, or a mere trace of it; and he then passed a current of electricity through the vacuous tubes. Now, we know that matter in a very rarefied state allows the passage of the electric spark; and we see that in the colour of the vacuum we get the peculiar spectrum of the gas itself.

Here I can show you some tubes like those which were used by Plücker. Here we have one which contains hydrogen; here we have one which contains nitrogen; here we have one which contains another substance—chloride of tin; and you will see in all these, when the electricity passes through, that we have a very different kind of spark. If we place these beautiful luminous tubes in front of the slit of the apparatus, and look at the light which passes through the slit, through the prism, into our telescope, we see the spectrum of each of these peculiar substances. The red is the hydrogen, the purple is the nitrogen, and the light green is the spectrum produced by the chloride of tin. Now, in the same way, I may show you this larger tube which gives us the same effect. This larger tube contains nitrogen; it is a nitrogen vacuum; and here you will see the peculiar light which is given off by the nitrogen; and we should also get the peculiar spectrum of nitrogen if we looked at the light given off by this gas through the prism. If we want to see the spectrum of these things, the retina must be the screen; and, therefore, I can only show you a drawing—a very rough one—which I have made of the spectra of these gases. This is the complicated spectrum of nitrogen, and this is the more simple one of hydrogen,—one bright red line and one bright blue line. It gives off only these two kinds of light.

Now, in these new and interesting subjects like the one which now occupies our attention, the mind is very likely to be led away into speculations which, however engrossing they may prove, are foreign to the spirit of the exact scientific inquirer. Such speculations would, in this case, have special reference to the possibility or probability of arriving, by help of the observations of the bright lines which these elementary bodies give us, at some more intimate knowledge of the composition of the so-called elementary substances. Shall we, for instance, we may ask ourselves, find, in examining the spectra of the various elementary bodies, that we get bright lines in one which occur in another? Can we find out any relation between the spectra of such substances as iodine, chlorine, and bromine; or between those of the alkaline metals, such as potassium, sodium, cæsium, and rubidium? These questions will naturally occur to every one. At present the subject is in such a youthful state that such speculations are almost useless, because they are premature and the data are insufficient; but doubtless a time will come when these matters will be fully explained: and a future Newton will place on record a mathematical theory of the bright lines of the spectrum as a striking monument of the achievements of exact science.

Now, we must pass on to the immediate subject of to-day's lecture, the time for which I am sorry to say is more than half gone before we commence it.

We have this day, ladies and gentlemen, set a somewhat formidable task before ourselves—that, namely, of showing that a result apparently as marvellous as the discovery of the *elixir vitæ* or of the philosopher's stone, is a legitimate and necessary conclusion drawn from well authenticated and exact experiments. We have, in short, to prove that chemical analysis has now stretched forth her hand beyond the limits of our earth, and that she now affords us certain information respecting the chemical constitution of the sun and the far distant fixed stars. What I wish to do is to make plain to you the grounds upon which Professor Kirchhoff has proved with certainty that the atmosphere of our sun, situated at a distance of ninety-five millions of miles, contains certain substances which we meet with on this earth and with which we are well acquainted; and I shall point out to you the reasons for thinking that the fixed stars are differently composed.

The knowledge which we have concerning the composition of the sun's atmosphere is all derived from Kirchhoff's discovery concerning the nature of these Fraunhofer's lines to which we have so often referred; and although other philosophers have come near to the explanation of the nature of these lines which throw light upon the constitution of the solar atmosphere, still the glory of having first clearly explained their nature doubtless belongs to Kirchhoff.

Fraunhofer, as early as the year 1814, showed that the yellow lines, which we now know as the sodium lines, coincided with the dark lines *D* in the solar spectrum. If we place the one spectrum above the other, and look at the spectrum of the solar light compared with the spectrum of the sodium light, we see that the dark lines in the one spectrum and the yellow lines in the other possess precisely the same degree of refrangibility. Supposing that this is the solar spectrum, here we see those two dark lines; now exactly under that we place the sodium spectrum, and we see that the dark lines are coincident with the yellow. This is a fact which Fraunhofer observed, and which has been borne out by other observers. Fraunhofer did not then know that the yellow lines were caused by sodium; and he was not aware of the cause of this coincidence. The cause was first really discovered by Kirchhoff. Other lines were also known to be coincident. Sir David Brewster, in the year 1842, showed that a line in the potassium spectrum—the line *alpha* of potassium, coincided exactly with the dark solar line *A*, which is a double line, the one being a double red line, and the other a double black line in the solar spectrum. These facts, however, remained altogether barren of consequences, as far as regards the explanation of the phenomena, except to a few bold minds such as Angström, Stokes, and William Thomson; the latter two of whom, combining this fact with an ill-understood experiment of Foucault's, made in 1849, foresaw the conclusions to which they must lead, and expressed an opinion which subsequent investigations have fully borne out. Clear light was, however, thrown upon the subject by Kirchhoff in the autumn of 1859. Kirchhoff, wishing to ascertain the truth of this often-repeated statement of the coincidence of these yellow sodium lines with the dark lines *D*, allowed solar light to fall through a flame coloured by sodium on to his most beautiful apparatus, a diagram of which I showed you in a former lecture; and he observed, much to his astonishment, that instead of these two dark lines in the solar spectrum, to which he was directing his attention, becoming less distinct, or blotted out altogether by the yellow light of the same degree of refrangibility which was falling into his apparatus from the sodium flame, they became more intense, more distinct, and more visible. Struck by this, Kirchhoff took the light which is given off by intensely-heated lime—the oxyhydrogen, or Drummond's light—and between this and his apparatus he placed the sodium flame; and he made the very important observation, which is the keystone of the whole subject, that in this spectrum of the Drummond's light (which, as you remember,—because it is the spectrum produced by a glowing solid,—is continuous, and contains neither bright bands nor dark bands, but exhibits one continuous series of differently coloured rays) there appeared, when the sodium vapour passed before the light, two dark lines which were coincident with the bright yellow lines which the sodium flame itself would produce. This, as I say, is a most important observation, because it leads to the explanation of these dark lines in the solar spectrum. What, now, does this experiment teach us? It teaches us that the luminous sodium vapour has this very extraordinary power—at least, a power which seems extraordinary at first,—it absorbs that kind of light which it itself gives out. We

see that it takes away from Drummond's light those kinds of rays which it itself gives off. Now, the consequence of this is, that the yellow rays of the Drummond's light being very much greater than the light of the soda flame,—the sodium vapour absorbing the yellow of the Drummond's light,—the difference between the amount given off by the two lights is rendered apparent on the screen, and we get dark spaces in the spectrum where we should otherwise have bright lines. If the amount of light given off by the sodium flame was exactly the same as the amount of that light which it absorbs from the Drummond's light, we should see no difference at all in the spectrum. But there is a difference. The intensity of the light given off by the sodium flame is not so great as the same kind of light given off by the Drummond's light; and the yellow rays given off by the Drummond's light are absorbed by the sodium vapour and dispersed in different directions, so that we see dark lines in the yellow instead of a continuous spectrum. As I have said, I am unable to show you on a large scale this interesting phenomenon in all its beauty and exactitude; but I can show you, I believe, and hope that we can here produce, a black band with the sodium light. We will first obtain the continuous spectrum, and then we will place in front of the slit a very intense flame coloured by sodium. Here we have the colourless gas flame, and we are going to burn here a small quantity of metallic sodium, which I will place in a little cup. What I hope you will see is, that in the place of the yellow light given off by the sodium itself, we obtain a dark absorption band. There you see that dark band,—it is produced by the absorption of this yellow light by the glowing sodium vapour, and the same causes which produce this band produce in the solar spectrum the dark line *D*.

I can illustrate this to you in another way. Here we have a large sheet of flame which I can colour by sodium. I am going to burn in front of this a flame of hydrogen, and I will place in that hydrogen flame also some sodium compound; so that we shall have two sodium flames burning, one in front of the other. I want you to notice that the sodium light passing from this large flame at the back through the sodium flame in the front will be absorbed, and that at the outer rim of this hydrogen flame we shall see a dark band. In fact, it will look just as if the hydrogen flame was smoky, as though we had a smoky candle burning in front of the large flame. There is no carbon in this flame to produce a smoky appearance. We shall have nothing but pure hydrogen burning. We will light our hodrogen here, but we must first make our large sodium flame. This we do by burning a little sodium and wafting the fumes of it into the flame of the gas. Here we have our burning sodium, the fumes of which I waft into the flame. Now, you see the whole flame is turned yellow, and you will notice that here we get a smoky flame. It is now very distinct. If, instead of sodium, we place some lithium in the flame, no black rim to the flame will appear. We shall get the red colour of the lithium flame; but that will not give us any of the black shadow, because we have not there the same yellow light. The placing of the red lithium in the flame shows us that the smoky appearance was really caused by the absorption of the yellow light by the sodium vapour in a state of incandescence.

I can also show you in a third way the fact that sodium vapour is opaque to the light which it itself gives off. That is the point which I wish to make evident to you. Here I have a tube which contains some sodium which I can convert into vapour. By heating the tube as I am doing, it will become filled with sodium vapour, and you will see that it is perfectly colourless and transparent when we look at it with the common gas-light; but when we look at it with the yellow sodium light, which I can here make, it is perfectly opaque. We shall then see that the tube containing the sodium vapour throws a dark shadow on the screen. [The gas-lights were turned down, and the screen was illuminated with a yellow sodium flame.] Now the tube looks black; we cannot see through it; it throws a dark shadow. [The gas-lights were turned up.] Now, by the common gas-light, it is colourless. This shows us, then, very distinctly, that the sodium vapour is opaque for the rays which it itself can emit.

Thoroughly understanding, then, the nature of the phenomena with which we have to deal, let us follow Kirchhoff to the interesting conclusions which he draws from this experiment. He states that from this fact it appears likely that a glowing gas has the power of absorbing especially rays of the same degree of refrangibility as it emits; and that, therefore, the spectrum of such a glowing gas can be reversed, or the bright lines turned into dark ones when a light of sufficient degree of intensity, giving a continuous spectrum, is passed through it. This idea was confirmed by further experiment, for by substituting for the sodium flame in the last experiment the flame coloured by potassium, *dark* lines appeared in the exact position of the characteristic *bright* lines of this metal. Bunsen and Kirchhoff showed in this way that the potassium spectrum could be reversed, for by using a potassium flame we get the potassium lines cut out in dark bands; and they have likewise succeeded in reversing the flames of lithium, calcium, strontium, and barium; and Dr. Miller has also reversed some of the lines in the spectrum of copper. An experiment, lately described by M. Fizeau, beautifully illustrates this reversal of the spectrum of sodium; for although M. Fizeau thinks that the phenomenon is in contradiction to Kirchhoff's theory, it seems to me to be a direct confirmation of it. The experiment consists in observing the spectrum of metallic sodium burning in the air; instead of seeing two bright yellow lines on a dark background, two dark lines in the position of the soda lines on a background of a continuous spectrum are observed. These black lines are produced by the absorption of the yellow light, from the highly luminous sodium vapour, by the less luminous film of sodium vapour outside; and the continuous spectrum is produced by the incandescence of the solid particles of soda mechanically carried up into the flame. This diagram represents what we see when we look through the apparatus at the flame of burning sodium and at the ordinary soda-flame.

Generalising from these facts, Kirchhoff has arrived, by the help of theoretical considerations which I am unable to lay completely before you, at a law, previously partially enunciated by Prevostaye and Dersains, in France, and by Mr. Balfour Stewart, in this country, which expressed the relation between the amount of heat which a body receives, and that which it emits. This law has been called "the law of exchanges." It asserts that the relation between the amount of heat emitted, and that which is absorbed at any *given temperature*, remains *constant for all bodies*; and that the greater the amount of heat emitted, the greater must be the amount of heat absorbed. Kirchhoff has proved that the same law holds good for light as well as heat; that it is as true of the luminous as of the heat-giving rays; and for rays of different kinds, if we compare the same kind of rays. For instance, if we compare red rays emitted with red rays absorbed, or yellow rays emitted with yellow rays absorbed. From this we see that an incandescent gas which is giving off only certain kinds of light,—that is, whose power of emission is finite for light of certain definite degrees of refrangibility, must have the power of absorbing those kinds of light, and those kinds only. That is what we find with the luminous sodium vapour: it has a very large power of emission for the *D* rays, and it has a proportionately large power of absorption for that kind of light; it is opaque for it; and it allows all other kinds of light to pass through it. And we see that every substance which emits at a *given temperature* certain kinds of light, must possess the power at that *same temperature* of absorbing the same kind of light.

We must, however, remember that the emissive and absorptive powers of substances can only be compared at the same temperature. This is of very great importance, for it has been supposed that the law of exchanges does not hold good, because the comparison has not been made at the same temperature. It must not be assumed, as has frequently been done, that because the bright lines of the incandescent iodine spectrum, for instance, do not correspond to the dark absorption bands of the gas at a much lower temperature, therefore the law is faulty or incorrect. We must compare the lines at the same temperature.

Now, we know that the same kind of thing holds good with the other vibrations with which we are acquainted—the vibrations of the air, the vibrations which we call *sound*, the vibration of gravitating matter, while light is supposed to be the vibration of non-gravitating matter—the luminiferous ether. We all know what is called *resonance*. When we sing a particular note in the neighbourhood of a piano, that same note is returned to us. The particular vibrating string which can emit that note has the power also of absorbing vibrations of that particular kind, when proceeding in a straight line, and emitting them again in all directions. We are not, therefore, without analogy in the case of sound, for the correspondence between the absorption and emission of the same kind of light by the same substance.

We will now pass to the application of this principle of the reversibility of the spectra of luminous gases to the foundation of a stellar chemistry. How does this principle assist us in our knowledge of the constitution of the solar atmosphere?

Kirchhoff examined the spectra of the various metals which I have exhibited to you. The spectra were obtained in the way I have described; and he found that a great number of the lines produced by the metals corresponded to dark lines in the solar spectrum. I think this is shown clearly on the larger of the diagrams which I will now show on the screen. These are photographic reductions of diagrams drawn by Kirchhoff, accompanying his Memoir, giving an account of the whole of the subject, which has just been published by Messrs. Macmillan, of Cambridge. Kirchhoff, in examining the spectra of the metals, compared the position, or ascertained the position, of the various bright metal lines by help of the dark lines of the solar spectrum. He used these dark lines as a means of measuring the position of the other lines, and, to his astonishment, he found, in looking at the bright lines which the metal iron produces, that every one of the iron lines which he had occasion to observe in this part of the spectrum under examination—and he observed more than sixty of them—coincided exactly with a dark solar line; that to every one of these dark iron lines there was a corresponding dark solar line. Here you see a representation of them. These are the iron lines, in this particular part of the spectrum; and you will notice that to each one of these iron lines there is a corresponding dark solar line. And not only is there this coincidence in the number and position, but we find that the brightest iron lines correspond with the darkest solar lines.

Kirchhoff has calculated what the chance would be that these bright iron lines should all coincide with the dark solar lines without having some common cause, or some relationship in common. He calculated this chance, which is one-half to the power of sixty. That number is one divided by one million of millions of millions. This is a matter which any one can calculate for himself. The data are all given in Kirchhoff's Memoir; and we may say, in other words, that it is perfectly impossible that this coincidence could occur between the dark lines in the sun and the bright iron lines which we see here, unless they had some relationship in common.

Now, what can this common cause be? It is explained if we suppose that the rays of light which we thus examine have at one time been rays of all degrees of refrangibility, such as we get from the Drummond's light, and that this light, consisting of every kind of rays, has passed through the vapour of iron; for this iron vapour would then absorb just exactly that kind of light which it is capable of giving off, and we should really see what we find is the case—that in the place of these bright iron lines we have dark lines which exactly correspond to them. This explanation is not only one which is satisfactory, but it is the only one which can be given, and therefore we are bound to accept it, at present, as the true explanation of the phenomenon.

This vapour of iron causing these dark lines may be present either in our atmosphere or in the atmosphere of the sun. But it is, however, very unlikely that there can be sufficient iron vapour in our atmosphere to effect this alteration; because we know that the temperature of our atmosphere is so low that it could contain at any rate a very small quantity; and we find, too, that these lines do not become more distinct, or more intense when the sun sinks down to a short distance above the horizon, when the light must pass through a much longer film of atmosphere. Therefore, we come to the conclusion that the iron is not contained in the terrestrial atmosphere, and that it must be contained in the solar atmosphere, and this is the result to which Kirchhoff comes. He draws this remarkable conclusion—"That the spectrum observations point out the presence of the vapour of iron in the solar atmosphere with as great a degree of certainty as we can attain in any question of natural science." This is a pretty positive statement on the part of Kirchhoff, but it is, I believe, perfectly justifiable.

As soon as the existence in the sun of one substance occurring on our globe had been proved, it immediately suggested the comparison of the spectra of other metals with the spectrum of the sun, in order to see whether the vapour of other terrestrial matters were present in the solar atmosphere, and by exerting their absorptive power on the light might thus be recognised. This was found to be the case with chromium, calcium, magnesium, potassium, and sodium. The bright lines which these substances give are less numerous than those of the iron, but they are so characteristic and so distinct that there can be no doubt of the presence of these metals in the solar atmosphere. I will here show you these bright lines of the magnesium. We will put in this screen in which we have these magnesium lines, and I can point out to you the dark lines in the solar atmosphere which are caused by the presence of magnesium. You see these two dark lines on the upper part in the right hand corner. These are the bands which Fraunhofer called *b*, and they are caused by magnesium, or at least some of them are caused by magnesium. You see that the *b* lines are made up of magnesium and iron. They are caused by the presence of iron and magnesium in the solar atmosphere. The metals chromium and potassium also give very distinct bright lines which coincide with dark solar lines. I have written down here a short *résumé* of the reasons why Kirchhoff arrives at this conclusion.

Sodium and Iron in the Sun's Atmosphere.

1. The light emitted by luminous sodium vapour is homogeneous. The sodium spectrum consists of one double bright yellow line.

2. This bright double sodium line is exactly coincident with Fraunhofer's dark double line *D*.

3. The spectrum of a Drummond's light is continuous; it contains no dark lines or spaces.

4. If between the prism and the Drummond's light a soda flame be placed, a dark double line identical with Fraunhofer's double line *D* is produced.

5. If, instead of using Drummond's light, we pass sunlight through the sodium flame, we see that the line *D* becomes much more distinct than when sunlight alone is employed.

6. The sodium flame has, therefore, the power of absorbing the same kind of rays as it emits. It is opaque for the yellow *D* rays.

7. Hence we conclude that luminous sodium vapour in the sun's atmosphere causes Fraunhofer's dark double line *D*; the light given off from the sun's solid body giving a continuous spectrum.

8. Kirchhoff found that each and all of the bright lines produced in the spectra of certain metals—for instance, of iron, magnesium, and chromium—coincide exactly with dark lines in the solar spectrum.

9. Hence it is certain that these bright metallic lines must be connected in some way with the dark solar lines.

10. The connection is as follows: each of the coincident dark lines in the solar spectrum is caused by the absorption effected in the solar atmosphere by the glowing vapour of that metal which gives the corresponding bright line.

In this manner, Kirchhoff has proved that the following metals are contained in the solar atmosphere:—Iron, nickel, chromium, calcium, magnesium, potassium, sodium, barium, zinc, and copper,—the last three only in small quantities, for we see that there are some lines in the copper, zinc, and barium spectra, which are not seen in the solar spectrum, whereas some of the most striking ones are seen. But all the lines of iron, nickel, chromium, calcium, and magnesium are found to be coincident with solar lines. None of the other metals have yet been seen in the solar spectrum.

There are a great many very interesting points which I should like to show you with regard to Kirchhoff's map. Here, for instance, is an extract from the tables which are given accompanying these diagrams; and you see in column I. the numbers representing the lines which refer to his arbitrary scale of millimètres on the top line of his drawing. In column II. we have the thickness and the darkness of the lines, and column III. gives the lines which are coincident with certain metal lines. For instance, this is coincident with the magnesium line,—this with the calcium line,—this with the iron line. Here you see this one line, 1655·6, is both an iron line and a nickel line, and this is both an iron and a magnesium line.

Extract from the Index Table to Kirchhoff's Maps, showing the coincidences of the dark solar and bright metallic lines.

I.	II.	III.	I.	II.	III.
1621·5	1 b		1648·4	4 e	
1622·3	5 c	Fe	1648·8	6 f	Mg
1623·4	5 b	Fe	1649·2	4 e	
1627·2	5 b	Ca	1650·3	6 b	
1628·2	1 b		1653·7	6 b	Fe Ni
1631·5	1 b		1654·0	4 c	
1633·5	4 g		1655·6	6 e	Fe Mg
1634·1	6 g	Mg	1655·9	4 d	
1634·7	4 g		1657·1	5 b	
1638·7	1 b		1658·3	2 b	
1642·1	1 b		1659·4	1	
1643·0	1 b	Ni	1662·8	5 b	Fe
1647·3	5 a				

Whether these apparently coincident lines will prove to be absolutely identical, is a matter which we cannot as yet decide. Kirchhoff thinks that it is necessary to have a much more delicate apparatus than even that which he used.

Fraunhofer's line *D* corresponds on Kirchhoff's map to the lines 1002·8 and 1006·8; Fraunhofer's *E* to the lines 1522·7 and 1523·7; and Fraunhofer's *b* to the lines 1633·4, 1648·3, and 1655·0. Kirchhoff observed the traces of many lines and nebulous bands which the power of even his instrument did not prove adequate to resolve. He adds, "The resolution of these nebulous bands appears to me to possess an interest similar to that of the resolution of the celestial nebulæ, and the investigation of the solar spectrum to be of no less importance than the examination of the heavens themselves."

Kirchhoff by no means wishes to imply that all the lines seen in the solar spectrum are due to absorption exerted by the sun's atmosphere. We know very well that some of the dark lines frequently seen are due to the absorption of the sun's light by the earth's atmosphere, as they are found to increase in intensity as the sun sinks below the zenith. Some of these lines were observed by Dr. Miller, and some have been figured by Sir D. Brewster and Dr. Gladstone. These [referring to a diagram] are lines which are due to atmospheric absorption. They are seen to increase in distinctness as the sun approaches the horizon, and they are therefore proved to be produced by absorption in the terrestrial atmosphere. But the other lines, which we regard as produced by absorption by the sun's atmosphere, are not found to increase with the diminution of the altitude of the sun.

I do not know that I can do better than, in conclusion, read an extract from Kirchhoff's admirable Memoir concerning the physical constitution of the sun. "In order to explain," says Kirchhoff, "the occurrence of the dark lines in the solar spectrum, we must assume that the solar atmosphere encloses a luminous nucleus producing a continuous spectrum, the brightness of which exceeds a certain limit. The most probable supposition which can be made respecting the sun's constitution is, that it consists of a solid or liquid nucleus, heated to a temperature of the brightest whiteness, surrounded by an atmosphere of somewhat lower temperature. This supposition is in accordance with Laplace's celebrated nebular theory respecting the formation of our planetary system. If the matter now concentrated in the several heavenly bodies existed in former times as an extended and continuous mass of vapour, by the contraction of which sun, planets, and moons have been formed, all these bodies must necessarily possess mainly the same constitution. Geology teaches us that the earth once existed in a state of fusion; and we are compelled to admit that the same state of things has occurred in the other members of our solar system. The amount of cooling which the various heavenly bodies have undergone, in accordance with the laws of radiation of heat, differs greatly, mainly owing to difference in their masses. Thus, whilst the moon has become cooler than the earth, the temperature of the surface of the sun has not yet sunk below a white heat. Our terrestrial atmosphere, in which now so few elements are found, must have possessed, when the earth was in a state of fusion, a much more complicated composition, as it then contained all those substances which are volatile at a whiteheat. The solar atmosphere at the present time possesses a similar constitution."

As regards the chemical composition of the fixed stars, all we know is that they contain substances not found in the solar atmosphere, because their spectra exhibit totally distinct dark lines. In Sirius-light we find dark bands which do not occur in the solar spectrum; and it appears that each fixed star possesses a spectrum peculiar to itself. We are, however, only on the threshold of these subjects; the possibility of obtaining an insight into the composition of stellar matter has indeed been shown, but it remains for the astronomer and the physicist to obtain for us that knowledge which we so much desire.

In concluding the brief review which I have thus been able to give you of the principles of spectrum analysis, and of their application to the foundation of a new stellar chemistry, I must express my regret that, owing to the limited time at my disposal, I have found it impossible to bring before you many of the interesting and important points to which I had hoped to be able to direct your attention. Enough, however, has been said to effect that which I most value in a course of lectures of this kind—namely, to interest you in the subject; for I believe that lectures like the present chiefly are useful, not for the direct amount of information which they impart, but for the position which they give to the hearers, enabling them

to study the subject under examination for themselves. If I have succeeded in imbuing the minds of even a few of my audience, not with the passing interest of the hour, but with that deep interest and love which I feel for the subject myself, I shall think myself well rewarded. (Applause.)

CHEMICAL SOCIETY.

Thursday, March 15, 1862.

Dr. A. W. Hofmann, F.R.S., President, in the Chair.

M. St. Claire Deville, delivered a discourse, in the French language, "*On Vapour Densities.*" In many of the laws that govern physical and chemical science, slight variations had been detected, as, for instance, in the value of the angles of the crystals of isomorphous bodies, the law of specific heat, &c.; these, on further examination, would probably prove not real exceptions, but, on the contrary, the natural results of the laws in question, in the same manner as the attraction of gravitation acting on the heavenly bodies caused them to revolve in orbits, which, although having the general form of an ellipse, presented irregularities arising from perturbations caused by the same law. His intention was to consider on the present occasion the law of vapour densities, which assumes that equivalent quantities of different substances occupy the same volume when in the state of vapour. This law had been found correct in the greater number of instances, but there were a few substances which apparently did not conform to it; of these the bodies belonging to the sulphur group were a good example. A remarkable change, however, in the density of the vapour of these substances was caused by heat; at low temperatures the co-efficient of expansion was not constant, but by determining the specific gravity of the vapour at a high temperature a result was obtained which fully bore out the law in question. There was only one substance which, under these circumstances, still continued to be an exception, namely, phosphorus; it was to be hoped, however, that this element would be found on further investigation to offer no real objection, the discrepancy arising from interfering circumstances which had not as yet been thoroughly examined.

At the request of the President, Dr. Frankland remarked that he had made experiments on the same subject, with a view to ascertain whether any real exceptions to this law existed. There was a substance called ammonio-boric-methyl, containing equal volumes of ammonia gas and vapour of boric-methyl, which when in a state of vapour occupied twice the theoretical volume; he had endeavoured to discover whether this result was owing to the decomposition of the compound while in a state of vapour; for the determination of this point a substance was required, which had the property of absorbing ammonia at a temperature above the boiling-point of the liquid, but was incapable of decomposing the compound in question. Chloride of copper seemed to answer these conditions, for it might be boiled with the liquid substance without any decomposition taking place, and it had the power of absorbing ammonia at the necessary temperature. The vapour was therefore exposed to the action of this salt, when a gradual absorption took place, and the volume became reduced to half its original bulk; the absorption, when once started, was quite as rapid as when pure ammonia was operated upon, the only difference being that in the latter case it began immediately, whereas in the former a short interval elapsed before any diminution in volume was perceptible.

It was announced that an extraordinary meeting would be held on Thursday, May 29, when several papers would be read.

NOTICES OF BOOKS.

Chemical Tables, arranged for Laboratory Reference. By Thomas W. Keates, F.C.S. Published at "The Artizan" Office, 19, Salisbury Street, Strand.

These are a comprehensive set of tables, printed on one large sheet, measuring thirty-one by twenty-five inches, the figures and type being remarkably open and distinct. The series consists of twenty-three individual tables referring to data of interest and value to the chemist, manufacturer, and general scientific inquirer. All are subjects upon which information is frequently required during the prosecution of laboratory research, and the details given appear to be sufficiently ample without being extended, on the other hand, to too laborious an expression. The following headings are included in the general series:—Lists of elementary bodies, and of some of the more important binary compounds, with their symbols and equivalents; boiling points of liquids, and, in the case of water, variations in the temperature corresponding to different heights of the barometer; a scale for the determination of altitude by the barometer; elastic force of steam at different temperatures; specific gravity of gases and vapours; melting and freezing points of solids and liquids; a comparison of the degrees of Twaddel's hydrometer, and those of Beaumé, with the ordinary readings of specific gravity; thermometric scales of Fahrenheit, centigrade, and Reaumur; English and French weights and measures; expansion of air by heat, and rates of expansion of certain liquids and solids; degrees of cold produced by several artificial frigorific mixtures; and, lastly, tables indicating the proportions respectively of alcohol, pyroxilic spirit, ammonia, potash, soda, and sulphuric, nitric, hydrochloric, and acetic acids contained in liquids of various densities, as stated on the authority of Dalton, Tünnermann, and Ure.

CORRESPONDENCE.

The Composition of Milk.

To the Editor of the Chemical News.

Sir,—Would you be kind enough to insert the following letter which appeared in the *Dublin Medical Press* of May 7, as it will prove that we were the first to correct our own errors.—We are, etc.

William T. Woufor.
S. R. Pontifix.

Dublin, May 19.

"To the Editor of the Medical Press.

"Sir,—At the time our paper on the Dublin Milk was read, we regarded any increase of water over the quantity contained in the standard milk as so much water added. This we have since seen is not the correct way to view it, and as this error appears in the abstract of our paper given in last week's *Medical Press*, we request you will allow us the opportunity of correcting it. Each per-cent. increase of water in the milk must be regarded as the decrease of so much solid matter, and each per-cent. of decrease in the solid matter is equivalent to the removal of 8.7 per cent. of milk, and the replacement of it by an equal quantity of water.—Your obedient Servants,

"William T. Woufor.
"S. R. Pontifex.

"Dublin, May 1, 1862."

Schnitzer's Process for the Preparation of Nitrate of Potash.

To the Editor of the Chemical News.

Sir,—In your last Number a notice appears, from the *Répertoire de Chemie*, of a process adopted by M. Guido Schnitzer for the preparation of nitrate of potash. The

process appears to me very good until he speaks of the conversion of the caustic soda into Seignette salt (or, as is suggested in a foot-note, into carbonate of soda). What need is there of converting it at all. Caustic soda is now met with in large quantities in commerce, and is extensively used by soap and paper makers, who are thereby saved the tedious operation of causticising, which is necessary when they employ soda ash. All that is required after the nitrate of potash has been separated by two or three crystallisations is to boil down the mother-liquor in an iron pan until on cooling it will become solid, and in that form it can be sent into the market. If necessary, the slight excess of nitrate left in the mother-liquor can be removed by adding a little crude soda liquor (containing sulphide of sodium), or even by the addition of a small quantity of sulphur. As the liquor becomes concentrated, the sulphate of soda formed will rise as a scum, and can be removed, and the nitrogen of the nitric acid will escape as ammonia.

If we are to understand by the last paragraph in your notice that caustic soda is absolutely uncrystallisable, it does not agree with my experience, for by evaporating caustic soda to the consistence of an oily liquor, and then allowing it to cool, I have frequently obtained it as a fibrous mass of long needle-shaped crystals.—I am, &c.

ALFRED NOBLE.

Netham Chemical Works, near Bristol.

MISCELLANEOUS.

Pharmaceutical Society.—The Annual *Conversazione* of the Pharmaceutical Society was held on Tuesday evening last, and was attended by a great number of the members and friends of the Society. As usual, a variety of interesting and valuable objects were exposed to view in the rooms, among which we were pleased to notice the handsome testimonial presented the evening before to Professor Bentley by his former pupils. It consisted of a very elegant claret-jug, with two goblets and a tray, all in silver, the selection of which was as creditable to the taste of the choosers as the testimonial is flattering to the painstaking Professor. The walls of the rooms were on this occasion hung with a few good pictures, and a large collection of dried ferns, and marine plants, made by Mr. Jardine. The arrangement of some of the marine plants, formed into large wreaths, and other patterns on the cards, showed great skill and taste of the collector. Mr. Wright exhibited a large block of Iceland spar, and some fine natural crystals; and Messrs. Knight and Sons contributed a case containing some rare and valuable minerals. Messrs. Bell, of Newcastle, sent a large bar of aluminium, and another of their aluminium bronze, not distinguishable in appearance from gold. Messrs. Simpson and Maule sent one of their beautiful Magenta crowns, and Messrs. Johnson and Sons some fine crystals of nitrate of silver. There were also several ounces of chloride of rubidium shown by Dr. Müller, and some sulphide of thallium exhibited by Mr. Crookes. In the laboratory Mr. Griffin displayed a number of dialysers and furnaces, and Messrs. Jackson and Townson some well made glass apparatus. Here, too, Mr. Ladd delighted the company by throwing the electric light upon bubbles of soap and glycerine, which gives the bubble the appearance of a small moon suspended in the room. Here, also, was shown a quart bottle of frozen benzol. The fact that pure benzol freezes at nearly the same temperature as water, seems in general to have been quite forgotten in estimating the purity of this body, the boiling point being almost exclusively relied on. The ready freezing of benzol, however, affords an easy means of separating it from other bodies. In another room Mr. Ladd exhibited his beautiful experiments with the induction coil; and these things, with a number of microscopes and stereoscopes, and plenty of refreshments, helped the visitors to spend a very pleasant evening.

The Patent Eureka Sanatory and Manure Company.—Among the several proposals which have been entertained for the commercial economisation of fæcal matter and its conversion into inodorous manure, there appear to be many advantageous circumstances in favour of the adoption of a system of collection and manufacture which is set forth in this Company's prospectus. The works are situate at Hyde, near Manchester, where about 200 houses have been brought under the operation of the system, and in that neighbourhood the "Eureka manure" has been somewhat extensively applied to all kinds of crops with very beneficial results. There is no necessity in this place to insist upon the highly valuable nature of the human excreta which constitute the staple of the Company's manufacturing operation; they are known to contain phosphates, ammonia, and other nitrogenous matters, besides bases, all of which are essential to the growth of plants, having indeed been in a great measure derived from vegetable structures originally consumed as food. The importance of restoring these constituents to the earth, and so completing the endless round of changes which they are by nature destined to undergo, has been the favourite theme of Liebig and the agricultural chemists, and is even now once more the subject of anxious Parliamentary inquiry. The difficulty which has hitherto been experienced in carrying out arrangements for the collection and unobjectionable transport of these odorous matters seems to have been overcome by the Manchester company, who have organised a very efficient system of deodorisation (the full particulars of which are not announced in their prospectus), and have thus succeeded in avoiding the necessity for ample dilution with water—a stroke of policy the importance of which was remarkably illustrated in the course of the former Parliamentary report. The concentrated materials are then removed from the premises in a closed box, which is immediately replaced by a like receptacle containing only the small proportion of disinfectant needful for the treatment of two or three weeks' accumulations, whilst the deodoriser, in the form of a dry powder, is said to cost no more than one halfpenny. The price of the manure is quoted at 7*l.* per ton, and in regard to its composition it is stated to consist of the products of both solid and liquid excreta mixed together. Chemical analyses of these individual components are given, but no statement is furnished in regard to the actual proportion of valuable ingredients in average samples of the manure. The phosphoric acid and nitrogen are said to exceed in amount that usually contained in the best Peruvian guano.

ANSWERS TO CORRESPONDENTS.

*** All *Editorial Communications* are to be addressed to the EDITOR; and *Advertisements* and *Business Communications* to the PUBLISHER at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

R. W. writes to ask if some of our correspondents will kindly favour him with a good method of blackening rifle barrels, and which, at the same time, would resist hot water and be comparatively durable.

Analysis of Nitre.—B. H. A.—Weigh out a certain quantity of the finely-powdered nitre; then dry it, and weigh again for the moisture. Dissolve in water and weigh the insoluble residue (if any). Divide the solution into two parts, and estimate the sulphuric acid in one, and the chlorine in the other by standard solutions of chloride of barium and nitrate of silver.

F. R. Y.—You can obtain the Number required on application at our office.

Burette.—The production of a brown precipitate on adding nitrate of silver to your sample of carbonate of soda must have been due either to the presence of caustic alkali or of a sulphide in the specimen examined. The former assumption is more probable under the circumstances mentioned, but we conceive it possible for an impurity, originally in the form of *sulphate*, to exhibit the reactions of *sulphide* after ignition with small particles of carbonaceous matter or in a reducing atmosphere.

THE CHEMICAL NEWS.

VOL. V. No. 130.—May 31, 1862.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Presence of Rubidium in Certain Natural and Industrial Alkaline Matters, by M. L. GRANDEAU.

AT the meeting on the 16th of December last, I had the honour of presenting to the Academy the first results of my researches on the natural occurrence of the metals recently discovered by MM. Kirchhoff and Bunsen. I now propose to make known the numerical results obtained from an analysis of saltpetre residuums from the Paris refinery, and the examination of several products resulting from the treatment of beet-root vinegar for the extraction of potash.

1. Residuums of the Paris Refinery.—To the kindness of M. Maurey I am indebted for placing at my disposal a sufficient quantity of materials in which to search for the new metals. I have analysed the following matters:—

1. Residuums of the year 1861.
2. Residuums of the year 1862.
3. Mother-liquors furnished by the latter residuums.
4. Water oozing from these residues.

Each of these products I have treated in the following manner: I determined the loss sustained by two kilogrammes by desiccation and calcination (to destroy the organic matters); I dissolved in water acidulated by hydrochloric acid a known weight (from 20 to 30 grammes) of the dry residuum. Then I precipitated the filtered and duly concentrated liquid by chloride of platinum, washed the precipitate in alcohol, and weighed. I then washed several times in boiling water, observing the precautions indicated by MM. Kirchhoff and Bunsen. The washings in boiling water were discontinued, when the salt of platinum introduced into the spectrum apparatus ceased to give perceptibly the characteristic rays of potassium.

In the following table I have united the results of the approximative estimation of rubidium in these various matters:—

Residuums, 1861.

1 kilogramme melted residuum corresponded to 1·240 kilogrammes of rough residue.
1 kilogramme melted residuum contained 2·64 grammes of chloride of rubidium.
1 kilogramme rough residuum contained 2·13 grammes of chloride of rubidium.

Residuums, 1862.

1 kilogramme melted residuum corresponded to 1·280 kilogrammes of rough residue.
1 kilogramme melted residuum contained 3·74 grammes of chloride of rubidium.
1 kilogramme rough residuum contained 2·92 grammes of chloride of rubidium.

Mother-water of 1862.

1 kilogramme melted residuum corresponded to 2·100 kilogrammes of mother-water.
1 kilogramme melted residuum contained 6·35 grammes of chloride of rubidium.
1 kilogramme mother-water contained 3·02 grammes of chloride of rubidium.

Ooze-water.

1 kilogramme melted residuum corresponded to 2·300 kilogrammes of water.
1 kiiogramme melted residuum contained 6·85 grammes of chloride of rubidium.
1 kilogramme water contained 2·97 grammes of chloride of rubidium.

The water which in damp weather flows from the heaps of residuums in the workshops is very rich in chloride of rubidium, which shows clearly why the residuums of the year 1861 contain far less of the new metal than those of 1862, the latter having been exposed for a shorter time to the damp air.

None of the matters I collected at the Paris refinery contained cæsium, while the first specimen I examined, some months since, contained a notable quantity of it. It was, it is true, the result of a different treatment to that received by the products forming the subject of this memoir, and I shall presently revert to this subject.

2. Saline and Mother-waters proceeding from Beet-root Vinegar.—Struck by the richness in rubidium of the residuum of the Paris refinery, I inquired what was the principal substance employed in manufacturing saltpetre which could introduce the new metal into these residuums. M. Maurey informed me that for several years the saltpetre used in the Paris refinery has been manufactured exclusively with indigenous chloride of potassium and natural nitrate of soda. Now, both MM. Kirchhoff and Bunsen and myself have many times proved that nitrate of soda is entirely free from cæsium and rubidium,—a fact that I have often verified. It remained, then, to examine the chloride of potassium, or rather the mother-waters from which it had been extracted.

The mother-waters of salt marshes and sea water yielding decidedly negative results as to the presence of cæsium and rubidium, I was then naturally led to analyse the beet-root salt works and the mother waters proceeding from the treatment of vinegar for the extraction of alkalies. M. Lefebvre, of Corbehem (Pas de Calais), was kind enough to place at my disposal the materials required for these researches. I analysed the mother-waters and saline matter from his factory in exactly the same manner as above described with the residuum of saltpetre.

The following are the results of my analysis:—

Estimation of Rubidium.

Beet-root salt works: 1 kilogramme contained 1·87 grammes of chloride of rubidium.

Last mother-waters: 1 kilogramme contained 4.70 grammes of chloride of rubidium.

After this it seemed to me very probable that the rubidium found in considerable quantities in the residuum of saltpetre is due to the chloride of potassium extracted from beet-root.

This plant, it appears, may imbibe the new metal from the soil in quantity so minute as not to be detectable even by the optical process. What influence does the nature of the soil exert on this assimilation? is a question which future experiments may perhaps enable me to decide.—*Comptes-Rendus.*

TECHNICAL CHEMISTRY.

Chemistry and the Manufacture of Iron.

(*Continued from page* 228.)

No. II.—Fuel.

Three kinds of fuel are used in the manufacture of iron,—viz., charcoal, coal, and coke.

The use of charcoal is altogether discontinued in this country in the reduction of iron on a large scale, although it is well known that the very best results may be obtained with it. The insufficient supply is the sole reason why coal and coke have been substituted in its place.

The good results obtained when charcoal is used, is generally attributed to the potash it contains, which frequently amounts to as much as 20 per cent. of the ash. I do not know how far this opinion may be correct, but one would think that, at the temperature of the blast furnace, the potash must be volatilised; and this appears to be corroborated by the fact that in analyses of slags from charcoal iron, potash is seldom or never shown. The good results obtained with charcoal may rather, I think, be owing to its small per-centage of ash, and the freedom of this from large quantity of phosphorus or sulphur; its physical condition is also very suitable for combining instantly with the oxygen of the blast, as well as for allowing the iron as it melts down assimilating carbon.

Since the introduction of the hot blast in the manufacture of iron, coal has been largely used, and in some districts it bids fair to supersede coke altogether. The kind used is called "Anthracite," or stone-coal; it contains but a very small per cent. of bituminous or volatile matter, and does not cake when exposed to intense heat; on the contrary, it cracks or falls to a powder in some instances, and this is one of the greatest difficulties met with in its use. It ought not to contain more than 5 per cent. of ash and 1 per cent. of sulphur.

The bituminous matter acts no part in the changes going on in the reduction of iron, it being all driven off before this takes place; in fact, most writers on iron metallurgy very strongly urge the importance of coking the anthracite.

Coke.—Coke is yet so generally used in most districts that I must refer more largely to it than I have done to coal and charcoal. Of course, I have nothing to do with the manner in which it is prepared, my object being rather to explain the means by which an ironmaster may best tell what coke is suited to his purpose. To do this, attention must be paid, first, to its physical condition; second, its chemical constituents.

Most ironmasters having seen the ill-effects arising from the use of soft, "frizzy" coke, at once conclude that what they want is a coke of a very dense and hard character; a hard coke is certainly wanted to withstand the force of blast, as well as the pressure of the "burden;" but a hard coke need not necessarily be a dense one; on the contrary, it is quite possible for a coke to be exceedingly hard and yet porous and light; nay, other circumstances being equal, this is the kind of coke really wanted, for, as I hope to show in my next paper, a very dense coke defeats to a certain extent the effects of heating the blast. The porosity and hardness of coke can best (in my opinion) be judged of from its appearance. There are some who think the porosity is shown by getting the specific gravity of the coke, first, with the air contained in its pores; second, after the air has been expelled, and its place occupied by water,—the greater the difference between the two results thus obtained, and the greater the porosity. This holds good only in a limited number of instances; for a coke may have such large pores as to be unable to retain the air it contains in them on immersion in water, and thus it may show almost as high a specific gravity before expellation of air as it will after. The following results were obtained on three samples of coke; they may serve to illustrate this point:—

COKE.

Specific gravity, with air contained in pores.		Specific gravity, after air expelled from pores and its place occupied with water.	
Very dense . .	1·49	Very dense . .	1·80
Porous . . .	·87	Porous . . .	1·76
Very porous . .	1·30	Very porous . .	1·77

The chemical constituents of the coke (and coal) must also be attended to. One of the most deleterious ingredients found in coke is sulphur. This substance coke is never entirely free from, and some contain as much as 3·5 per cent. As a rule, if coke contains more than 1·50 per cent. of sulphur, it is deemed unfit for blast furnace purposes; it is refused sometimes when containing only 1·25 per cent.; in fact, every ·25 per cent. above 1·00 per cent. which the coke contains, renders it much more injurious than is generally thought; hence the importance of correctly determining the sulphur. I shall refer more particularly to the action of sulphur in the furnace, as well as its influence on the properties of iron, in the articles on "The Constituents of Cast Iron," and "The Chemical Changes taking place in the Furnace."

The per-centage of ash and its composition is also of importance; the coke that contains the most ash (apart from the question of sulphur) is not always the best,—the ash, by adding something to the furnace which the ironstone may already contain in excess, may act injuriously; for instance, an ash containing much alumina, when used with an aluminous ironstone, must necessitate an increase in the quantity of limestone to be used. Whereas such an ash would act very well with either a silicious or calcareous stone, a coke containing 9 per cent. of a calcareous ash has been found to give better results with an aluminous ironstone than one containing only 5 per cent. of aluminous ash. It is of importance, then, to get an analysis of the ash of each coke used.

(To be continued.)

On Aluminate of Soda and its Uses, by M. Paul Morin.

The principal substance employed in the manufacture of aluminate of soda is a kind of oxidised iron ore, containing much alumina and very little silica. This ore is found in France, in the Departments of Bouches-du-Rhone and Var, in a line between Tarascon and Toulon,

and it has been met with in Calabria and Senegal. The following is the composition of this ore, which varies according to its different localities:—Alumina, 60 to 75 per cent.; peroxide of iron, 12 to 20; silica, 1 to 3; water, in variable quantities. This is an unworkable iron ore.*

The ore is attackable by soda in various ways, of which the two principal are the attack by caustic soda and the attack by carbonate of soda.

In the first case, mix the pulverised ore in a sheet or cast-iron cauldron, with a concentrated lixivium of caustic soda; heat the mass, stirring it occasionally, and a solution of aluminate of soda is obtained by lixiviation. In the second case, mix the ore with a salt of soda, one of high per-centage being preferable; throw the whole on the floor of a reverberatory furnace, and heat to redness. When a sample of the mass under experiment effervesces no more with an acid, it is known that the carbonate of soda is decomposed and used in the formation of aluminate.

As in the preceding case, a specially-constructed filter is required to effect the lixiviation, in which the steam, at the same time, serves to heat the lye-water and to produce a vacuum, by condensing itself below the filtrating surface. Anhydrous aluminate of soda is obtained by evaporating the lye to dryness.

This product, in the form most convenient for industrial purposes, is a white powder, with a slight yellowish tint, dry to the touch, and, consequently, easily barreled for transport.† As employed commercially, its composition, in round numbers, consists of,—alumina, 48; soda, 43; sulphate of soda and chloride of sodium, 9. The formula, $2Al_2O_3.3NaO$, would require,—alumina, 52·79; soda, 47·21.

The sulphate and chloride proceed from the soda salt; no inconveniences attend the actual or probable employment of the product.

Aluminate of soda is infusible at the highest temperature of a reverberatory furnace; heated for a long time, it loses by volatilisation only the sulphates and chlorides which it retains as impurities. It is extremely soluble either in hot or cold water. Exposed to the air, the exterior absorbs moisture, and carbonates slightly. In this case some aluminate decomposes, and the solution contains alumina in suspension, but, even after a considerable time, the alteration is but slight.

Solutions of aluminate, when not too concentrated, remain limpid for a long time,—for instance, when they have not a density of more than from 10° to 12° Baumé's aeromètre. In concentrated solutions, on the contrary, a deposit of alumina is quickly formed, which attaches itself to the sides of the vessel in the form of mammillated concretions. In solutions of density from 35° to 40°, this deposit occupies the whole volume of the liquid, forming a cavernous mass, from which the liquid can be extracted by decantation. The solution in this case contains only a sub-aluminate with excess of soda, not possessing the properties of the normal aluminate. In fact, if attempted to be dried, it passes to igneous fusion, like caustic soda. It is extremely hygrometric and deliquescent. The alumina which deposits itself spontaneously in a concentrated solution of aluminate is dense, granulated, and rough to the touch, like sand.

Bicarbonate of soda precipitates the alumina from the aluminate, yielding to the soda of the latter salt its second equivalent of carbonic acid. With certain precautions, alumina free from carbonate of soda can be obtained by means of bicarbonate. Alumina precipitated by carbonic acid always retains, in spite of any number of washings, a certain quantity of carbonate of soda, probably in the state of insoluble double carbonate of soda and alumina.

In making aluminium, the alumina is precipitated by hydrochloric acid. After precipitation, it is dried and calcined, so as to produce anhydrous alumina, mixed with marine salt, useful in the preparation of double chloride of aluminium and sodium.

Alumina is also precipitated by sulphuric acid, an equivalent of which, in manufacture as well as in commerce, costs less than that of hydrochloric acid, and which is easily freed from iron, even when it has been manufactured from pyrites.

Ammoniacal salts, among others the carbonate, bicarbonate, and hydrochlorate of ammonia, precipitate alumina, yielding an ammoniacal liquid.

M. Chevreul has proved that aluminate of soda will act as a mordant for wool and silk. The tints obtained by this mordant differ more or less, according to the colouring matters, from those produced by alum. With the aluminate the reds verge to violet, the yellows to orange, and the violets to blue.

It may be advantageous, in some cases, to utilise this property of the aluminate, which it owes to its alkalinity; but to obtain the same tints, as with alum, it is necessary to transform the aluminate into an acid mordant.

Aluminate employed alone as a mordant acts on wools with great energy. To mordant with aluminate alone, we use only half the quantity necessary to represent the weight of alumina contained in the alum which would be employed in the same case; and under these conditions, the tones, if not superior, are at least equal to those obtained with alum.

In the preparation of lakes, the colouring matter being added to the solution of aluminate is precipitated by sulphuric acid. Lakes obtained in this way are generally richer in tone than those produced by alum, especially for yellow wood, which inclines to orange, and Brazil wood, which tends to violet. The shades of colour may be modified at will; the liquid remains alkaline, neutral, or acid, according to the quantity of acid employed.

The fact that salt of tin and aluminate, when in contact, reciprocally precipitate their bases, can, no doubt, be utilised, as shown by our experiments, in the preparation of lakes in which the salt of tin is or can be employed.

In the preparation of lakes aluminate of soda presents an anomaly which has been with difficulty resolved. Lakes from aluminate are very difficult to precipitate, whether the operation is performed by heat or cold, or whether the colouring matter is added to the acid or aluminate before precipitation. However, the difficulty has been met in the following manner:—Suppose three litres of water are required to wash a certain quantity of lake, the lake is precipitated in one volume of it only. When the lake is formed and properly agitated, add the remaining two litres of water, and agitate again. The deposit takes place, and occupies only a third of the whole volume, like alum lake.—*Répertoire de Chemie.*

* Besides this, M. Henri Sainte Claire Deville has pointed out the presence, in very small quantities, of vanadium, chrome, and perhaps of tungsten. These metals are no obstacle to the extraction of the alumina, and do not interfere with the applications of the aluminate.

† This product is manufactured by M. Henry Merle and Co., by the processes of MM. Le Chatelier, H. Sainte Claire Deville, and Jaquemart, with the assistance of MM. Meissonier (mining engineer) and Paul Morin.

Contributions towards a Knowledge of the Inorganic Constituents of Plants, by Dr. Charles A. Cameron, *M.R.I.A., F.C.S.L, Corresponding Member of the Agricultural Societies of New York, Belgium, &c.*

(*Continued from page* 284.)

Experiments with Lithium.

By submitting various substances to the admirable method of photo-chemical analysis, to which I have so repeatedly adverted, MM. Bunsen and Kirchhoff discovered two new elements resembling the metals of the alkalies, and frequently associated with them. These elements have been termed cæsium and rubidium. By the same means Crookes more recently discovered another new element resembling sulphur, to which the name thallium has been given by its discoverer. By means of the spectrum analyser Bunsen and Kirchhoff also ascertained that the metal lithium, instead of being limited to a few localities, as had previously been supposed, was almost as widely diffused as the common alkaline metals which it so much resembles. These chemists and their pupils found lithium in the mineral springs of Germany and of France; in the waters of the Dead Sea, of the Mediterranean, and of the Atlantic; and in a large number of earthy and saline substances. The wide-spread occurrence of lithium, the alkaline nature of its oxide, and the solubility of its compounds, at once suggested its probable occurrence in plants. It was accordingly sought for, and found in the ashes of sea-weeds, of the vine, of tobacco, and of all the plants grown on the granitic soils of the district of Oldenwald, in Germany. It was also found in the milk of animals; and Folwarcz detected it in the ashes of the blood, and of the muscular tissue of man.

Immediately after the publication of these remarkable investigations, I suggested, in a letter published in the London *Gardeners' Chronicle*,* that the failure of our crops might, in certain cases, be owing to the absence from the soil of some element, such, for instance, as lithium or cæsium, which existing originally in the soil, but in minute quantity, had been abstracted by preceding crops without an equivalent proportion being restored in the form of manure. I also threw out the suggestion that the high fertilising properties of sea-weeds as a manure for potatoes might be due to their containing lithium, as I had found that element in the *Fucus vesiculosus*, and other species of fucus, grown upon the coast of the County of Dublin. I further stated that it was my intention to try by experiment whether or not lithium was an indispensable constituent of plants. Some months after the publication of this paper, the Prince of Salm-Hortsmar communicated to a German journal† the results of some experiments made by him to determine the influence of lithium on fructification. The Prince's experiments appear to have been conducted on a very small scale. A few barley seeds were sown in coarsely powdered felspar, and supplied with all the known mineral constituents of plants: feeble shoots were sent forth, which perished without flowering, but on the addition of a trace of nitrate of lithium ($\frac{1}{100}$th of a milligramme) one of the stems produced perfectly matured seeds. From this experiment the Prince concludes that lithium is indispensable to the development of grain.

The results of my experiments with lithium are not in accordance with those of the Prince of Salm-Hortsmar, and they appear to me to prove that the absence of this element from certain soils is not, to what I had suggested, the cause of their infertility, or of the failure of particular kinds of crops on them. I shall describe these experiments as briefly as possible:—

1st. The ashes of the plants grown in the vessels of group *c* were carefully examined by the spectrum analyser for lithium, but not the slightest trace of that metal was discovered.

2nd. In the ashes of twenty-four specimens of wheaten flour, the produce of various parts of the world, lithium was present in seventeen instances. Of twelve specimens of Irish barley four contained no lithium. Three samples of Irish oats yielded negative results. All the fuci examined — embracing two specimens of *Fucus vesiculosus*, two of *Fucus serratus*, and one of *Fucus nodosus*—contained traces of lithium. J. W. Mallet, in a paper read before the Geological Society of Dublin, in the year 1849, stated that oxide of lithium existed in a weighable amount in killinite, a mineral found at Killiney Hill and Dalkey Hill, in the County of Dublin. Galbraith, however, who has more recently analysed two specimens of this mineral, failed to detect in them the slightest trace of lithium. I have sought for this metal in the ashes of three kinds of plants grown on Dalkey Hill and Killiney Hill, but did not recognise it in any of them.

3rd. Barley seeds were sown in soil composed of quartz, alumina, and sulphate of barium, and contained in four vessels. Those in No. 1 vessel were manured with a compound made up of all the substances supposed to be essential to the growth of plants; and, in addition, a proportion of salts of lithium, equal to the amount of potassium salts contained in the artificial manure. No. 2 vessel was supplied with a compound identical in composition with that placed in No. 1 vessel, with the exception that it contained no potassium. No. 3 vessel was supplied with all the mineral constituents of plants, excepting sodium, the place of which was occupied by lithium. The manure in No. 4 vessel contained only a minute quantity of potassium, but a considerable amount of the nitrate, carbonate, and phosphate of lithium. The seeds in No. 1 vessel germinated and produced healthy plants, which in due season fructified and developed grain. The plants were burned, and their ashes, on being submitted to analysis, afforded the following results:—

	Potash.	Soda.	Lithia.
Seed . . .	14·28	2·84	1·66
Straw . . .	10·70	2·58	2·00

The seeds sown in No. 2 vessel germinated and produced shoots, which were sickly, stunted, and speedily perished.

The seeds sown in No. 3 vessel germinated and produced healthy shoots, which flowered and developed seed. The plants were burned, and their ashes examined with the following results:—

	Potash.	Lithia.
Seed	15·17	1·64
Straw	8·90	3·12

In this case the lithium appears to have acted as a substitute for sodium in the vegetal organism.

An accident happened to vessel No. 4, which vitiated the results of the experiment, and prevented me from determining to what extent lithium could be substituted for potassium.

The results of these experiments lead me to conclude:—

1st. That lithium, if present in the soil, is likely to be taken up into the organisms of plants.

* "Problems in Agricultural Chemistry." *Gardeners' Chronicle* for March 29, 1861. It also appeared in the *Agricultural Review*, and in the *Farmer's Gazette*.

† *Annalen der Physik und Chem.*, vol, cxl. p. 644. 1861.

2nd. That it is capable of being substituted for sodium, and probably, to a limited extent, for potassium, in plants.

3rd. That it is neither useful nor essential to vegetables, being wholly absent from specimens of cultivated plants of luxuriant growth, and from soil on which barley had been perfectly developed.

Experiments with Aluminium.

Aluminium, the most abundant component of soils, is one of the rarest constituents of plants. Berzelius, I believe, was the first to point out its occurrence in vegetal ashes. Sprengel states that alumina is present in almost every plant; and, according to Wittstein, it occurs in almost all garden shrubs. Grafen found it in weighable proportion in the ashes of *Lycopodium denticulatum*. The Prince of Salm-Hortsmar found 38·5 per cent. of alumina in the ashes of *Lycopodium complanatum*, minute quantities in *Juniperus communis* and *Erica vulgaris*, and none in the needles of *Pinus sylvestris*. Ratthausen states that alumina makes up 39·07 per cent. of the weight of the ashes of *Lycopodium chamacy parizsus*. The investigations of Knop and Erdmann throw great doubt on the accuracy of Sprengel and Wittstein's analyses. Erdmann examined a great variety of seeds and woods, thoroughly freed from adhering earthy matter, without finding in a single instance the slightest trace of alumina. The Prince of Salm-Hortsmar endeavours to account for the large proportion of alumina in *Lycopodium complanatum*, and the small amount of that substance in *Juniperus communis*, by the acid reaction of the roots of the former—a property not possessed by the roots of the latter. The Prince is silent on the subject of the preparation of his specimens, which it is possible, nay probable, were not altogether freed from mechanically adhering soil. Altogether it is very doubtful whether or not the alumina which has been found in the ashes of plants really existed in the vegetal structure. The results of the experiments which I shall now describe are offered as a small contribution towards the solution of this question.

Four specimens of *Lycopodium clavatum* (common club-moss) were so thoroughly washed with distilled water that, on being subsequently examined by means of a lens, they were found perfectly free from earthy matter. The plants were burned, and their ashes examined. In three of the specimens no alumina was present; in the fourth, it existed to the extent of 1·25 per cent. Six specimens of "vegetable sulphur" (a yellow powder furnished by *L. clavatum*, and employed in pyrotechny) were next submitted to examination. In one of the specimens ·130 per cent. of alumina was found (in the ashes), in another a scarcely weighable quantity, and in the rest not a trace.

Onions, spearmint, and American cow-grass were watered daily, for some weeks, with solutions of nitrate and sulphate of aluminium. They were subsequently burned, and their ashes afforded, on analysis, the following results:—

Plants.		Alumina.		
Onions	0·14	Trace	None	None
Spearmint	0·52	0·50	0·45	Trace
American cow-grass ..	0·75	0·60	0·60	0·25

There were four analyses of each kind of plant made. In the case of the onion, two whole plants were operated upon in each analysis. The other plants being too small to admit of individual examination, specimens (of about six ounces weight) of them were selected from different parts of the bed. The application of the salts of aluminium to the plants was discontinued four days previous to their removal from the soils. The plants were grown in the open air. Before incineration they were carefully cleaned.

The results of the experiments of Knop, Erdmann, and my own, appear to me to render highly probable the following conclusions:—

1st. That alumina is not an essential constituent o plants.

2nd. That it may be absorbed without injury by plants

3rd. That it is very rarely present in vegetables.

It is my intention to make further experiments in relation to this subject, with the object of determining the following points:—

Firstly, whether alumina, being isomorphous with the sesquioxide of iron, is capable of replacing that base or not.

Secondly, whether the extreme insolubility of the compounds of aluminium—phosphate and silicate—present in the soil is, or is not, the cause of the rare occurrence of that element in plants.‡

PHARMACY, TOXICOLOGY, &c.

On Ferri Subcarbonas, by T. A. Rex.

This is one of the most important of the numerous iron preparations, and its use as a remedial agent is very general. The U. S. Pharmacopœia directs it to be employed in making the *Tinctura Ferri Chloridi*, which is now extensively used in medicine.

That authority orders, in preparing this tincture, that 6 oz. of subcarbonate of iron, one pint of muriatic acid, and three pints of alcohol be taken. The subcarbonate is directed to be dissolved in the acid, and when effervescence has ceased, the action is to be aided with a gentle heat until all the salt is dissolved. The tincture is then completed by filtration and the addition of the alcohol.

As a large proportion of the subcarbonate of iron found in commerce will not dissolve freely when so treated, thus causing the tincture of the shops in many cases to be a very uncertain preparation, the following experiments were instituted to find which mode of conducting the precipitation and drying would render the precipitated powder freely soluble in muriatic acid.

The results of these experiments having been inspected by my preceptor, Mr. Edward Parrish, they are the more freely offered in a thesis.

Experiment 1.—A saturated solution of sulphate of iron, containing one part by weight of iron to two parts of cold water, was mixed with a saturated solution of carbonate of soda of the same proportions, both solutions being cold. The resulting precipitate of protocarbonate of iron was thoroughly washed with warm water, particular care being taken in the washing to remove all the soluble salts. It was then dried between folds of bibulous paper at different temperatures; a portion at the ordinary temperature of 60° F., a second quantity at a sand bath heat of 160° to 175° F., and a third at a boiling water bath heat.

When these different specimens of subcarbonate were thoroughly dried, they were reduced to powder and treated, 45 grs. of each with 132 grs. of muriatic acid (which is the proportion for the tincture directed in the

‡ According to Brande, there is a compound of alumina with water, which is slightly soluble, but I doubt very much the existence in considerable amount of such a hydrate in soils.

U. S. Pharmacopœia), and gently heated. The acid was found to be of officinal strength, specific gravity 1·116.

All the specimens were soluble to some extent, but differed greatly in their respective degrees of solubility.

The portion dried at the temperature of 60° F. was much the most soluble; next in this respect was that dried at the sand bath heat, while the water bath temperature had rendered the powder still more insoluble.

To determine their proportionate solubilities definitely, tinctures were made by adding to each solution 285 grs. of alcohol, which was found to be the proportion ordered in the Pharmacopœia. These tinctures were thrown upon accurately weighed filters, and the insoluble portions carefully washed with alcohol. The filters were then dried, and when weighed with the precipitates the resulting insoluble portions from the quantities used determined to be—

Residue of specimen dried at	60° F.	3·2 grs.
,, ,, ,,	160° to 175° F.	8·3 grs.
,, ,, ,,	water bath	9·4 grs.

Experiment 2.—Saturated solutions of sulphate of iron and carbonate of soda, in the same proportion as before, were mixed at a temperature of 150° F. The resulting precipitate, as in the former case, was washed carefully, and dried between folds of paper at the different temperatures of 60° F., sand bath heat of 160° to 175° F., and the water bath heat.

When dried and powdered, the different specimens were treated with muriatic acid in the proportion of 45 grs. of the carbonate to 132 grs. of acid, and exhibited generally a better degree of solubility than in the former instance. The different modes of drying, however, modified the solubilities of the powder, the portion dried at the lowest temperature being the most soluble.

Tinctures were made of each solution by the addition of 285 grs. of alcohol, as before, and the same mode of procedure resorted to for obtaining the insoluble residues, resulting as follows:—

Residue of specimen dried at	60° F.	1·85 grs.
,, ,, ,,	160° to 175° F.	6·25 grs.
,, ,, ,,	water bath	8·9 grs.

Experiment 3.—A dilute solution of sulphate of iron, using 2 oz. to one pint of water, was mixed with a solution of carbonate of soda of the same proportion, both solutions being cold.

The same precautions with regard to washing the precipitate were observed, successive portions of warm water being used, until no soluble saline matters could be detected in the washings.

The protocarbonate was subjected to the three different degrees of temperature, 60° F., sand bath 160° to 175° F., and water bath, and when dried and reduced to powder was, as in the former experiments, treated 45 grs. of each with 132 grs. of muriatic acid. In comparing the respective solubilities of these portions, it was found, as before, that a high temperature rendered the subcarbonate more insoluble.

When alcohol was added to each solution in the proportion before used, and the insoluble residue obtained as in the previous instances, the weight in grains was found to be—

Residue of specimen dried at	60° F.	1 gr.
,, ,, ,,	160° to 175° F.	6·65 grs.
,, ,, ,,	water bath	8·5 grs.

Experiment 4.—Diluted solutions of sulphate of iron and carbonate of soda, in the proportion of 2 oz. to one pint were mixed hot, at the temperature of about 200° F., and the resulting precipitate washed and dried at the three different temperatures before named.

When treated with the same proportion of acid used in the former experiments, this mode of preparation, which accords with the direction of the U. S. Pharmacopœia, was found to furnish a very soluble precipitate; yet, as in the other instances, a high temperature used in drying it rendered the subcarbonate less soluble.

To these solutions alcohol was added in the requisite proportion, filtered and washed, and the resulting precipitates or insoluble portions determined as follows:—

Residue of specimen dried at	60° F.	1 gr.
,, ,, ,,	160° to 175° F.	3 grs.
,, ,, ,,	water bath	5·7 grs.

To facilitate a comparison of the results of these experiments, a table of the solubilities of the several precipitates is appended, the insoluble residues being deducted from the weight used, and the result calculated into per-centages.

SOLUBILITIES OF PRECIPITATES.

Dried at	Exp. 1.	Exp. 2.	Exp. 3.	Exp. 4.
60° F.	93 per ct.	96 per ct.	98 per ct.	98 per ct.
,, 160° to 175° F.	81 ,,	86 ,,	85 ,,	93 ,,
,, water bath	79 ,,	80 ,,	81 ,,	87 ,,

Experiment 5.—As in all the above experiments the most soluble specimen of subcarbonate, by treatment with muriatic acid, still deposited a small proportion of insoluble matter, another lot of the powder was made. In this the Pharmacopœia directions as to quantity were taken:—2 oz. of sulphate of iron and 2¼ oz. of carbonate of soda dissolved each in a pint of boiling water. The solutions were mixed at a temperature of 200° F. The resulting protocarbonate of iron was washed thoroughly in a precipitating vessel with warm water, the supernatant liquid at each washing being drawn off by means of a syphon, until the washings gave no precipitate with a solution of nitrate of baryta. Washing by decantation was found to be much preferable to washing on a filter.

A portion of this precipitate was wrapped in bibulous paper and dried on a sand bath, the temperature carefully kept at 80° F.; another quantity at the same temperature, but thinly spread on paper uncovered, so as to expose a large surface to the action of the air; and still another portion subjected to a sand bath heat of 180° to 190° F.

When these specimens were thoroughly dried and pulverised, they were found to differ materially in colour. The low temperature gave the precipitated carbonate a light chocolate brown shade, and the high heat produced a reddish-brown approaching that of Spanish brown of commerce; both differed materially from the article usually found in the shops.

These were all treated with muriatic acid, 45 grs. of each with 132 grs. of acid, and gently heated. The subcarbonate dried at 80° F. dissolved with much effervescence, and was perfectly soluble, producing a clear solution, without a particle of cloudiness. No difference of solubility or colour was observed between the two specimens dried under different circumstances of exposure at 80° F.

The specimen dried at 180° to 190° F. was as imperfectly soluble in acid as those dried by sand bath heat in the other instances.

While experimenting with these different modes of preparation, there seemed to be a loss of weight when a high heat was used in the drying. To determine more accurately this loss, 45 grs. of the precipitated (moist) protocarbonate of iron last described was dried at a temperature of 80° F., and the same quantity of the moist precipitate dried at a sand bath heat of 180° to 190° F. When thoroughly dried the portion subjected

to the gentle heat weighed 15 grs. and the other only 10·5 grs.

This difference of 4·5 grs. may be attributed to loss of water of hydration and carbonic acid, which, when present in the salt, dried at a gentle heat, has a tendency to promote its ready solubility in muriatic acid.

From these various modes of preparing and drying subcarbonate of iron, it would seem that dilute solutions mixed hot, and the resulting precipitates dried at a temperature not exceeding 80° F., makes a salt freely soluble in muriatic acid. A heat higher than this in every case rendered the subcarbonate less soluble.

From the appearance of the precipitated carbonate of iron generally found in commerce, it is not unlikely that a large proportion of it is made without regard to the officinal directions, and probably dried at high temperatures. Sometimes, no doubt, the soluble salt of iron used contains impurities, which remain in the preparation. With reference to the preparation of *Tinctura Ferri Chloridi* from precipitated carbonate of iron, there seems to be no difficulty if the officinal directions are observed, both in the preparation of the subcarbonate and the tincture. A gentle heat is described in our Pharmacopœia as between 90° and 100° F., and this is the temperature indicated for the drying of the precipitated carbonate of iron.

The London College does not indicate any temperature; the Edinburgh and Dublin indicate higher temperatures than are adapted to produce a soluble preparation.—*American Journal of Pharmacy.*

PHYSICAL SCIENCE.

On Adjusting the Needles of a Galvanometer, by J. Broughton, *Esq.*

In making or adjusting an astatic galvanometer, the difficulty of magnetising two needles to exactly the same intensity frequently causes the expenditure of much time and trouble, which may be greatly reduced by the following simple method of procedure:—

Let the two needles be as nearly as possible of the same dimensions; a piece of watch-spring will make a good pair for a large instrument. Then, after strongly magnetising them, suspend each singly by a piece of fine silk, and producing a definite deflection by means of a magnet, allow the needle to return to rest, counting the number of oscillations it makes in a constant time (say 1½ minutes). The needle that makes the greater number will be the more highly magnetised, and will probably require, as the case may be, a very slight touch with the similar pole of a magnet in order to reduce its magnetism to the intensity of the other needle, which can be readily managed after one or two trials in the above manner. When the two needles are subsequently superposed with opposed poles, and finely suspended with the lower one between the coil, the pair will be found to retain but little polarity. The needle whose directive force still preponderates can then be readily detected,—should it not already be determined by the position of the pair,—by carefully holding at the distance of about two feet from one end of the pair and equidistant from each the north pole of a magnet, and noticing whether the two nearest ends are attracted or repelled. Should the former be the case, it will then be the needle whose south pole is nearest the magnet whose magnetism is still in excess, which must be slightly demagnetised by holding for a second the north pole of a magnet at the distance of two or three inches from that of the needle. By proceeding in this manner and by noticing the deflections produced by a weak current, as that of a single thermo-electric pair, the needles may be quickly rendered astatic, which will be indicated by the extreme slowness of their oscillations and by resting equatorially.

INTERNATIONAL EXHIBITION.

Chemical Substances and Products, and Pharmaceutical Processes.

(Continued from page 287.)

Let us at once with pleasure record the fact that the general appearance of the Exhibition is now greatly improved. Minton's grand and beautiful majolica fountain, under the eastern cupola, is now finished, equalling, perhaps surpassing, in attraction the crystal fountain of 1851. In the production of ornamental pottery our country has made immense progress within the last few years; and it will not be long, we venture to predict, before English pottery is as famous as that of Dresden and Sêvres. Mr. Minton's fountain is perhaps the largest work of this kind which has been executed in Europe, and its production will mark an epoch in the history of this department of art in our country. Seeing how largely chemistry contributes to the art, a chemist must regard this work with as much pride as the artist and the potter.

Let us also observe with satisfaction that the labels in the Pharmaceutical collection are being revised by an intelligent gentleman, who would do well to put English as well as correct Latin labels throughout; and let us candidly confess, that we may have done a few gentlemen some injustice when we said that the leading members of the Society had not contributed to that collection. A closer inspection reveals that the President has sent some small bottles of chemicals, that Mr. Morson has contributed some very good gum resins and various specimens of opium, and Messrs. Hanbury some bottles of distilled waters, looking very genuine and very turbid. Some remarkably perfect crystals of alums contributed to the collection by Mr. Brewer ought not to be passed over without notice.

In case 666, Mr. Squire exhibits beautiful samples of salts of the alkaloids of opium and nux vomica, and particularly fine specimens of piperine, caffeine, and benzoate of ammonia. There are also specimens of some of the rarer neutral principles, arbutin, coumarin, &c., so white that they might all be starch, and the spectator none the wiser. Outside the case are some fine extracts, which the visitor is at liberty to examine. Among them we noticed some extract of hemlock of a remarkable colour, but with scarcely any odour of the plant. Messrs. Savory and Moore, 595, exhibit specimens of cod-liver oil, and also Dugong oil from Ceylon. The latter oil was introduced as a substitute for cod-liver oil, but has never come much into use, and great doubts have been thrown (See Chemical News, vol. i. p. 87) on the character of many samples received in this country. The genuineness of this sample, however, is guaranteed by Dr. Macgregor Croft, and Messrs. Savory should give an account of its reactions. The case, among other things, contains a beautiful specimen of santonine, of a bright yellow colour, some fluid extracts, with specimens of hypophosphites, and lithia preparations, which make up a good display by this firm. Mr. Keating exhibits a collection of medicinal plants, with queer

names, from the West Coast of Africa. The names of them, at all events, are quite new to us, and we should like to know something of the botanical characters and therapeutical effects of the plants. Mr. Keating had better give specimens of them to Professor Bentley, or publish a full account of them himself. The next case, of Mr. Holland (655), must not be passed without a word of commendation. His well-known reputation as a grower of medicinal plants and distiller of essential oils is well supported by the appearance of the articles in this case. English rhubarb looks well in the specimens of Mr. Usher, 688. In size and colour they might well pass for East Indian, and artfully trimmed, would sell for any sort. There is no doubt that rhubarb of English growth is quite equal in its effects to that of any country, and it ought to reward a grower who takes pains in its cultivation. Mr. Ransome's case, 665, contains some very good-looking extracts, bright essential oils, and an unusually large scammony root.

Whilst speaking of extracts and dried plants, we may direct our readers' attention to some specimens in the French division. In Case 145 they will see extracts of bark and liquorice, prepared *in vacuo*, of a remarkably light colour. We should suspect that the decoctions had been treated with animal charcoal, although the labels do not say so. The samples of dried hemlock and violets, in the same case, have the colour remarkably well preserved. In future papers we shall further contrast the foreign productions with our own, and now return to the magnificent case of the Messrs. Howard, 532. This case fully vindicates the reputation of the exhibitors as manufacturers of pharmaceutical chemicals. For cleanness and finish the salts of the alkaloids shown have never been surpassed. These are all, we presume, commercial samples; but the case contains, as well, some *fancy* specimens, particularly some very large crystals of chlorate of potash, and some very fine carbonate of soda. Nor will the visitor overlook the specimens of calomel in three conditions. The articles are too much crowded together for the spectator to get a good view of each, but he will see enough to convince him that it is the first of its kind in the department. In the case of Messrs. Hopkin and Williams, 530, there are also admirable commercial specimens of pyrogallic acid, cyanide of potassium, caffeine, aconitine, and the scale preparations for which they are famous. Among these latter is a lithio-citrate of iron, which looks very well. Lithia salts are now in request as medicines, and Messrs. Hopkin and Williams have here several preparations to meet the demand. In Case 533 Mr. Hulle shows some clean and well-crystallised salts of strychnia and quinine.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Six Lectures on Some of the Chemical Arts, with Reference to their Progress between the Two Great Exhibitions of 1851 and 1862, by Dr. LYON PLAYFAIR, *C.B., F.R.S., Professor of Chemistry in the University of Edinburgh.*

LECTURE I. (*Thursday, May 8, 1862.*)

Distillation of Coal; especially in its Relation to the Production of Gas and to the Formation of Paraffin.

LADIES AND GENTLEMEN,—In ancient Jewish times, the son of Sirach, in Ecclesiasticus xxxix. 26, has the following passage:—"The principal things for the whole use of a man's life are water, fire, iron, and salt, flower of wheat, honey, milk, and the blood of the grape, oil, and clothing." If it were not that chemistry has advanced so much in recent years that this definition of the son of Sirach no longer represents the comforts and enjoyments requisite for our advanced civilisation, I would show you that each of these substances, either in their actual production or in their improvements, depend upon the chemical arts; but it is the very progress of chemistry which has rendered this definition of little significance to us at the present time. No science has done so much as chemistry for penetrating into the secrets of nature and discovering those applications in the arts which are necessary for our daily comforts. It would be impossible for me to attempt in the course of six lectures to go over the range of the chemical arts. All that I can do is to take a few selected examples, and to refer to those chiefly with a view of showing you their progress between the Exhibition of 1851 and the Exhibition of the present year. But it would be contrary to the usual habits of this Institution, were I to confine myself simply to an explanation of the progress of these arts, for I cannot assume that the audience whom I have the honour to address are perfectly familiar with the arts themselves, and, therefore, can be interested by a mere account of their progress. I therefore propose, with your permission, to describe to you, generally, the nature of these arts in a popular manner, and then to refer to the discoveries which have recently taken place with regard to them.

When we examine the application of science to industry, we find that these applications benefit industry generally in one of three ways—first, by adding to human power, either by furnishing substitutes for brute labour, or by affording tools and methods for results formerly difficult or impossible, as, for example, when we use gunpowder in blasting rocks, and in the course of a few minutes can perform what would require ages of brute labour. It very often happens in this application of science to diminishing the efforts of brute labour that natural forces are employed, as, for instance, when Hercules was obliged to clear out the Augean stables, not by using a pitchfork, but by turning the waters of the Alphæus into it and sweeping it out by the use of this natural force. The second method in which science benefits industry is by producing economy in the time necessary to attain the result. Historians record as an example of wonderful dispatch the feat of Sir Robert Carey, who rode from London to Edinburgh to tell James the First that he was the King of England by the death of Elizabeth, and who occupied, in his weary ride, from Thursday to Saturday night. By means of the electric telegraph we can now despatch a message to the northern capital in much less time than the groom of Sir Robert Carey could have saddled his horse. The third advantage which science renders to industry is in the economy of material. There is nothing so characteristic of chemical improvement as the uses which it makes of waste products. A philosopher justly defined dirt as being merely matter in a wrong place. Put it in the right place, and it becomes a utility. The substances apparently the most worthless to-day are even elegant utilities to-morrow. For instance, the parings of horses' hoofs, the horns of cattle, and cast-off woollen garments of the inhabitants of the sister isle, are mixed with scraps of old iron, and grace the dress of the courtly dames who wear dresses dyed with Prussian blue. All these substances when properly applied become important utilities.

In all the manufactures to which I shall have to draw your attention in this short course of lectures, you will find that one or other of these three forms of benefiting industry is very apparent, sometimes appearing altogether, sometimes offering only one of the advantages.

I select to-day the manufacture of gas, not that that manufacture has made any decided progress during the last ten years as a chemical art,—it has made very little

progress,—but its economy is better understood, and its science is also better comprehended. But I select it because I shall show you in the next lecture that the materials which were extremely waste and noxious in the manufacture of gas have all been utilised—at least, most of them have already been utilised; a few of them remain still to be utilised—and that they have produced important, common, and even elegant utilities. I, therefore, commence to-day by giving you an exposition of the manufacture of gas.

No great discovery in the industrial arts starts into the world without giving abundant indications of its approach. There is no such thing as an industrial invention starting up full grown and panoplied in armour as Minerva did from the brain of Jupiter: it creeps into the world by slow degrees, and by many indications fortells that it is approaching. So with the manufacture of gas. The Persians, ages since, saw gases coming through the soil; they set fire to them and worshipped them as holy fire. In our own country gases constantly came from the coal measures below the surface of the earth; and Shirley describes, in 1659, how he obtained these gases and set fire to them, and how illuminating they were. Sir James Lowther, in 1733, actually took the gases coming from the coal measures and conducted them away by pipes, and he burnt them at the surface and showed that they were combustible and highly illuminative. Clayton, the Dean of Kildare, in 1739, distilled coal in a retort, and he got what he calls "a phlegm, a black oil, and an incondensible spirit." This spirit is what we now know as gas. He could not in any way condense this spirit, so he collected it in bladders, and he pricked holes in the bladders and lighted the issuing gas for the amusement of his friends. Lord Dundonald did nearly the same thing on a much larger scale in 1786; but it was Murdoch, in 1792, who gathered up these isolated threads of science and spun them into a rope capable of supporting an industry. He showed how the knowledge previously acquired might be applied in the preparation of gas, and how that gas might be used for the purposes of illumination. It was not, however, till 1812 that this industrial art took root in London, and then so little was known about it that it is recorded that a lady of fashion, seeing a brilliant gas-lamp burning at Ackerman's shop in the Strand, insisted upon taking it away, light and all, in her carriage. It was no wonder that at that time gas did not become very popular; it was extremely bad; it had a nasty fetid odour resembling that of rotten eggs; it produced intolerable smoke when burning; it discoloured cottons, from the sulphurous acid which was formed in its combustion; it eat off the backs of books, and it produced languor and headache. Luckily it had an advocate in Winsor, who thought that all these defects were advantages, and who inspired the public with his own enthusiasm, and who gradually extended the art in spite of these disadvantages; but, what was much more important, it had a practical man of science in Clegg, who admitted the defects, and immediately applied his great skill to remove them; and it is owing to the great labours of Clegg in improving the manufacture of gas that we possess its excellence at the present day.

I need scarcely demand your attention to this as an important subject, because it must be quite obvious to you what an enormous advance civilisation took when the coal gas became cheap and common. It has improved morality and it has lessened crime, by making every passer-by in the street a detective policeman. The art of street lighting was discovered as early as the fourth century, when Antioch was lit up by oil-lamps; and even at that time—as early as the fourth century—the art of street-lighting showed its advantages by ending unseemly brawls. We read at that time of a controversy between a Luciferan and an orthodox, which continued in the midst of a crowd of people till the lamps were lit, when the disputants spat in each other's faces and retired. From the fourth century up to the fifteenth or, perhaps, the sixteenth century, the art became lost; and it was only in the sixteenth century that streets began again to be lighted by oil lamps. Now we have from thirty to forty thousand of these street lamps in London alone.

I now ask your attention a little to the chemistry of coal gas.

Coal gas is obtained by the distillation of common coal, or by the distillation of coals highly bituminous, as they are called,—that is, which give out bituminous products in their distillation. Coal consists of carbon and hydrogen largely, of oxygen and nitrogen in smaller quantity, of sulphur a slight quantity also, and of incombustible matter and ashes.

When the coal is heated in a close vessel,—that is, when it is distilled,—at first the oxygen contained in that coal acts just as the oxygen that is in that fire: it burns some of the constituents of the coal. It unites with the hydrogen of the coal and forms water. Just as the oxygen there [referring to a small portable furnace burning on the lecture table] unites with the hydrogen of the coal and produces water, so does it in the close retort. It unites also with part of the carbon and forms carbonic acid, as it does in the open fire-place. But soon the oxygen of the coal is exhausted, and then the hydrogen, which is in large quantity, divides itself partly between the carbon and produces the illuminating gases and the liquids which I shall afterwards describe, and which are called hydro-carbons; and the rest of the hydrogen goes to the nitrogen and it forms ammonia, and this ammonia we shall afterwards have to deal with also.

We will now take a simple case, which is an exact model of the manufacture of coal gas on a large scale. I have here an arrangement which will take a little time to act, and therefore I set it going just now. We have put coal in that retort, and it is connected with a receiver here. You will find, after a short time, that a liquid, which consists partly of water and partly of tar, comes over into the condenser. Our large bottle there is the condenser which we employ for this purpose. It afterwards passes through a case here, which I shall afterwards describe to you, which is called the purifier; and after that we shall be able to collect the gas; but at the present moment I simply wish to start it.

When coal is distilled it produces several substances which are described in that diagram. It produces, first, gaseous products; second, crude coal oil, which is commonly known by the name of tar; and it produces also a watery portion which contains ammoniacal salts. To-day we have nothing to do with the crude coal oil, or with the watery portion: that will be the subject for the next lecture. All that I have to direct your attention to now are the gaseous products; and these are divided into three divisions. The first class of gaseous substances produced by the distillation of coal consists of hydrogen, carburetted hydrogen, and carbonic oxide; and those are classed as diluents, for a reason which you will presently see. Then there are, secondly, illuminants, which consist of olefiant gas, and two substances called propylene and butylene. And, thirdly, there are impurities which are also gaseous, which are carbonic acid, sulphuretted hydrogen, and nitrogen. We can readily show the differences in the nature of these gases. I have collected these gases in a separate state here. I first have got hydrogen, which is one of the substances classed as diluents; and I will take these different gases and show you that they have very different properties as regards the purpose of illumination. I can easily pass a little hydrogen through here, and then you will be able to see, when we get the draught sufficiently, the amount of light it is capable of affording. We will just allow a little gas to pass through first before we ignite it. Now, you see that there is very little illumination there; in fact, the little illumination which is produced there is

derived from the dust and the material upon the burner. Now, we will take carbonic oxide, which is another of these gases, and you will find that there is very little illumination here too. In this case we are obliged to burn it without a burner. You see there we have produced only a blue flame, without much illumination. Now, I have got here some marsh gas, or carburetted hydrogen, and you will find that this, also, is not much of an illuminant. They are all classed there as diluents. I have now here olefiant gas, which is a strongly illuminating gas. You see there that olefiant gas is very illuminating. I have here common coal gas, and you will see the great differences between these different gases. You see that coal gas, which contains all these gases—diluents as well as illuminants—is not so illuminating as olefiant gas. In that way you can compare the value of these gases as they are placed on that table.

Now, when we examine what is the chemical nature of these gases, we soon get an explanation of the reason of their different illuminating values. The amount of carbon contained in olefiant gas is 86 per cent., the amount of carbon in carburetted hydrogen is 75 per cent., and the amount of carbon in carbonic oxide, which burns with that blue flame, is only 43 per cent. Therefore, the illuminating properties of the gases are in proportion to the amount of carbon which they contain. I think that I can show you this by a simple experiment. I have here hydrogen,—a gas which I showed you before. You have seen that it burns with very little flame. I should tell you that the reason that it produces very little flame, is that it produces a gaseous substance in burning. In the act of burning it forms water. Now, gases require a very high temperature before they are luminous, and the products of the combustion of hydrogen is steam; and that product requires such a high temperature to become luminous, that hydrogen is only faintly so. But solids, on the other hand, become luminous in the dark at 700 degrees, and brightly so in daylight at from 1000 to 2000 degrees. Now, I want you to understand the nature of the experiment I am going to make, before I put it in operation. I have here hydrogen, burning as you saw before, and producing simply steam, and therefore not producing much light, because that product is gaseous. I will now turn that off, and I will produce the combustion in this tube with peroxide of barium. The peroxide of barium will supply the flame with oxygen, and form water exactly as is produced when the gas is burning in the open air; but at the same time, during the act of formation of this water by the oxygen of the peroxide of barium, there is a solid to be heated to redness. We will now heat the peroxide of barium. I have wrapped it up in talc. You see now that water is being produced, and gradually as the heat gets up you will see that the action will become much more perceptible. This tube is rather too small for me to apply as much heat as I desire. The water is now being formed in the presence of a solid. What a brilliant amount of light the gas gives out on account of the solid being at the same time heated to redness. You see, therefore, that when we form exactly the same product under the conditions under which a solid becomes heated, a large amount of illumination takes place. I want to show you that in another way, and in a very instructive way. I have here a means of passing hydrogen through this row of bottles. I now can pass hydrogen through these various vessels, which contain various volatile liquids. Now, this hydrogen will, on its passage through the liquids, suck up portions of these various preparations, and we shall charge the gas with solid matter, and obtain in this way an illuminant. The liquids through which it passes are chlorochromic acid, chloride of antimony, and benzole. The first two will give solid oxides to be heated to redness; the latter will give solid carbon, and the hydrogen in all cases becomes illuminating. Unfortunately, however, I cannot continue this instructive experiment, for some air has entered into the bottle since the beginning of the lecture, and produced an explosive atmosphere.

The fact which I want you to notice is that there are illuminants in coal gas, which are illuminants because they contain so much carbon,—that in proportion to that carbon they are illuminating; that as that carbon is small in quantity, they are merely diluents, and dilute the gas without in any way illuminating it.

And now I must explain to you what is the action of these diluents. These diluents which are described on the diagram, and which you have seen, are the hydrogen, the carburetted hydrogen, and the carbonic oxide. Their object is to sweep the gas out of the retort and prevent the illuminating gases being decomposed by the heat. In distillation a large quantity of the illuminating gases is being produced, but they may be decomposed by the red hot vessel; and unless there were these diluents formed at the same time to sweep out these illuminating gases, it would be impossible for the gas to preserve the highest quality.

I will now, before we go into any further experiments, describe to you the general conditions of the coal manufacture. First, we have to try to get a large amount of illuminating ingredients mixed with enough diluents to protect them in their formation, and to enable them to be burnt without smoke and without smell; and second, we have to try and remove the noxious gases and impurities.

The first condition or process in the manufacture is distillation. For this purpose there are certain retorts which are represented on that first diagram. These retorts may be made of iron, as in our experiment, or they may be made of earthenware as they are represented in the diagram. In these retorts is placed coal, which varies in amount from 12 cwt. to one ton, according to the size of the retorts. The fire overlaps the retorts, as it is doing in this case of the gas manufacture which is now going on as experimentally, and heats the coal gradually. The substances which are formed are partly tar, which comes over as the tar is now coming over into this condenser, partly water, and partly the gas which is also going over. If you follow these retorts you will see that there are certain stand-pipes. These stand-pipes conduct the gas up into what is called the hydraulic main. The stand-pipes are generally about five inches in diameter, and dip into a larger cylinder or pipe, which is fifteen or sixteen inches in diameter. There is a curving over as you observe, so that the pipe dips into tar-water, which condenses in this hydraulic main. By this arrangement the ends of the pipe are sealed up by the water in the main, but can bubble through it. The gas comes over here, and the surplus tar passes over into what is called the tar-well by means of this small pipe which conducts away the tar and water into the tar-well. And now the gases, having come out of the retorts, pass throught the series of pipes which are represented in the next diagram. The object of those pipes is to cool the gases. They expose a large surface to the cooling influence of the atmosphere. Sometimes water is poured on them, but usually they are merely exposed to the air. This condenses the coal-tar and water which the gas still retains in suspension. Now, the gases pass after that through what is termed an exhauster. The use of the exhauster is this—that in passing through that system of pipes a considerable friction is caused, and that friction would retard the flow of the products were it not that this exhauster is used, which prevents the friction keeping the gas back in the retorts. If the gas were retained for any time in the retorts, the heat would decompose the illuminating gases and convert them into diluents, and, therefore, this exhauster is employed. After passing through the exhauster, the gas traverses what is termed the scrubber. This scrubber is merely a large cylinder filled with coke, which is sometimes dry and sometimes has a quantity of water passing over it. This washes the gas and removes many of the impurities from it. It acts mechanically, also, by taking away

the tar and removing it from the gas. The gas having passed through the scrubber, it now goes through a very important piece of apparatus which is termed the purifier, and which I must now show you here. You see here a vessel containing different shelves. These shelves are covered with lime, and the lime has the power of absorbing two of the constituents of the gas, which are very injurious: first, the carbonic acid; and secondly, the sulphuretted hydrogen, both of which are serious impurities. The milk of lime, or lime, which is employed for this purpose, takes away the sulphuretted hydrogen, which in itself has a bad smell, and which, in its combustion, produces sulphurous acid and decolourises drapery and other matters when it comes in contact with them; and the lime also removes the carbonic acid which, even when present to the amount of only one per cent., diminishes the illuminating value of the gas six per cent. But the ammonia is not removed by this operation.

In recent years, another plan has been employed for purifying gas, and it is to this I would like to draw your attention. The new process consists of passing the gas through a mixture of sawdust and iron. The oxide of iron takes away the sulphuretted hydrogen by producing water and forming sulphide of iron; and after it has acted for some time, the passage of air through the mixture restores the sulphide to the state of oxide depositing sulphur, so that the purifying agent can be repeatedly used till its pores get choked with sulphur.

I have here the means of showing you how completely this plan of oxide of iron mixed with sawdust purifies the gas; and it is this which, not entirely, but to a great extent, is employed to the exclusion of the lime purification. I have here a bent tube containing oxide of iron and sawdust. The coal gas which I turn on is forced to pass through a saturated solution of sulphuretted hydrogen, so as to become fully charged with that noxious gas. I first allow the gas to pass, not through the iron, but at once through a solution which I have here as a test for sulphuretted hydrogen. In this solution I have a little nitro-prusside of sodium, which is an extremely delicate test for sulphuretted hydrogen—so delicate, that if I were to take a lock of lady's hair, dissolve it in an alkali, and put it into this liquid, it would immediately assume a purple colour. You see [after passing the unpurified gas into the test water] the solution has now become of a purple colour, showing the presence of sulphuretted hydrogen abundantly in the gas which we are employing. Now, having shown you that, I will turn this off and pass the gas through the oxide of iron and sawdust intervening; and you will observe that, at all events for a considerable time, it will pass through the solution and produce very little coloration. It has produced a trace. We are passing it too quickly. The least quantity does it, but, you see, not nearly so rapidly as before the gas passed through the purifying mixture. The smallest quantity—the most minute trace passing will produce the coloration, but you notice that scarcely a sensible coloration is produced, and we get nothing at all like this effect [referring to the coloration caused by the unpurified gas]; so that the oxide of iron has the power of taking out the sulphuretted hydrogen, and producing sulphuret of iron, and relieving the gas from the presence of this noxious ingredient. There is another substance, however, present as an impurity which this process does not remove; it does not remove the bisulphide of carbon, or the compound of carbon and sulphur, which is always present in gas. The bisulphide of carbon, CS_2, which is present in coal gas, has lately been removed by passing it over heated lime; the water of the lime is decomposed, and sulphuretted hydrogen and carbonic oxide produced; and then this sulphuretted hydrogen can be removed by the ordinary lime-purifier in the manner which is shown in this experiment. Dr. Smith has, within the last few weeks, introduced a second plan for removing the carburetted hydrogen by passing the gas through a solution of oxide of lead in sawdust. I need not follow the gas through the various other mechanical contrivances for collecting it and distributing it through towns. After the gas is purified it then passes through the gas-meter, where it is measured, into the gas-holder, from which it is distributed to towns. There is an ingenious arrangement here which is interesting. The diagram shows a gas governor or register which regulates the quantity of gas which is to be given out at various times during the day. If you look at that little diagram in the centre, you will see that the amount of gas used varies according to the time of day. During the day it is almost a dead level; about nine o'clock, at this time of the year, the gas begins to be lighted, and you see how rapidly it goes up till about eleven o'clock; people go to bed about that time, and then it as rapidly descends till about day-break, when it goes up a little further. Now, the plug by rising and falling determines the amount of gas which shall be given out during the different hours, and according as it is lowered in the tube, or is allowed to ascend, so the quantity of gas escaping into the town is regulated.

I want you now, for a moment, to consider what is the chemistry of the ordinary candle, not that I am going to do what has been so often and so successfully done here by Dr. Faraday, but I propose merely to draw your attention to a very important discovery in the manufacture of candles, which has been introduced within recent years. If candles had followed instead of preceded gas manufacture it would have been said that they were the greatest discovery of modern times. A properly constructed candle is merely a portable gas-works, requiring not costly or complicated apparatus; it is a means by which a very pure gas is produced little by little, as we desire to burn it, and in this respect forms an interesting illustration of the subject I have brought before you.

Candles and lamps differ only by one being fed with a liquid oil, the other having an oil liquefied for it as is desired. The heat of the candle liquefies a certain portion of oil which is drawn up by the wick, and is there converted into a gas, and is burned. The earliest candles that were introduced were, no doubt, torches. We read of Ceres instituting the search for her daughter Proserpine by the light of two burning pines, which were merely a rude substitute for our modern invention. The history of the candle is tempting, but I cannot enter upon it. I would simply draw your attention to this diagram, by which you will see the manner in which it acts as a portable gas-works. Here I have a diagram of the candle. This represents the liquefied fat which is drawn up into the pores of this carbonised wick, which you must consider as so many small gas-retorts. The fat or the wax is constantly distilled, and forms the gas which burns in the ordinary flame of the candle, the gas being produced as it is required, and needing no purification. If it were possible to take the most illuminating ingredient of coal gas, which is olefiant gas,—that ingredient which contains 85 per cent. of carbon,—if it were possible to take that illuminating ingredient and to condense it into a solid, we should have the highest conditions for the manufacture of coal-gas. Liebig, as early as 1841, said in one of his letters that it would certainly be esteemed one of the greatest discoveries of the age if any one should succeed in condensing coal gas into a white, dry, odourless substance, portable, and capable of being placed upon a candlestick or burned in a lamp. This was in 1841. In 1851, at the Exhibition, Mr. Young exhibited a substance termed "paraffin," that had formerly been made from peat, or from the distillation of wood in small quantities, and he exhibited a single candle made from it. Now, this paraffin is nothing but olefiant gas in a solid form, that is to say, it is isomeric: it is of the same composition exactly as olefiant gas, and is simply olefiant gas, if you will allow the "bull," in a solid state. When coal is distilled at a lower temperature than that necessary

to form gas, there first comes over an oil, which contains in solution a solid. This oil itself is called paraffin oil, it is in reality of the same composition as olefiant gas, that most illuminating gas which I showed you. When it is cooled it deposits a solid substance known as paraffin. Now, it is very interesting to observe that all these three are the same in composition. This oil is liquid olefine, the solid body is also an olefine, and is termed paraffin. The ordinary olefiant gas, which gives an illuminating character to our coal gas, is the same substance in a gaseous state; or, rather, is not the same substance, but a substance of the same composition. In 1851 I was so struck with the one candle that was exhibited in the Exhibition that I gave one of the Friday evening lectures here upon the subject, and stated that it was probably the germ of an industry which would become a very important one. It has now grown so large that the Bathgate Chemical Works, for the manufacture of paraffin and paraffin oil, rank among the largest chemical works in the world. If you will go now to the Exhibition of 1862 you will see enormous blocks of this solid wax produced from coal Here is the coal from which it is derived—the bog-head coal: it certainly does not look much like coal, and has promoted great discussion as to whether it is a coal or a schist. This bog-head coal, and other coals, when slowly distilled, produces an oil, from which this beautiful solid wax called paraffin is prepared. It is, as I have explained, of the same composition as olefiant gas, and from it are now produced those beautiful wax candles which we ordinarily burn, and which are the perfection of the manufacture of coal gas, because each of these contains the illuminating material of coal gas in a condensed and solid form, so that when the candle is burned little by little, this olefine is changed from its solid state to its gaseous state, and is consumed. This paraffin is a beautiful wax, melting at 120 or 130 degrees; it produces a beautiful white light, on account, in fact, of producing the true olefiant gas. The oil also has the same qualities. You observe it burning here, and you must distinguish this from what are termed the paraffin oils which are now ordinarily sold in commerce, and which come from Canada and other parts; they contain various volatile bodies which take fire at ordinary temperatures, and which are extremely dangerous; they may be as dangerous as camphine or benzole on the application of a light. Paraffin oil made from coal does not burn at ordinary temperatures, it only burns in the presence of a wick, and is perfectly safe, carrying up, by the slow capillary attraction of the wick, a certain quantity of the oil in contact with the heated surface,—olefiant gas being thus gradually produced, so that you have the true perfection of an illuminating gas formed in the lamp.

In the next lecture I propose to follow out that tar and water which are now in the condenser, and to show you what beautiful utilities they have been converted into; we shall take the ammoniacal salts produced from the water, and the beautiful coal-tar colours which are formed from the tar.

NOTICES OF BOOKS.

A Handy Book of the Chemistry of Soils; Explanatory of their Composition and the Influence of Manures in Ameliorating them, with Outlines of the various Processes of Agricultural Analysis. By John Scoffern, M.B. London: Bell and Daldy. 1862.

Dr. Scoffern always writes well, and sometimes eloquently, when he takes a subject in hand; and his present work will be read with pleasure by others than the class of readers specially addressed.

The scope and purpose of the book is thus described by the author:—

"The chemistry of soils is confessedly a very extensive subject. . . . Unlike most technical arts dependent on chemical principles, the subject does not involve one branch only of that science, but many; including amongst them the most difficult of all—the chemistry of organic beings. But whatever may be the ultimate line of divergence of our inquiries—whatever special points of the science we may choose for investigation—the commencement of every work on agricultural chemistry must consist of a description of the constituents of the soils; that is, of the chemical elements and material substances which enter into their composition. These points being investigated, we naturally proceed to discuss the subject of the food of plants, which involves the theory and the practice of manures. Afterwards we may enter upon the consideration of certain animal and vegetable products and educts; thus bringing under the student's notice the subjects of malting, brewing, cider-making, the manufacture of wines, the preparation of butter, cheese, bread, &c."

There follows consequently a rapid sketch of the principles of chemistry, and a short description of the elements forming the crust of the earth, and entering into the composition of organic beings. Special chapters on the philosophy of vegetation: other subjects of particular interest to the agriculturist succeed, and the book is closed by a section on the theory of manures, with instructions for their analysis which will be of great service to an intelligent farmer.

There are several passages which we should be glad to quote, but our limited space forbids, and we must content ourselves with cordially recommending the book to those of our readers who wish to become acquainted with the elements of chemistry as practically applied to agriculture.

In another edition, which we hope may soon be called for, the author will do well to correct an unfortunately large number of errata, which may either mislead or puzzle the non-chemical reader; thus *Berzelius* is variously printed *Berzilius* and *Berzcline; legumin* is tranformed into *legunim; precipitate* stands for *proximate*, and *methylated* spirit is made *methlagated;* a change which may get Mr. Hodge laughed at, if he goes to a pert druggist for the article.

The Law Against Adulteration of Articles of Food and Drink, as Affected by the 23 & 24 of Vict., cap. 84. With a Summary of the Various Statutes relating thereto. By S. F. Bilton, Esq., Barrister-at-Law. To which is added, *Simple and Reliable Tests for the Detection of Adulterations in Bread, Tea, Wine, &c.* By E. A. J. Timothy. London: Mann, Nephews. 1861.

The above is the long title of a short pamphlet of forty pages, which contains an analysis of the provisions of the Act of 1860, with forms of procedure. As this enactment is to all practical purposes a complete failure, it is quite unnecessary to go through it once again to show that it was never likely to accomplish the end proposed. So long as an Act of this kind is simply permissive, and its application depends upon individual caprice, so long will it remain to all intents and purposes a dead letter. The only effectual Act will be one which will empower the analyst to visit shops and take specimens of the stock for examination—to constitute, in fact, the analyst a public prosecutor. Let us suppose a similar Act to that on Adulteration applied to weights and measures, and how many fraudulent tradesman do our readers suppose would be prosecuted under such an Act? And what is the moral difference between short weight and weight made up with some worthless ingredient? The analyst should have the same power to inspect stock that the officers of Inland Revenue have to inspect weights and measures, and then an Adulteration Act would have some good effect.

Appended to the review of the Act of 1860 is a short

notice of other enactments which relate to the practice of adulteration, all of which, except those which concern the Excise, are practically as inoperative as the last law on the subject.

The legal part of this little book we can safely recommend to all our readers who wish to be acquainted with the laws on adulteration; but there is not much to be said in favour of the simple and reliable tests added thereto by E. A. J. Timothy, Esq., author of "Poisons: their Effects and Antidotes." The detection of alum in bread chemically is not such a simple business, so Mr. Timothy shirks it, merely telling his readers that when they heat bread-and-water in a *pipkin* over a gentle fire, the alum is dissolved out by the water, "*and may be extracted from it in the usual way.*" Here, however, is "a simple and reliable" process:—

"*To Detect the Presence of Lime in Bread.—To the clear liquor in which a loaf of bread has been boiled,* add a small portion of the oxalate of ammonia; should lime be present in the smallest quantity the liquor will become milky, and a white insoluble precipitate will be produced; if mineral acid is present in the solution, previous to applying the oxalate of ammonia, add a small quantity of an alkali to the water in which the bread has been boiled."

But how to find the acid, Mr. Timothy? asks an experimenter, innocent of the uses of litmus-paper. Here is another simple test:—

"*To Detect the Presence of Iron in Tea.*—Mixed with the dust of inferior teas, iron is frequently found, being blended therewith for the purpose of fraud; a simple method of detecting its presence is by suspending a slice of gall-nut by a silken string in the liquor of the suspected tea; if iron is in the solution it will produce a violet or black precipitate."

According to Mr. Timothy, London wine merchants, especially those who attend *rummage* sales at the docks, have the wickedness to add "*litharge, or oxides of lead*" to the fluid called wine they buy, so forming a compound which St. Paul would certainly have recommended Timothy, for his stomach's sake, to *avoid*. Our author therefore gives four simple tests for lead, the third of which we quote for the sake of the English:—

"Test 3.—Impregnated water with sulphuretted hydrogen gas combined with a small portion of muriatic, or any weak acid. By adding this test to the suspected wine, if lead is present, a blackish muddy precipitate will ensue." Not a doubt of it. Most of the other tests are quite as simple as those we have quoted.

NOTICES OF PATENTS.

1428. *Preparation for Hardening and Preserving Stone and Cement.* Jesse Rust, Carlisle Street, Lambeth. Dated June 6, 1861. (Not proceeded with.)

The inventor claims the use of hydrofluo-silicic acid in conjunction either with caustic baryta, or bicarbonate of magnesia, as a means of producing in the pores of the stone an insoluble earthy compound, which is comparatively unaffected by weak acids and ordinary atmospheric influences. In order to prepare a surface of stone or cement in this manner, the inventor first washes it with a hot aqueous solution of caustic baryta, and then applies a second wash consisting of dilute hydrofluo-silicic acid alone, or the same in which a certain quantity of clay (silicate of alumina) has been dissolved. The product of this combination will be a difficultly soluble silico-fluoride of barium or magnesium, and, in the event of clay being used, the precipitate will contain likewise a small proportion of fluoride of aluminium. This double treatment is to be repeated, if necessary, until the pores of the stone or cement are filled up, and the surface sufficiently hardened.

Inasmuch as the proposition of Mr. Rust was among those entertained by the late Parliamentary Committee on the decay of the stone of the New Palace at Westminster, we have already had an opportunity of expressing an opinion upon the merits of this system of preserving stone. It is sufficient, therefore, to refer to our former notice of the Committee's Report, which will be found in Vol. iv., page 237, of this Journal.

1448. *Sulphate of Alumina.* A. A. Croll, Coleman Street, London. Dated June 7, 1861.

These improvements refer to the preparation of dry sulphate of alumina used in the manufacture of paper and for other purposes. The alum shale, china clay, or other aluminous mineral, in the state of fine powder, is first to be roasted in the usual manner. Having cooled the same to the temperature of about 300 degrees Fahrenheit, a quantity of sulphuric acid, having a specific gravity of 1.70 and likewise heated to 300 degrees, is allowed to flow upon it, and digested for some time with frequent stirring. It is recommended to employ the sulphuric acid and alum shale in the proportion of about equal parts by weight.

The foregoing conditions appear to be highly favourable to the solution of alumina in sulphuric acid. The saline mass thus obtained has received of late many applications, for which the more expensive crystallised alums have usually been employed.

1462. *Economising Fuels in Evaporating and Concentrating.* J. Roman, Liverpool. Dated June 8, 1861. (Not proceeded with.)

This invention has for its object economising heat in a great variety of manufacturing operations, and in the generation of steam for motive power. It consists in adopting a peculiar form of evaporating pan or boiler; and in so arranging the flues underneath that their longitudinal section will resemble a blunt-pointed wedge, and the pans themselves shallower at one end.

1468. *Composition for Cleaning and Polishing Metals and Glass.* W. Clark, Chancery Lane, London. A communication. Dated June 8, 1861. (Not proceeded with.)

The inventor prepares a composition, which he terms "brillantine," according to the formula undermentioned: An extract of guano is obtained in the first instance by boiling that substance with water, and concentrating the liquor until it furnishes a pasty crystalline mass on cooling. Of this material he takes 100 parts by weight, of calcined tripoli powder 25 parts, of flour 12 parts, and common salt 10 parts. The whole is warmed, intimately mixed, and formed into a homogeneous paste, which is allowed then to cool and harden. If to be used for polishing metals, the composition is diluted with spirit, but for cleaning glass and common purposes water is sufficient.

The special efficacy of the guano compound may be traced to the hard nature of the crystallised urates which separate during the concentration of the extract. It is probable that for the same reason crude tartar would answer the purpose equally well. The advantage of employing chloride of sodium for polishing metals is open to question, but for cleaning glass a solution of common salt alone has frequently been employed.

Royal Institution.—Monday, June 2, at Two o'clock, General Monthly Meeting. Tuesday, June 3, at Four o'clock, the Rev. G. Butler, M.A., "On the Art of the Last Century;" Thursday, June 5, at Three o'clock, Dr. Lyon Playfair, C.B., "On Progress of the Chemical Arts (1851-1862);" Friday, June 6, at Eight o'clock, Professor Tyndall, F.R.S., "On Force;" Saturday, June 7, at Three o'clock, Professor Anderson, M.D., F.R.S.E., "On Agricultural Chemistry."

CORRESPONDENCE.

On the Estimation of Guano.

To the Editor of the CHEMICAL NEWS.

SIR,—In the CHEMICAL NEWS, No. 128, p. 270, there is certainly an error in Liebig's paper "On Guano." Should not nitric acid have been acetic acid? * By boiling guano with acetic acid the greater proportion of the colouring organic matter, &c., is got rid of, the remaining constituents being chiefly the oxalic acid in combination with lime, uric acid with ammonia, some organic matter and sand; the whole merit of the process consists in getting rid of the colouring matter, &c., without loss of oxalic acid.

A mixture of oxalates of lime and ammonia, phosphates of lime and of ammonia, uric acid and sand, presents no difficulties to the chemist. Thus—

Treat the mixture with water, and filter, &c. The filtrate (*a*) contains the oxalate and phosphate of ammonia. The residue (*b*) all the other constituents.

To (*a*) add ammonia and acetate of lime; the precipitate consists of mixed oxalate and phosphate of lime, which may be separated by acetic acid.

To the residue (*b*) add hydrochloric acid, phosphate and oxalate of lime dissolve, which, when precipitated by ammonia, are separated by acetic acid. The residue (*c*), insoluble in HCl, is treated with potash and boiled, the uric acid dissolves, and may be obtained by acidulating the solution and heating gently. The sand remains untouched.

The ammonia may be estimated by boiling a portion of the mixture with lime and water.

The process is twenty years old or more.—I am, &c.

D. R.

Foreign and English Weights and Measures.

To the Editor of the CHEMICAL NEWS.

SIR,—Would any of the readers of the CHEMICAL NEWS favour a subscriber by giving a precise definition of the French kilogramme or gramme? Say, take the standard gallon of water as defined by Act of Parliament, and from it deduce the litre and kilogramme.

To those who deem the prefixed request too frivolous, and unworthy of notice, the writer begs to state that he has consulted a dozen works, chemical and commercial, and that in all are quasi definitions of the kilogramme or of the gramme; in none are the definitions complete.

The books alluded to express the weight of the kilogramme in English grains, each giving a different value. These differences, it is true, amount only to a few grains, and are, commercially, of no importance; but what becomes of tables of specific gravities, atomic weights, &c., with five and six places of decimals, deduced from calculations involving grammes, grains, inches, litres, &c.? —I am, &c.

D. R.

[A gramme equals 15·432349 grains; a gallon contains 70,000 grains; and a kilogramme equals 1000 grammes; a litre contains 61·027052 cubic inches, and is equal to 0.22009668 imperial gallons. From these data other deductions can be easily made.—ED. C. N.]

MISCELLANEOUS.

Sewage of Towns.—The Select Committee on Sewage of Towns have agreed to the following cautious and safe first report:—"1. That careful and exact experiments are necessary to elucidate the agricultural value of sewage, and the best mode of applying it. 2. That such experiments have been carried on at Rugby by the commission appointed to inquire into the best mode of distributing the sewage of towns, and applying it to beneficial and profitable use. 3. That it is desirable that these experiments shall be continued during the present year."—[We shall very shortly give all that is new and interesting in the evidence laid before the Committee.]

Improved Funnel for Laboratory Use.—Messrs. Bourne and Taylor have just brought out an improved form of funnel. It is made of hard glazed porcelain, and the peculiarity consists in a narrow tube running down the inside from the top and terminating in a small hole within an inch of the extremity of the stem. The part where the body of the funnel joins the stem is also provided with a collar quite flat on the lower side. The advantage of this arrangement is that the funnel may be placed in a bottle, and the mouth of this can then be closed tight round the neck of the funnel, either by moistening it with water or by using some lute. The filtration will then proceed in the usual way, the air driven from the bottle by the ingress of the liquid escaping through the small tube. Access of air or evaporation of the liquid may be prevented by covering the funnel with a glass plate. We think an improvement could be made by having the rim of the funnel ground perfectly flat, so as to allow of a glass plate being applied to it air-tight if necessary. Etherial and other highly volatile liquids could then be filtered without any loss.

Artificial Stone.—The *Suffolk Chronicle* contains a notice of the manufacture of artificial stone in large masses, upon a plan lately discovered by Mr. Frederick Ransome, of Ipswich. The composition of the stone is not given, but it appears that the principal binding material is the indestructible silicate of lime. Blocks weighing a ton and a-half may, it is stated, be completely solidified and hardened in the brief space of two hours, whereas by Mr. Ransome's original process, only small blocks could be made, after a long period for drying and hardening in the kiln. The *Chronicle* quotes a report by Dr. E. Frankland, F.R.S., of St. Bartholomew's Hospital, who says the "patent concrete will be found equal to the best of Portland, Whitby, Hare Hill, and Park Spring stones in its power of resisting atmospheric degradation, and if the newness of Ransome's stone (the specimen experimented upon not having been made a fortnight) be taken into consideration, together with the well-known fact that its binding material, silicate of lime, becomes harder and more crystalline by age, I am induced to believe that Mr. Ransome has invented a material which, with the exception of the primary rocks, is better capable of giving permanency to external architectural decorations than any stone hitherto used." We are informed, moreover, that such is the confidence entertained in the imperishable properties of this material, it has been selected by Mr. Fowler, the engineer, for the facing of the stations of the Metropolitan Railway now in progress. We may also state that its capabilities of resisting strain and sustaining pressure have been found to be nearly three times that of Portland stone; thus, it may be fairly assumed that these qualities, combined with facility of production and the inexpensive nature of the materials used, must ensure for it general adoption in the construction, as well as in the embellishment, of buildings generally, and in works of art. Mr. Ransome has made enlargements and introduced fresh machinery at his works to carry on an extensive manufacture, but it should be observed that the process is so simple that the stone can be manufactured on the spot where the demand arises.

ANSWERS TO CORRESPONDENTS.

W. R.—The best account with which we are acquainted is to be found in the CHEMICAL NEWS. Consult the Index of our previous Volumes.

P. P. P.—Sulphate of bartya is well-known to be soluble in strong sulphuric acid, especially by the aid of heat, but such a solution would be quite unfit for analysis. Water, of course, reprecipitates it.

A Reader.—Apply to Messrs. Longman and Co., the publishers of the work. You will find an excellent introduction to the study of organic chemistry in the new edition of "Fownes' Chemistry," published by Churchill.

* The original paper says nitric acid.—ED. C. N.

THE CHEMICAL NEWS.

VOL. V. No. 131.—June 7, 1862.

THE PATENT LAWS.

ON last Tuesday week, Sir Hugh Cairns brought forward in the House of Commons his long-threatened motion for a Royal Commission to inquire into the operation of the Patent Laws. It is needless to say the motion was carried without a division. The whole policy of the Patent Law will now be brought under consideration, and the opponents and supporters of grants of monopoly will have a fair opportunity of urging their various opinions. Our own views on the subject may be stated in a very few words. We believe it is only right that an original inventor should be secured, as far as possible, some reward for his ingenuity; and this perhaps is best accomplished by giving him for a time a monopoly in his invention. But we have no doubt that we shall fairly state the opinion of all disinterested readers when we say that but one claim for a monopoly should be allowed, and that is, the novelty or the originality of the invention. This satisfactorily established, the right to protection, we think, is indubitable. But it is clear that, proceeding on this idea, an entirely different method of granting patents must be adopted. At present, as Sir Hugh Cairns stated, the only investigation which alleged inventors undergo before patents are granted is conducted by the law officers of the Crown, and is confined to the descriptions in the specifications. Of the novelty or usefulness of an invention they, of course, can be no judges. As regards the usefulness, about which so much parade is made in a patent, we believe that that may be left entirely out of the question. If an invention is not useful, there will be small advantage in obtaining a patent for it. The novelty, however, is a matter deserving some investigation, and this could only be conducted by persons well acquainted with the subjects concerned. Speaking as a lawyer, Sir Hugh is of course opposed to any preliminary *scientific* investigation. This, to a legal mind, we have no doubt, would appear the height of absurdity. It is so much more simple and reasonable for any one to get a patent revoked by applying for "a rule to show cause, or something of that sort, by which a person might be called on to justify his patent, and thus the air might be cleared, and the manufacturing public would be, *at a small expense*, disembarrassed of worthless and trivial patents." No doubt of it, if any one would take the trouble; but unfortunately such a "short and simple mode" might have another effect different to that contemplated. Probably if Sir Hugh had set his wits to work to see in what way a grasping and litigious capitalist might most easily rob an inventor of the fruits of his ingenuity and industry, he could not have proposed a better scheme than the one suggested above. Most likely in the end it would be found quite as expensive to justify a patent as to prosecute for infringements; and another legal reformer coming a few years after Sir Hugh, would have equally gross instances of expensive litigation to quote, and, we believe, with greater wrong done.

The whole reasoning of Sir Hugh on this part of the subject is so well answered in the *Times* that we shall make no excuse for quoting the passage. "The vice of too many inventions now patented," says the *Times*, "is not their want of utility, but their want of novelty, or rather their suspicious resemblance to some other contrivance already in use; and that is a point quite capable of being sifted. Nothing can be more impolitic than to grant a lucrative and exceptional right as a matter of form, on the chance that, if it should have been granted in error, some one will move for its revocation, though not, perhaps, till after it has been transferred by assignments and deeds of licence to a dozen different persons." And so, we may add, been proved to be of sufficient value to make it worth disputing. Nobody would think of going to law about a "worthless or a trivial patent;" but there are people who would move all the Courts at Westminster to upset a valuable one. And then, argues Sir Hugh, scientific men are so sceptical and prejudiced, that they cannot be trusted to give an opinion as to the novelty or usefulness of an invention. Sir Humphry Davy, he says, did not believe in the possibility of lighting houses with gas, and had he been acting as a judge, he would have condemned that invention as useless. We do not believe that Sir Humphry would have done anything so absurd. If an inventor had come forward professing to accomplish what had before been deemed impossible, Sir Humphry Davy, like a sensible man, would have considered it the strongest ground for granting him a patent, and would have left him to demonstrate the practicability of his scheme when he had been fairly protected. Sir Humphry might have objected,—he did so, in fact,—to the lighting of a house with gas as *dangerous*, but he would never have condemned the invention as useless.

Taking chemical patents as illustrations, and not being lawyers, we believe that the manufacturing public would be most easily disembarrassed of worthless and trivial patents if the specifications of the alleged inventors were submitted to a competent chemical authority before the patent was granted. It would be invidious now to mention special instances; but any reader of the CHEMICAL NEWS, or any person having much acquaintance with chemical patents, will remember numerous cases in which they ought never to have been granted. The ideas in these patents have not the least show of novelty, they may even be found in a familiar manual; but an insidious wording of the specification and the claim opens the matter for litigation if it should ever turn out worth disputing, and most manufacturers, unless very wealthy, would rather pay for a licence than defend an action for infringement.

With all that fell from Sir Hugh Cairns respecting the tribunals for the trial of patent cases, we fully agree. He seems inclined to accede to the proposal for scientific assessors, which we advocated some weeks ago, but expressed no decided opinion on that point. It is hard to

see why, if scientific assessors may advise a judge in the trial of patent disputes, scientific commissioners may not advise the Attorney-General in granting patent rights, except, by the way, that the latter proceeding might perhaps render much of the former impossible.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Presence of Rubidium in Certain Vegetable Substances (Beet-root, Tobacco, Tea, Coffee, Grapes), by M. L. Grandeau.

On the 24th of February last, I had the honour to communicate to the Academy the results of my researches on the presence of rubidium in the salts from beet-root, and in the mother-liquors obtained from treating them for the extraction of chloride of potassium. Since then I have actively pursued this research, both in the laboratory of the upper Ecole Normale and also in the important factory of M. Lefebvre, distiller at Corbehem, who has kindly placed at my disposal the substances necessary for extracting chloride of rubidium on a larger scale.

Thanks to this assistance, I now possess 400 grammes of pure chloride of rubidium, about half of which has been prepared at the factory at Corbehem, according to my instructions, by the assistance of M. Martel, a skilful young chemist attached to M. Lefebvre's establishment.

When presenting to the Academy, at a subsequent sitting, the new salts of rubidium which I had been able to prepare from the pure chloride at my disposal, I will describe the processes which I have employed for the extraction of the chloride, and will show by the aid of a few figures that the quantity of rubidium annually removed from one hectare of land by beet-root is an amount not to be neglected from an agricultural point of view.

I now propose to submit to the Academy some new results, proving the great dissemination of rubidium in nature. Having found the new metal in the salts from beet-root, which it is known are very rich in potash, it seemed of interest to seek for it in other vegetable substances, which, by the readiness with which they remove potash salts from the soil, more or less approach beet-root in this respect. I will confine myself in this extract to point out the analytical results which I have obtained, omitting the methods of separation and analyses given in my memoir.

1. Tobacco.—I have at present only examined the leaves from Kentucky and Havannah. M. Schlesing, Directeur de l'Ecole d'Application des Tabacs, has been good enough to evaporate to dryness in his laboratory a certain quantity of water which had been used for the prolonged washing of Kentucky leaves. The ignited residue was tolerably white, spongy, and very rich in potash. On spectral analysis this residue gave the characteristic lines of lime, lithium, potassium, and rubidium. The quantity of lithia was very slight. There was, on the other hand, a notable quantity of rubidium.

Leaves from Havannah, best quality, were carefully burnt; their ashes gave me on analysis results identical with those obtained from the Kentucky leaves.

2. Coffee and Tea.—Coffee and tea completely and carefully incinerated left ashes rich in potash. An examination of these ashes, after appropriate treatment, showed in each of these products considerable quantities of rubidium, but not a trace of lithium. Coffee is much richer in rubidium than tobacco.

3. Grapes (crude tartar).—M. Kestner, of Thaun, has kindly sent me, at my request, some mother-liquors obtained in the treatment of crude tartars. These liquids were freed from organic matters and foreign substances which they contained, and the residues then submitted to spectral analysis. I am able to state definitely that they contain rubidium, but in very small quantity.

It appears certain from these facts that rubidium is one of the most widely distributed simple bodies in nature. The most different vegetable bodies from the most distant localities remove it from the soil. Moreover, it is evident from my researches that the presence of rubidium is not necessarily allied with that of lithium, as might have been imagined from the analyses of minerals and waters in which M. Bunsen has discovered this metal. I ought to add that several vegetable bodies of which I examined the ashes appeared to contain no rubidium, although many of them were rich in potash. I may especially mention as instances the colza, the cacao, the sugar-cane, and some species of fucus.

The dissemination of the new alkaline metal being placed beyond doubt by the researches of which this is a *résumé*, it is of great interest to examine with this particular object the soils in which the above-mentioned vegetables grow. With this view I have undertaken experiments and analyses, which I am pursuing as rapidly as the long and delicate character of these investigations will allow.—*Comptes-Rendus.*

On the Production of Nitrate of Methyl, by M. Carey Lea, *of Philadelphia.*

For the production of nitrate of methyl but one process appears to have been proposed, and that is to be found in all our text-books, English, German, and French. Two parts of powdered nitre are to be distilled in a capacious flask with a recently prepared mixture of 5 parts wood spirit and 10 oil of vitriol. Judging from the reactions of ethylic alcohol, it did not appear to me probable that such a proceeding could succeed. It was tried, however, and with the following results.

The substances were placed in a flask capable of containing twenty times their united volume, which was connected with a Liebig's condenser by a wide delivery tube. For a few minutes no action was perceptible, but it soon set in, with rapidly increasing violence. Torrents of gaseous products with deep red fumes of oxides of nitrogen were evolved, and presently the apparatus blew up with a loud explosion, and had not due precaution been taken with a view to a possible unpleasant conclusion, personal inconvenience might have resulted, for the 3-litre flask was shattered into very small pieces, which were thrown to a considerable distance. The quantities operated upon were small; 50 grammes of methylic alcohol and proportionate quantities of the other substances. No heat was applied.

It is scarcely probable that the gases were evolved in such quantities as to have caused the explosion. It seems more likely that the heat generated by the reaction was sufficient to raise the temperature of the interior of the flask to 150° C., at or below which point, according to Dumas, the vapour of methylic nitrate explodes.

I have had no difficulty, however, in preparing this ether by a different process. By dissolving a considerable quantity of urea or nitrate of urea in methylic

alcohol, it supports the action of nitric acid with the utmost facility. The following are the proportions which I have employed.

Into a retort of the capacity of a litre, 200 c.c. of purified wood spirit are placed, and about 40 grammes of nitrate of urea are added and heat applied. When solution has nearly taken place, 150 c.c. of nitric acid, free from the lower oxides of nitrogen,* sp. gr. 1·31, are added, and the mixture is distilled to one-third. 170 c.c. of wood spirit and 130 of nitric acid are then added and distilled to the same point. Finally, 150 c.c. wood spirit and 110 nitric acid, with 10 grammes of nitrate of urea, are added, and distilled to the same point as before. It is useless to carry the distillation further than the point here specified, not that it is accompanied by any inconvenience, but because nitrate of methyl ceases to be evolved. The temperature rises very high at the close of the distillation.

The operation may be carried on rapidly. We are recommended in the text-books to carry off the vapours very carefully in preparing nitrate of methyl, on account of the production of cyanhydric acid as a by-product. In chemical laboratories there is, doubtless, generally rather too little precaution taken than too much against noxious vapours; but in the present case I have carefully examined the distillate, both in the old process, which failed, and in that which I here propose, and I could find no trace of cyanhydric acid either by the iron or the silver tests, or by conversion into sulphocyanide. Both the ether itself and the watery part of the distillate were tested. As, however, it is impossible without special analysis to know what impurities may be present in so variable a substance as commercial wood spirit, it is difficult to foresee what substances may be generated in its decomposition; but I think I am justified in concluding that cyanhydric acid is not generated by the action of nitric acid upon methylic alcohol; at least, not in the presence of urea.

Treated as above described, 420 grammes of wood spirit yielded a distillate, from which by agitation with solution of salt there separated the very large quantity of 300 grammes crude nitrate of methyl. This may be subsequently agitated with a little weak solution of carbonated alkali.

The wood spirit before use should be distilled with one-third of its bulk of very strong (almost saturated) solution of caustic soda, to decompose any acetate of methyl which may be present. This operation must be performed over the water bath.—*Amer. Jour. Science and Arts.*

A Quick and Easy Method of Preparing Sulphate of Cadmium.

THIS method, adopted by the author, is nothing more than the application of the fact observed in 1792 by Richter, that a metal plunged into a saline solution substitutes itself for the metal, which forms the base of the salt employed. A quantity of crystallised sulphate of copper, say 100 grammes, is dissolved in water, and a piece of cadmium, rather more than is necessary to saturate all the sulphuric acid, or in this case more than 44·59 grammes, is plunged into the solution. The whole having been allowed to stand for some time, the precipitated metallic copper is then separated by filtration and the liquid slowly evaporated. If, during evaporation, the neutral solution of sulphate of cadmium should deposit a small quantity of sesquioxide of iron, which not only constitutes an impurity, but gives the salt a bad appearance, it is necessary to expose the solution to the atmosphere until all the iron which it may contain has been eliminated, which is accomplished when, after a second filtration, the transparency of the solution is no longer disturbed. To obtain finally the sulphate of cadmium in well-formed crystals, it is necessary to acidulate the solution slightly with dilute sulphuric acid. —*Journ. Md. Col. of Pharm.*

New Method of Estimating Alkaline Hydrates and Carbonates, and other Compounds of this Class, by M. PERSOZ.

IN a recent number of the *Annales du Conservatoire des Arts et Metiers* a new method of estimating nitric acid was described, founded on the following facts:—

1. Fluorides, chlorides, bromides, and anhydrous alkaline sulphates are not decomposable by potassic bichromate heated to fusing point, and even to nascent red heat.

2. All nitrates are decomposable under these conditions. Nitric acid is completely expelled in proportion as chromic supplies its place, and an equivalent quantity of chromate is produced.

Now, in applying this method to the estimation of certain commercial salts of soda,—mixed with carbonate, chloride, sulphate, and nitrate,—which are frequently sent to the laboratory of the Chamber of Commerce, which salts sometimes contain as much as 19 per cent. of nitrate, I found that by heating with precaution a mixture of these salts and bichromate, taking care not to raise the temperature sensibly beyond the fusion point of the latter, that the whole of the carbonic acid was expelled without taking the nitric acid with it.

It is evident that we here have the means of determining through successive losses estimated by weighing first the carbonic and then the nitric acid. As the loss of nitric acid in a well conducted operation corresponds exactly with the alkalimetric standard, one more step only is necessary to the formation of a rational method of estimating commercial alkaline carbonates without having to fear, in certain cases, the errors inevitable to the employment of the ordinary methods, and owing to the presence of alkaline sulphides, oxisulphides, lime, sulphites, and hyposulphites, &c.

It is easy to understand, and it has been ascertained by direct experiments, that potassic bichromate either oxidises or saturates oxisulphides, sulphides, sulphites, hyposulphites, and lime without causing any disengagement. A carbonate, on the contrary, decomposed by potassic bichromate occasioned a disengagement of carbonic acid exactly proportioned to the amount of the base which retained it in combination. A hydrate equally disengaged a quantity of water corresponding to a simple hydrate or to a bihydrate, according to the temperature employed.

It only remained to devise an apparatus so to conduct the experiment that the products of the reaction might be collected. Liebig's apparatus was ready to our hand, and this, slightly modified, was employed to analyse the organic matters. We used then a combustion tube from 50 to 60 centimètres long, very slightly curved in the centre to a U-form, and bent inversely on both sides, so

* Freedom from the lower oxides is an essential condition of success. That nitric acid is colourless is not in itself a sufficient indication of purity in this respect. An acid which causes the least darkening to a solution of ferrous sulphate is wholly unfit for use in the preparation of either methylic or ethylic nitrate.

as to keep the two extremities horizontal. By one of these extremities the tube was in communication, by means of a small copper cock, with a system of columns and tubes furnished with all the agents usually employed for freeing the air from foreign bodies; by the other it was connected with a Liebig's apparatus for retaining the water and carbonic acid disengaged by the combustion of an organic matter. Finally, the apparatus communicated with an exhauster by the intervention of a flask or of a drying tube, to prevent contact of the humid air of the exhauster with the air of the apparatus.

To sum up, the apparatus is composed of the following parts:—

V, an aspirator, serving to circulate air through the apparatus and to pass the disengaged water and carbonic acid over the bodies destined to absorb them.

A, a system of gauges and tubes furnished with agents necessary for the purification of the air.

B, a combustion tube containing the bichromate and the substance to be analysed.

C, a complete system of tubes for the total absorption of the water and carbonic acid.

D, a tube of U-form midway between the exhauster and Liebig's potash tube, and intended to prevent the access of moist air.

Thanks to these arrangements, it is possible, as will be seen, to regulate at will an operation of which the results leave little to be desired, if some precautions to be described further on are not neglected.

Method of Operating.—In operating upon a carbonate, it is sufficient to introduce into the tube *B* 30, 40, 50, or 60 grammes of melted potassic bichromate,* previously mixed with 1, 2, or 3 grammes of carbonate, if this is insoluble, but if it is soluble the preliminary mixture is superfluous.† The tube *B* being securely connected at its two extremities with the two systems described, we determine the flow of water in the aspirator *V*, in order to produce a current of air in the apparatus, and then we heat the tube *B*. Simultaneously with the fusion of the bichromate the disengagement of carbonic acid begins, which is easily moderated during the whole of the experiment. The operation is at an end when the whole mass is fused. The increase of weight of the potash tubes indicate the amount of carbonic acid disengaged, from which the proportion of carbonate is to be deducted.

In operating upon a hydrate or upon a mixture of hydrate and carbonate, we proceed in the same manner, only it is necessary to take all precautions, both before and after the operation, that no moisture exists in the tube *B*. Then the respective weights of the water and carbonic acid show the relative proportions of carbonate and hydrate, taking into account the fact that mixtures of commercial alkaline carbonates and hydrates are always formed of a bihydrate. This result is attributable to the custom of manufacturers of submitting these mixtures to simple aqueous fusion instead of making them red hot. However, by estimating the base common to the water and carbonic acid all uncertainty disappears.

For commercial potash and soda, containing sulphides, sulphites, lime, &c., we use the same processes as with carbonates and hydrates, it being only necessary to know how to raise the proportion of bichromate, and, according to the nature of the salt, to observe certain precautions.‡ For the rest we cannot give a better proof of this than by citing some of the results of our experiments.

Having at our disposal a commercial soda—a mixture of carbonate, bihydrate, chloride, and sulphate—of which we knew, besides the alkalimetric standard, the exact proportion of each of the principal ingredients, we selected it on account of its complex nature as the basis of our operations. Submitted to the action of bichromate in our apparatus, it yielded—

Carbonic acid . . .	29° per cent.
Water	5·5 „

These numbers given in sodic carbonate and bihydrate correspond within a few thousandths with the alkalimetric standard of this product.

We treated it in the same manner, but with the addition—

1. Of 50 per cent. its weight of sulphate of lime; we collected—

Carbonic acid . . .	29·5 per cent.
Water	6·5 „

2. Of 5 per cent. its weight of sulphate of soda containing carbonate; we collected—

Carbonic acid . . .	29·8 per cent.

(The salt not having been dried, the water was not taken into consideration.)

3. Of 100 per cent. its weight of commercial quick lime, containing water and carbonic acid, we collected—

Carbonic acid . . .	31·2 per cent.

We must not forget that the compounds used in these experiments are with difficulty kept from contact with the air, from which they rapidly absorb water and carbonic acid, which accounts for any difference in the results. Nevertheless, the numbers obtained prove that in circumstances as exceptional as those under which we operated, and where alkalimetric testing was impossible, our results are not far from exact.—*Journal de Pharmacie et de Chemie.*

TECHNICAL CHEMISTRY.

Paraffin Oil: Report on the Quality of Illuminating Oils sold in Manchester and the Neighbourhood, by Charles O'Neill, *F.C.S.*

In compliance with a request of the General Committee of the Manchester and Salford Sanitary Association, I have examined a number of samples of oils sold in this town and neighbourhood. In all I have examined thirty-two samples, twenty-five of which were bought at as many different retail shops. Two samples of paraffin oil I obtained direct from the Paraffin Light Company's premises; and five samples of oil, obtained from London,

* Before being used the bichromate should be carefully heated. When cold it should be placed in a bottle stopped with emery, as it absorbs and fixes the ammonia of the air. Notwithstanding the fusion to which the bichromate is submitted, we take the precaution of again melting, just before using it, the quantity required for an operation.

† In operating on insoluble carbonates, such as those of lime, baryta, strontia, magnesia, manganese, iron, zinc, copper, lead, &c., it is essential, in the first instance, to reduce these salts to a fine powder, by one or other of the methods usually employed for that purpose. When, on the contrary, carbonates, with potash, soda, or lithia bases, especially the two first, are operated upon, this precaution is not only useless, but dangerous, no account of the rapid decomposition which takes place, occasioning projections of bichromate, sometimes reaching as far as the first Liebig's tube, if the precaution is omitted of placing at the anterior part of the tube *B* a piece of ignited amianthus, which has the effect of retaining the projected portions of bichromate, and thus preventing errors. The operation finished, that part of the tube containing the amianthus should be heated, so as to expel any water which may have condensed in it.

‡ In operating upon crude potashes and sodas containing charcoal, besides the sulphides and oxisulphides, it is indispensable previously to wash them, to evaporate the washings, and then to determine the weight of the saline matters thus obtained; the salt being properly dried, it is then only that the bichromate can react.

Liverpool, and Bradford, were forwarded to me by the Paraffin Light Company, and included in my examination.

Out of the twenty-five samples purchased in Manchester, sixteen were clearly identical with the genuine samples of paraffin oil supplied to me by the Manchester agency, the other nine samples consisted of other kinds of oil differing from the paraffin oil and from one another. Some of them were supplied in answer to a request for paraffin oil; three or four purported to be American rock oils, and were sold under the names of petrolene, kerosene, and photogene. The whole nine were doubtless various qualities of American rock oils, and, adding to these the five samples sent by the Paraffin Light Company, I had fourteen samples from the new and extraordinary oil wells in Pennsylvania and Canada; the remaining eighteen samples being the home manufactured oil sold as "Young's Patent Paraffin Oil."

I obtained three of the lamps employed for burning these oils; they were low-priced lamps, such as are used in the poorest sort of houses, but do not appear to differ in any essential point of construction from the more expensive lamps. These lamps are constructed purposely for burning the paraffin oil; they are not suitable for burning the older kinds of lamp oil, nor are they safe with very volatile fluids, as naphtha and camphine.

In deciding whether any of the samples under investigation were dangerous or not, I have considered them entirely in reference to the lamp in which they are used; some of the oils called dangerous might be safely used in another kind of lamp, and the safest of them would be dangerous if used in a moderator or Carcel lamp.

It results, from my experiments, that the chances of accident from the use of these oils may be referred almost exclusively to their greater or less proneness to form an explosive mixture with the air contained in a partly filled bottle or lamp reservoir. I believe it will be found that nearly all the accidents arising from the use of the new illuminating oils have been primarily caused by the ignition of the explosive mixture of combustible vapour and air, either in the bottle in which the stock of oil is kept, or in the reservoir of the lamp; very few accidents happen while the lamp is burning, they nearly all occur while pouring out the oil, or in lighting or trimming the lamp. The explosion may break the vessel and scatter the oil about, and if it be very inflammable in an ignited state.

My examination was then chiefly directed to ascertain whether any of the oils would form an explosive mixture with air at the medium temperature of 60° F.; this temperature may represent the average temperature of domestic rooms where the bottles of oil are kept. To ascertain this point, about a quarter of an ounce of the oil was put into a six-ounce stoppered bottle, the stopper inserted, and the bottle shaken and moved about so as to facilitate the escape of vapour from the oil; in three or four minutes the stopper was taken out and a lighted match held to the mouth of the bottle: if there was a rush of pale blue light through the bottle, the oil was said to give off an explosive vapour at the ordinary temperature, and to be highly dangerous; only two samples out of the thirty-two exploded at a temperature of 60°, numbers one and three, from London and Liverpool respectively. But this temperature can only be considered as the lowest limit of possible danger, the actual limit of danger is the highest temperature to which, under all ordinary circumstances, the oil is likely to be exposed, either in storing or while burning in the lamp. I concluded from my experiments that a temperature of from 85° to 90° might be calculated upon as often existing in the cistern of a lamp, this high temperature being produced by a long-continued burning of the lamp in a warm room, and on a table near the fire. Any oil, therefore, giving off a combustible vapour and forming an explosive mixture with air at this temperature must be looked upon as unsafe for ordinary use. I tested the remaining thirty samples, and found three which gave an explosive mixture at 85° F. These are numbers four, five, and sixteen, bought respectively in Liverpool, Bradford, Hulme, and Manchester. There are, therefore, five samples really dangerous, and which would require the constant exercise of care and skill to prevent serious accidents.

In order to classify the remaining oils, I submitted the whole of them to a temperature of 100° F., and found four of them to yield an explosive mixture at this temperature,—viz., numbers two, nine, eleven, and twelve; three of these were purchased in Manchester. These oils would be highly dangerous if used in any situation where the temperature was as high as 100°; and, though I consider them as unsafe oils, I cannot call them highly dangerous, because the temperature of 100° is hardly ever reached in this climate, and never in the night when lights are required.

As a next step, I exposed the whole of the remaining oils to a temperature of 120° F., testing the explosibility of the vapour; three samples proved explosive at this temperature,—viz., numbers twenty-one, twenty-five, and twenty-eight, purchased in Salford, Hulme, and Manchester. This temperature seems too high to be reached under any ordinary circumstances in a lamp, and I should consider them as ordinarily safe oils. I submitted the remaining twenty samples to a temperature of 150° F., and found that every one yielded an explosive mixture at that temperature; but this I consider beyond the limits of possible danger, and I consider these twenty as very safe oils.

Of the twenty samples which could not be made to explode at 120°, two were American rock oils, and eighteen were Young's paraffin oils; the whole of Young's paraffin oils tested were safe oils. Out of the twelve samples which exploded at or below 120° all were American oils, seven of which were purchased in this neighbourhood.

To recapitulate: Out of thirty-two samples I consider I found twenty quite safe, three less safe but not really dangerous, and nine too dangerous to be used for domestic purposes; four only of the dangerous oils were purchased in Manchester. Out of fourteen samples of American oil, two were as safe as paraffin oil, three less safe but not dangerous, and nine are to be looked upon as dangerous.

In addition to the explosive method of testing, I have tried the liability of the various samples to burst into flame upon contact with a lighted body. I find this inflammability is in close, if not in exact relation with the explosibility; but from various reasons I do not consider it as good a method of testing. I find none of the samples of Young's paraffin oil to take fire at 130°; out of fourteen samples of American oil eleven inflame at this temperature, and three do not. When the oils are scattered upon linen or woollen rags, at the natural temperature of the air, they burst into violent flame upon the most momentary contact with a lighted match or candle; in this, the most dangerous property of these oils, there is no considerable difference between the best and the worst samples of the American and paraffin oils.

The specific gravity of the samples does not give any idea of their liability to explosion, but it forms a tolerably reliable means of distinguishing between the British paraffin oil and the American substitutions. Eighteen samples of Young's paraffin oil had an average specific gravity of 833; the American oils are generally below 816, but two samples, and those bad ones, were as high as 830; while a sample of coal naphtha, incomparably more combustible and dangerous than any of them, had a density of 865. The boiling point is no better guide, for though volatility is generally in close relation to the boiling point, the vapours of many combustible bodies have so high a diffusive power as to compensate for very high boiling points. For example: A sample of coal naphtha boiling at 260° F. gives an explosive mixture almost instantly, even at the freezing point. The true paraffin oils commence ebullition at about 300°, rising as high as 380° in glass vessels; the worst of the American oils do not commence to boil until at 260°.

With regard to illuminating materials in general, the paraffin and so-called paraffin oils which I have examined, hold a place intermediate between the old lamp oils, which cannot explode in any kind of lamp, and the very volatile and inflammable fluids, such as naphtha, camphine, and spirits of wine, which are liable to explosion at all times, and are employed with this knowledge. The common coal naphtha is infinitely more dangerous, more explosive, and more inflammable than any of the American oils I have examined, yet it is largely consumed as an illuminating material, and with few accidents, because its dangerous properties are known and guarded against. The real danger to society is in selling one species of oil for another; if paraffin were sold for sperm oil it would be as dangerous as if naphtha were sold for paraffin. Genuine paraffin oil is perfectly safe in a paraffin lamp; a majority of the American oils are not safe in this lamp.

While guarding ourselves and the public against a real danger, it would be unwise to join in an undiscerning cry against the new American oils, which promise to become a valuable article of commerce, and which are, in reality, far less dangerous to handle than many a combustible substance largely dealt in and necessary for the wants of society.

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Contributions towards a Knowledge of the Inorganic Constituents of Plants, by Dr. Charles A. Cameron, *M.R.I.A., F.C.S.L., Corresponding Member of the Agricultural Societies of New York, Belgium, &c.*

(*Continued from page 299.*)

Experiments with Manganese.

Manganese is one of the doubtful constituents of plants. In the animal kingdom it has been detected in hair, in human gall-stones, and in the urinary calculi of graminivorous animals; but in few of those instances was it present in weighable amounts. If it were proved that manganese is an essential element in animal nutrition, we should at once admit it to be an indispensable constituent of plants; but it is more than probable that its presence in animal substances is accidental, or rather incidental, and admits of the following explanation:—Iron in its mineral state is almost invariably found in combination with manganese. Now, when we find that human hairs, particularly when of a dark shade, contain a notable proportion of iron, the source of their small proportion of manganese is apparent. Absorbed in its combination with iron from the soil by plants, it passes from them into the animal kingdom. In all gall-stones and urinary calculi in which manganese was detected, iron also was found in much larger amounts. I have sought repeatedly for manganese in the secretions of animals, and I have never yet found that it was not associated with iron, and then only in very minute, generally unweighable, proportions.

In by far the greater number of the statements of the analyses of plants, no mention of manganese occurs. In those published more than a quarter of a century ago, its presence is more frequently alluded to than in the announcements of later investigations. Crasso found 0·82 per cent. of protosesquioxide of manganese in the ashes of the juice of unripe blue grapes, and smaller proportions in different parts of other varieties of the fruit of the vine. In a specimen of prepared flax-stems examined by Sir Robert Kane, 1·09 per cent. of sesquioxide of manganese was found, but accompanied by no less an amount than 13·52 per cent. of sesquioxide of iron. In ashes of the *Acorus calamus* (sweet flag), there is, according to Rüling, 1·418 per cent. of sesquioxide of manganese; in those of the oak, according to Berthier, from 2·25 to 5·70 per cent.; and in the ashes of the wood of Scotch fir, as stated by Bottinger, from 5 to 15 per cent. In the last instance the manganese appears to have completely replaced iron in the fir tree.

In the experiments performed by me, already described, care was taken to separate the iron used in the artificial manure from manganese. In the ashes of the plants to which the iron had been applied, not the slightest trace of manganese could be detected. From this I infer that plants can be perfectly developed without the presence of this substance; but it remains to be determined whether or not it is capable of playing the part of iron in the vegetal economy. The sesquioxides of these metals are analogous in constitution, and in many of their properties, and it is probable that manganese can be substituted for iron in the same way that sodium is known to replace potassium,—not altogether, but to a limited extent.

Experiments with Copper and Lead.

The question whether or not other heavy metals, besides iron and manganese, occur as normal constituents of animal and vegetal bodies, has been repeatedly discussed within the last thirty years without a satisfactory solution having been obtained.

Some years since, several distinguished chemists stated that gold was frequently present in vegetal substances, but later researches have demonstrated the inaccuracy of this statement.

Silver, it would appear from the researches of Malaguti, Durocher, Sarzeau, and other chemists, is a constituent of sea-water, from which it is taken up by sea-weeds. The same chemists assert that silver is also to be met with in minute traces in land plants.

Arsenicum was stated by Orfila to be a constant component of the bones of man and of the inferior animals, but his assertion was proved to be erroneous by the results of the investigation of a commission of the French Academy of Sciences. G. O. Rees, who in England repeated Orfila's experiments, also failed to detect arsenicum. Hodges tells us that during twenty years' experience as a toxicologist, he never met with arsenic in the human body, except when there was reason to believe that it had been accidentally or criminally administered. Daubeny, last year, tried whether or not arsenicum could be absorbed by plants. He watered

vegetables growing in the field with a very soluble arsenical preparation, but in no instance (as analysis subsequently proved) was the metal taken up into the plants. The sulphuric acid employed in the manufacture of artificial manures frequently contains arsenious acid, derived from the iron pyrites, which is so largely used in the sulphuric acid manufacture. E. W. Davy believes that the artificial manures, of which this pyritic acid forms a constituent, contains arsenicum, and that the metal is likely to be absorbed by the plants to which the manure is applied. Horsley agrees with this opinion of Davy's, but other chemists dissent from it.

Copper and lead have been frequently detected in healthy vegetal and animal bodies; but the question whether in such cases their presence was accidental or otherwise has been the cause of many animated discussions. The occurrence of copper in organised structures appears to have been first noticed by Meissner, Bischof, and Sarzeau; the first two named found it in several kinds of plants, the last detected it in coffee and in flesh. Devergie, Barse, and Hervy found it in the ashes of the viscera of different individuals who had died from natural causes. According to Orfila, Lefortier, Deschamps, Harless, and Millon, it is a normal constitutent of the blood and flesh of man, and of the inferior animals. According to Bertazzi, copper is a constant constituent of gall-stones, except in cases where the concretions are colourless. Bertazzi, however, failed to detect copper in the bile, although the copper found by him in the biliary calculi was evidently derived from the bile-pigment. Heller states that the results of his experiments confirm those of Bertazzi; but Hein, who has more recently made a large number of analyses of gall-stones and biliary pigment, found in a few instances traces of manganese, but never any copper. Assuming the analyses of Bertazzi and Hein to be equally correct, the curious fact is demonstrated that the gall-stones of the Italians contain copper, whilst that metal is absent from the biliary calculi of the Germans. Of three gall-stones examined by Sthamer, one only contained copper. Von Bibra found copper in large proportions in the blood of the lower animals, and, in some instances, unassociated with iron,—one of the elements of the blood generally considered as essential to that fluid.* In other parts of the animal which yielded this cupreous, non-ferruginous blood, iron could not be detected. In the blood of those animals (*Conger vulgaris*, *Acanthias zeus*, *Cancer pagurus*) which contained both copper and iron, the amount of the former was in an inverse ratio to that of the latter, Are we to infer from this that copper can be substituted for iron in the blood of certain animals? A very elaborate investigation into this subject was made about five years ago by Odling and A. Dupré. They made about 100 analyses of different animal and vegetable substances, and in nearly every instance found a minute quantity of copper. From the whole liver of a sheep those chemists extracted half a grain of copper, and in the ashes of a specimen of wheat it was found in the proportion of 0·023 per cent. In many other specimens the quantity of metal was weighable. Chevalier sought for copper in the internal organs of animals, and found it in the greater number of instances. Lastly, Melsens informs us that, by the most careful analysis, he was unable to discover this metal in the blood of man, of the horse, or of the dog; Danger and Flandin failed to perceive it in the healthy human body; and Heyl, unlike preceding investigators, could not find it in the ashes of the officinal sponge.

Lead, according to Legrip, is a normal ingredient of the liver, spleen, and other parts of the animal body. In 3300 parts of the ashes of the human liver, this chemist states he found nearly three parts of lead. Millon, Devergie, Hervy, and some others are of opinion that lead is constantly present in small proportion in animal substances; while those chemists who assert that copper is not a component of animal or vegetal substances also exclude lead from the list of the elements of organised bodies.

Rossignon and Deschamps have pointed out the sources of the copper found in animals. The former discovered the metal in gelatine, bread, sugar, coffee, chocolate, and various other food substances; the latter states that all sedimentary deposits contain copper, that the plants grown on these soils take up the copper, and that from the plant it passes into the animal, of which it forms, like iron, a normal constituent.

Having thus briefly passed in review all the statements both in favour of and against the belief in the normal occurrence of copper and lead in plants and animals, I will now detail the experiments which I have made in relation to this question. I have examined, by a great variety of processes, various animal and vegetal substances for copper and lead, and with the following results:—

Of twelve specimens of flour obtained from different sources, four contained neither copper nor lead, one contained lead and copper, and seven contained copper only. In only two of the specimens was the amount of copper weighable; one of these yielded in 400 parts nearly one part of metal. The proportion in the other specimen was $\frac{1}{1100}$. The amount was determined by precipitating the copper electrolytically on a platinum wire, dissolving the deposit in nitric acid, evaporating the solution, and igniting the residual oxide. The figures apply only to the ashes of the flour.

In two samples of oats grown near Malahide, in the county of Dublin, not the slightest trace of either copper or lead was detected. In no instance did I discover either metal in the ashes of sea-weeds, but I invariably found lead in the plants grown near the lead-smelting works at Ballycorus, county of Dublin.

I have sought for these metals in the bodies of six ducks, and obtained the following results:—

1. Lead was not present.
2. Two of the animals contained no copper.
3. In the four ducks in which copper was detected, the metal was not equably distributed. The bones and fat of all, and the muscles of one of them, were destitute of copper; indeed, the metal appeared to have accumulated chiefly in the liver, with the object, perhaps, of its being eliminated from the system by means of that organ.
4. The amount of copper in any organ or other part of the animal was too minute to admit of being weighed.
5. The copper was in that part of the carbonised animal substance which is soluble in water—a circumstance which favours the assumption that it exists in vegetal and animal bodies in what H. Rose terms a teleoxidic state, and that it is not an integral constituent of the tissues, although it may be found in them.

In twenty eggs of hens, ducks, geese, and turkeys, not a trace of either lead or copper was discovered – a result which tends to confirm the statement of Cattanei, that neither of these metals occurs in the bodies of new-born infants or children. The ashes of a large skate, of a

* "If iron be excluded from the food, life cannot be supported."—*Liebig.*

haddock, and of three herrings, afforded no indication of lead or copper. Neither did I detect these metals in the ashes of the plants developed under artificial conditions, and the analyses of which have already been given.

From the general results of all the experiments which have been made as to whether or not copper and lead are normal constituents of plants and animals, I think I am near the truth in eliminating the following conclusions:—

1. That copper and lead frequently, but not invariably, exist in organised bodies.

2. That they are not present in immature or very young animals.

3. That the facts of their not always occurring in healthy animals and vegetables, and never existing in the embryo or very young animal, are sufficient proofs that their presence in animal and vegetal organisms is owing to purely accidental causes.

Nothing is to my mind more unphilosophical than the belief that substances which occur in all but infinitesimal quantities in organised bodies are essential or even useful constituents of those structures. The mineralogist does not consider the traces of earthy matter in the diamond, or the minute proportion of magnesia which even the purest Carrara marble contains, as essential to the constitution of those substances. The purest water in nature contains saline matters of undoubted accidental occurrence. There is not a single instance on record of a mineral of definite composition not containing substances in greater or less proportion which could be separated from it without affecting in any way its constitution. Yet in full possession of this knowledge, the cultivator of biochemistry no sooner discovers that the whole liver† of a sheep contains half a grain of oxide of copper, and that the one four-thousandth part of the ashes of flour‡ is made up of copper, than he forthwith enrols that metal in the list of the necessary elements of plants and animals. Surely it is not the homœopathists who alone believe in the potency of the infinitesimal!

PHARMACY, TOXICOLOGY, &c.

On the Detection of Strychnia as a Poison, and on the Influence of Morphia in Disguising the Usual Colour-test, by John J. Reese, *M.D., of Philadelphia.*

The progressive increase in the number of deaths within the last few years, occasioned by strychnia, used either for homicidal or suicidal purposes, is a subject demanding the careful consideration both of the toxicologist and the medical jurist; and every circumstance connected with the detection of this most potent agent cannot fail to interest the medical profession at large.

The author lately had occasion to investigate this subject closely in connection with a case of alleged poisoning by strychnia. A man was indicted, at the April term, 1860, for the murder of his wife, in Perry County, Pa. Although dying under suspicious circumstances, no post-mortem examination was made until six weeks had elapsed, when the body was exhumed. The stomach and a portion of the small intestine were carefully tied, and, along with the adhering pancreas, were conveyed to Philadelphia, and placed in possession of Dr. Reese for chemical examination. He found the organic structure but little changed in appearance (eight weeks after death), and the contents of the vessels consisted of four or five fluid ounces of a thick brownish homogeneous fluid.

Three separate analyses were made. The contents of the stomach—contents of the intestine—and the tissues themselves, each of which was carefully repeated; yet he "entirely failed to detect any evidence of the presence of strychnia, either by the bitter taste of the final extract, or by the very delicate colour-test employed."

Inasmuch as the moral circumstances of the case and the symptoms pointed to death by strychnia, the author naturally sought for an adequate cause to explain the failure to detect the poison. After giving due weight to the effects of elimination by the excretions during six hours that the patient survived, and to the agency of decomposing action during six weeks' inhumation, the author remarks on a circumstance in connection with the case which he viewed with especial interest,—namely, the fact that the woman had taken, just before death, by the advice of her medical attendant, a quarter of a grain of morphia, with a little ipecacuanha; but she did not vomit. Now the value of this fact is just this: It has been ascertained that the presence of morphia and other substances has the effect of disguising and entirely neutralising the usual colour-test used for detecting strychnia; so that the latter might be undoubtedly present, yet if morphia were also present at the same time, the strychnia could not be discovered. It will be readily admitted that this is a point of the extremest importance to be settled by the chemist in medico-legal researches. It is one to which no very special attention has hitherto been given. It is merely mentioned as a casual fact in the various works on toxicology, but the only actual experiments recorded, to my knowledge, are those published by Dr. T. G. Wormley, in the *Ohio Medical and Surgical Journal*, September, 1859, in which it is stated that when the morphia exceeds the strychnia in quantity, the possibility of discovering the latter by the colour-test diminishes. Dr. Reese accordingly undertook a series of experiments to satisfy himself in regard to this very important subject, with the following results:—

Experiment 1.—One-tenth of a grain of pure strychnia was added to about twelve ounces of water, into which were put several ounces of fresh beef, finely cut up, together with some starch, a little common salt, and a few drops of acetic acid (the object being to represent, as closely as possible, the contents of a human stomach after a meal). The whole was digested on a sand bath for twelve hours at a moderate heat. It was then strained, pressed, and filtered; and afterwards evaporated down to a very small bulk. It was next divided into two separate portions, each of which, of course, would contain the $\frac{1}{20}$th of a grain of strychnia. One of these portions was treated after the process known as Graham and Hoffman's (the alkaloid being removed by animal charcoal, and finally extracted by ether). Here a drop or two of the ethereal solution, representing about the $\frac{1}{40000}$th to the $\frac{1}{60000}$th of a grain, gave distinct evidence of strychnia by the usual colour-test. The second portion of the evaporated solution was divided into two parts, each of which would of course contain the $\frac{1}{40}$th of a grain of the alkaloid. The first of these was treated according to the process of M. Staas, in which ether is used as the ultimate solvent: and the second part after the process of Mr. Prollius, in which

† The liver of a sheep weighs, say about 2lbs. = 14,000 grains, of which the oxide of copper constitutes the 1-28,000th part!

‡ The per-centage of ashes in wheat grain varies from 1 to 3; assuming the proportions to be 2 per cent., the amount of copper 1-4000, found by me in the ashes, made up only the one-two hundred thousandth part of the fresh grain.

the ultimate solvent was chloroform. In both instances I obtained the most satisfactory proofs of the presence of strychnia, operating upon a single drop of the fluids —which would represent, certainly, not over the $\frac{1}{100000}$th of a grain of strychnia.

Experiment 2.—This was a repetition of the former experiment, except that the quantity of strychnia used was much smaller—only the $\frac{1}{100}$th of a grain. After treatment by Staas' process, and on concentrating the ultimate ethereal solution, the presence of strychnia was manifested both by the colour-test and by the bitter taste of the extract. Here the quantity of the poison operated upon was less than the $\frac{1}{100000}$th of a grain.

Experiment 3.—This was an exact repetition of Experiment 2, except that to the $\frac{1}{100}$th of a grain of strychnia three times that quantity of morphia ($\frac{1}{33}$rd of a grain) was added. On treating this by Staas' process, as in the preceding cases, I could not discover the slightest trace of either strychnia or morphia, even after the ultimate ethereal solution was concentrated to a very small bulk by evaporation.

As the first two experiments were precise counterparts of those employed in the analysis of the stomach, the author feels justified in believing them to be delicate and reliable, and that the poison should have been isolated if present, unless its presence was masked. The third, he thinks, seems to prove most unequivocally that morphia, when present in excess along with strychnia, has this property of concealing the latter from the usual colour-test.

Experiment 4.—This was also a repetition of Experiment 2, except that to the $\frac{1}{100}$th grain of strychnia, the $\frac{1}{50}$th grain of morphia was added instead of the $\frac{1}{33}$rd—or double instead of treble the quantity. Here, likewise, there was a total failure to discover the poison.

Experiment 5.—This was a repetition of the last, except that only $\frac{1}{100}$th grain of morphia was added to the $\frac{1}{100}$th grain of strychnia, or an equal portion. The result here was that I obtained the faintest possible evidence of the presence of strychnia, and only after repeated trials.

From these last experiments I think we may conclude that the influence of morphia in preventing the detection of minute quantities of strychnia, in the presence of an organic fluid, depends upon the relative quantity of the two alkaloids; the strychnia being not discoverable when the morphia is in excess, and barely discoverable when in equal quantity.

Dr. Reese next instituted a set of experiments with a view of ascertaining the effect of morphia in disguising the presence of strychnia in perfectly pure solutions, free from organic mixtures, and the result pointed to the fact that the minuteness of the proportions detectible by the colour-test was inversely as the proportion of morphia was increased, as will be shown in the following tabular view:—

When the proportion of the two alkaloids was—

				Of a grain.
1 strychnia to	1	morphia,	he was able to detect	1-500,000th
1 ,,	2	,,	,,	1-300,000th
1 ,,	3	,,	,,	1-150,000th
1 ,,	4	,,	,,	1-100,000th
1 ,,	5	,,	,,	1-80,000th
1 ,,	10	,,	,,	1-10,000th
1 ,,	20	,,	,,	1-5000th

Beyond this the experiment was not pursued, Dr. Reese being satisfied that Wormley's observation,—that the action of strychnia became more difficult as the proportion of morphia increased,—was correct.

But an important point to be determined was, how a quantity of strychnia, almost infinitesimal, might be effected by an amount of morphia, which, though small in itself, yet bore a relatively large proportion to the strychnia: just precisely such a state of things as would be likely to be met with in an analysis of the human stomach, and to decide this Dr. Reese tried the following experiments on three half-grown cats:—

Half a grain of pure strychnia was given to the first animal, and in eleven minutes it died in a violent convulsion. The poison was very easily discovered in its stomach, by the usual tests, on the following day. To the second animal a quarter of a grain of strychnia and the same quantity of morphia were given; and, somewhat to my surprise, the animal was deeply convulsed in six minutes, and died very quickly. Here the morphia, so far from counteracting the toxic influence of strychnia (as might have been inferred from its opposite physiological influence), seemed actually to have increased its effects. The stomach of the second animal was likewise examined; but I obtained scarcely-recognisable evidence of strychnia, owing, doubtless, to the influence of the presence of the associated morphia. It will be recollected that the quantity of morphia in this case was just equal to that of the strychnia. To the third animal the $\frac{1}{20}$th of a grain of strychnia and the $\frac{1}{10}$th of a grain of morphia (double the quantity) were administered. Convulsions took place in about fifteen minutes, and death in half an hour. The stomach was examined by Staas' process, as in the other cases, but with a total failure to detect the poison by the colour-test; although the bitterness of the extract, and its decided action in producing tetanic convulsions in a number of frogs, clearly established its presence.

From all the foregoing experiments, it appears to be conclusively established, that morphia does unquestionably possess the power, when present in excess, of completely disguising the colour-test of strychnia; and this is emphatically the case when they are associated in organic mixtures, as in the contents of the stomach. Consequently, this fact should always be taken into account in medico-legal investigations.

The moral evidence in the foregoing case was very strong, and although Dr. Reese's evidence entirely failed to establish the presence of strychnia, yet his evidence of the masking effect of morphia, taken in connection with the fact of the patient having taken morphia just before death, and the strong moral evidence, caused the conviction of the accused, who subsequently made a full confession of the crime.

The remainder of Dr. Reese's paper is occupied with a detailed explanation of the best modes of applying the colour-test, with the value of bitterness as a collateral test, with the physiological or frog-test of Dr. Marshall Hall, and with the microscopy of strychnia.—*American Journal of Pharmacy.*

Growth of Cinchona in India.

DR. ANDERSON has returned to Calcutta from his mission to Java, with the large number of 412 cinchona plants of three species, and with half a million of seeds. The cultivation of cinchona in Java, which began with 139 trees in 1855, has hitherto been most successful, but the Dutch do not possess many specimens of the variety whose bark yields the largest proportion of quinine. Dr. Anderson was enabled to supply them with some of the best specimens in return for their liberality and courtesy. Of one species the Dutch have now no less than a

million of plants, and of two or three others they possess several thousands. In June, 1857, the whole number they had was only 300, so rapidly have they propagated themselves. The well-known German naturalist, Dr. Junghuhn, is in charge of the plantations, and he was recently joined by Dr. De Vry, a chemist not unknown in England to the members of the British Association. The report of the first of these gentlemen, now before us, details, with scientific prolixity which we shall not inflict upon our readers, the various steps of the experiment. South-west of Tjibodas, 4100 feet above the sea, in the very centre of Java, natural forests extend for miles, the soil consisting of loosely-heaped blocks of trachytic larva. To this spot, under the shade of wide-spreading branches, the plants which had withered in a more exposed locality, were transferred in October, 1857. Six months after, they began to die in their new habitation, and the cause was soon discovered in a small beetle of the *bostrichus* species, never before seen in the Java forests, which deposited its brood in the trunk. After many difficulties and experiments with soils and localities, the Malawar mountains have proved the best spot. The slightest differences of altitude, light, and temperature, affect the *calisaya* variety. The hardiest, though least valuable, is the *lucumæfolia*; but if the former be planted early, in good, loose, forest soil, and moderate shade, particularly in the region of from 5000 to 5700 feet above the sea, it grows up as tall and luxuriantly as any. Each capsule of each plant contains an average of twenty-five seeds. One *calisaya* yielded 485 capsules, and each good plant may be said to yield 1300 seeds. Of 2000 well-developed seeds sown with care on cleared soil, under the shade of trees, only one grows to a real plant; for the slightest touch, even of a drop of water, or a crawling worm, kills the root of the young germ. At first the plants grow slowly, but after two years they are two feet high, and then shoot up with great rapidity if raised from seed. The plants thus successfully introduced, an important question arises, Will they yield as much quinine as in their native home in Peru? To test this Dr. De Vry was sent to Java, and he records with no little triumph how, on July 21, 1858, two and a-half years after the first plants were introduced, he was enabled to produce a pure white crystallised quina alkaloid from the bark of one of those stems which the beetle had destroyed. Soon after he obtained from a *calisaya*, not five years old, the same per-centage of quinine which the Bolivian trees yield, or 3·12. At the end of 1861 the experiment in Java, only six years old, has thus succeeded most completely. Many of the trees are now thirty feet high.

This is full of hope for India, where the plant is now being grown under exactly the same conditions. The total number of plants is 5847, and more than half are of that red bark variety which is most valuable, the dry bark selling at from 2s. 6d. to 8s. 9d. per lb. Rich as are the forests of Bolivia, from which the world draws its sole supply at present, they will not last long. Formerly the trees were everywhere found around the inhabited parts of the region; but now, to meet with one of a tolerable size, it is necessary to penetrate several days' journey into the forest. What India is doing as a substitute for North America in rice and cotton, she must become for South America as an exporter of quinia bark. We believe a nursery will soon be established on Khasai Hills. In less than ten years India should supply herself with all the quinine she needs. In a few more, sulphate of quinine, as well as quinia bark, should bulk largely among her exports.—*Friend of India.*

INTERNATIONAL EXHIBITION.

Chemical Substances and Products.—Pharmaceutical Products and Foods.

(*Continued from page* 302.)

WE may leave Sub-class B, of Class 2, with the safe assertion that it is a display highly creditable to English pharmaceutists. We have a very high opinion of English pharmacy. Less fanciful in their composition than French or German medicines, the Galenical preparations of our Pharmacopœias, generally speaking, contain the medicinal virtues of a substance in a simple and easily-available form, while the extra pharmacopœial novelties introduced by individual pharmaceutists often prove of great value, and soon take their place as established remedies,—witness the scaled preparations of iron, quinine, and strychnia, and the syrups of the phosphates and hypophosphites. We sometimes look round a pharmaceutical chemist's shelves, and wonder how it is that people should suffer and die, convinced that it cannot be the fault of the pharmaceutist. Before leaving the sub-class, however, we must notice the case of Messrs. Bullock and Reynolds (648). English pharmacy owes something to Mr. Bullock. It was he, if we do not mistake, who first brought out scaled preparations in their present elegant form, and he here exhibits some novel specimens, scaled, we imagine, on glass tubing, which gives them a very pretty appearance. Some desiccated biles, various hyocholates, and a specimen of hyodyslysin, $C_{50}H_{28}O_6$, give the case an interest to physiological chemists as well as pharmaceutists. Mr. Dickinson, in 652, exhibits a variety of "glycerides," intended, in some instances, to replace syrups, but not half so nice. When substances are soluble in syrup we see no advantage in the use of glycerine. There are some remarkable samples of "liquors" in Mr. Bastick's case (646). Those of hyoscyamus, aconite, and belladonna are almost colourless, but are said to contain all the active ingredients of the plants. There is also a colourless liquor digitalis, which, unlike other preparations of foxglove, it is said can always be relied on. The advantages of these colourless solutions we believe to be doubtful. Active medicines are best left in their native ugliness and nastiness. We look on these preparations as illustrations of misapplied ingenuity, except, of course, such instances as that of the liquor digitalis, which, if it can indeed be always relied on to do all that the doctor expects and the patient hopes, is a most remarkable and valuable preparation.

Passing for a moment to Sub-class A, we must notice the case of Messrs. Morson and Son, which needs no commendation. The pharmaceutical chemicals here shown look all that can be desired, and the spectator is gratified by the sight of a specimen of narceine such as he has probably never seen before. Another rarity is a large sample of papaverine. The case also contains some Japanese purified peppermint, and a fine specimen of crystallised citrate of lithia.

For a very fine sample of crystallised codein, the visitor must go to French Department No. 204, in which case he will also see the only specimen of igasurine we have yet noticed in the Exhibition. Complete as the English collection of alkaloids is, it seems to want this one. Having noticed the labelling in our own department, we may observe that M. Menier has departed from the usual practice of his countrymen, and given his goods strange names. Our old acquaintance, nitrate of nickel, figures here as Nitras Niccollicus. This case

contains also what may be looked upon as some of the most remarkable things in the building; we mean Berthelot's synthetical preparations. Here is "Alcohol, per syntheticam artem generatum," the union of the bicarburet of hydrogen with the elements of water. There are also stearine, and other natural organic products prepared by synthesis. Cases near this, 206, 207, and 208, contain some clean chemicals. The French seem to monopolise the manufacture of picric acid, some very fine samples of which in all conditions are exhibited in case 191 by M. Perra. In 167 there are other fine samples of picric acid, and a specimen of azo-sulphuric acid.

The French colonial display deserves a careful inspection. Algeria would appear to be a most productive country. There are hops, some, at all events, of excellent colour, as well as some of very bad,—we cannot answer for their flavour; several spices, opiums produced in the department of Constantine, and a small-leaved senna indigenous to Algiers, which we do not remember to have seen before. Algeria well cultivated would make France independent of half the world.

Perhaps we ought not to delay calling the attention of our readers to that part of Class 1 which comprises chemical utensils. On entering the Eastern Annex, the chemist will be struck by the display of crucibles (265) by the Patent Plumbago Crucible Company. The large melting-pots of the patent composition shown seem to have wonderful durability; and so do the pots of Mr. Smaile, of Newcastle (330), which are said to be only one-fourth the cost of the plumbago crucibles. The pots of both these exhibitors are remarkably well made,—a remark which will also apply to the Cornish display by Mr. Juleff (183).

The display of chemical glass in the English department is not remarkable. Powell's show comprises some well-made articles of good clear glass; the surgical glass exhibited by Mr. Wheeler (6795), comprehends a great variety of syringes; and Messrs. Kilner show some well-made useful productions. But there is nothing in the chemical glass of our department to compare with a large retort in the Austrian (653). For size and beauty of make, this is the finest vessel of the kind we have ever seen. The rest of the articles on that stand are remarkably good.

Jottings from the International Exhibition.

Had it not been for the watchfulness of the officials, the International Exhibition would have lately stood a good chance of being burnt down on very philosophical principles. In the Japanese Court, Messrs. Baring Brothers exhibit two extraordinary quartz spheres, four or five inches in diameter, ground and polished with mathematical nicety. These spheres stood side by side on a mahogany stand in the Japanese Court, attracting but little attention from the public, until one very sunny day a visitor suddenly rushed to the office of the department with the alarming intelligence that "the two glass globes had caught fire!" The officials, on going to the spot, found the stand in a blaze, the sun having shone directly through the globes, which, of course, acted as burning-glasses, setting the woodwork on fire. There are now two holes in the mahogany stand large enough to insert the top of the finger. These holes are very interesting, as they are each double, showing perfectly the double refracting properties of the quartz. The spheres have been removed into the Chinese Court, that part of the building being quite in the shade.

Every chemist should perform a pious pilgrimage to the Belgian Gallery, where he will find the two balances made by the celebrated Sucré, of Brussels, and used by M. Stas in his recent researches on the atomic weights.

In the Austrian mineral department there is a most magnificent collection of several hundred perfect crystals of various salts made by M. Von Hauer, of Vienna. We hear that they have already been purchased by the trustees of the British Museum. Amongst them are several specimens of concrete crystals, illustrative of their isomorphic properties. Amongst the chemicals in this department is an interesting specimen of chloride of cæsium and rubidium contributed by Professor Schroetter.

The jury work has brought to the Exhibition an immense number of scientific men of eminence. During a visit to the British chemical department a few days since, we encountered in a brief space of time MM. Stas, Fritsche, St. Claire Deville, Wurtz, Von Fehling, Hofmann, Balard, Boussingault, Payen, Persoz, Peligot, Miller, Brodie, Odling, Regnault, Forchhammer, Schroetter, Fremy, and many others who constitute the aristocracy of science.

The specimens of platinum, iridium, osmium, ruthenium, palladium, silicon, boron, and many other rarities exhibited by Messrs. Johnson and Matthey in Class 1, are well worth close examination. A mass of melted platinum, weighing nearly 2 cwt., is the gem of the collection. It was lately fused at the laboratory of Messrs. Johnson and Matthey by M. St. Claire Deville, who, we regret to hear, has been suffering severely from the effects of the fumes of osmic acid given off during the process.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, May 29, 1862.

The Master of the Mint *in the Chair.*

A Paper, by Dr. W. Crum, "*On the Action of Mordants in Dyeing*," was read by the Secretary. It had been supposed that a combination of a chemical nature took place between the fabric and the dye stuff or mordant, but this appeared to be erroneous; on the contrary, in those cases in which the dye was fixed without a mordant, the attraction was simply cohesive, and similar to that exerted by all extended surfaces. The structure of the fibre bore out this view, for on examining cotton under the microscope it was seen to be composed of flattened tubes with translucent walls which were permeable to fluids, and these acted in the same manner as any other porous structure, such as charcoal, &c. When mordants were used, they were often deposited within the fibre and retained there mechanically, and afterwards, by combining with the dye, served to fix this in the material. This explanation was rendered more probable from the fact that *dead cotton* did not take the dye. The dead cotton, which was found occurring in small quantities in company with the ordinary material, seemed to consist of fibre which had been imperfectly formed; under the microscope, it was seen to have no central tube, and was so transparent as to be almost invisible; this accounted for its refusing to absorb the dye. An example of an attraction between the fibre and the dye was afforded by a solution of reduced indigo; when a piece of cotton was immersed in such a solution it absorbed a certain quantity into its substance, and this, on exposure to air, became blue; by immersing successive pieces in the same solution the whole of the indigo might be removed. Tannic acid was attracted in

the same manner by the fabric, and this, when immersed in a solution of binoxide of tin, combined chemically with it and fixed it in the cloth, and the binoxide of tin afterwards acted as a mordant. When acetate of protoxide of iron was employed, the oxide of iron was deposited in the fibre and held there mechanically, and afterwards combined with the dye stuff. In some cases the mordant and dye could both be applied at the same time, but it was necessary that both should be in solution.

The Chairman remarked that it was a circumstance worthy of notice that the mordants were all colloids.

Mr. Malone said that he had lately examined the properties of precipitated gold, which was known to remain for a long time suspended in water, and had found that the fungus that sometimes grows in distilled water had an attraction for it, and collected it gradually on its surface; also that when the precipitation was made in an ethereal solution the subsidence of the precipitate took place readily, from which he had concluded that the precipitate possessed properties analogous to those of colloids, and that ether had the power of coagulating it.

The next Paper was by Professor Bloxam, "*On the Capacity of Arsenious Acid for Bases, and on some Arsenites.*" Arsenious acid was usually considered bibasic, but the experiments he had made tended to show that it could neutralise three equivalents of base. He first determined the amount of carbonic acid set free by arsenious acid from alkaline carbonates, and found that at a temperature of 212°F., if the arsenious acid were in excess, two equivalents of arsenious acid displaced one equivalent of carbonic acid; but with excess of the carbonate, one equivalent of the acid displaced two-thirds of an equivalent of carbonic acid. At a red heat a similar result was obtained with excess of acid, but with excess of base one equivalent of carbonic acid was displaced. The action of the acid on the hydrates of the alkalies was next investigated; but under these circumstances the acid was oxidised, hydrogen being evolved. Salts were then prepared by mixing solutions of arsenious acid and alkalies; in this manner the crystalline salt, $KO,HO,2As\,O_3 + Aq$, was formed; the water of crystallisation could be driven off at 212°F., and at a higher temperature the remaining water disappeared, and a biarsenite remained in a beautifully transparent state. A sesqui-arsenite, $2KO,3AsO_3 + 3HO$, and the corresponding soda salts, were obtained in a similar manner. The silver salt was tribasic, amorphous when first precipitated, afterwards becoming crystalline. There were three lead salts, a monobasic and bibasic salt, and a third which was obtained from an acid solution and had the composition, $3PbO,3HO,2\,As\,O_3$. The zinc salt was tribasic, even when deposited from an acid solution, and under the microscope was seen to consist of a number of *spheres*. The magnesia salt, $2Mgo,HO,\,As\,O_3 + Aq$, the copper salt, $2CuO,HO,\,As\,O_3$, and the baryta salt, $BaO,2HO,\,As\,O_3$, had also been prepared.

Mr. Gladstone then read a paper "*On the Reciprocal Decomposition of Salts in Solution.*" The question he had endeavoured to decide was, whether, when two salts which might be represented by the formulæ, M R and M′ R′, were mixed, the same result was obtained as with M′ R and M R′. To determine this, salts were mixed in which the resulting change caused a difference in colour, and in these cases no difference could be detected in the resulting mixtures; in some cases a coloured salt was added to the resulting mixture, and the change in colour observed. Dialysis was also employed, and from the results it was concluded that if a sufficient time for the rearrangement of the bases and acids was allowed, no difference could be detected in the resulting mixture. To determine quantitatively the amount of decomposition the polariscope was employed, and the substances which suggested themselves as appropriate for the purpose were nicotine and tartaric acid; the salts of nicotine do not rotate the plane of polarisation so much as the base itself, and a mixture of nicotine with chloride of ammonium had a greatly diminished power of rotation; with chloride of sodium the decomposition was not so great, and the quantitative determination was very decisive. Tartaric acid, however, did not give satisfactory results, but no doubt other substances could easily be found for the purpose.

The next paper was by Mr. Riley, "*On the Presence of Titanic Acid, and Means of Determining Iron, in Clay.*" From an examination of many samples of clay he had come to the conclusion that titanic acid was always present in this substance; in the ordinary method of analysis the titanic acid was sometimes left with the silica, at other times it passed into solution and might be separated by neutralising, boiling with sulphurous acid, and fusing the resulting precipitate with bisulphite of potash. The iron could not be accurately determined by precipitation by potash, for other matters were carried down and increased the weight of the precipitate; standard solutions, either of bichromate or permanganate of potash, were preferable. He had found that in all cases in which the blast furnaces produced good iron, titanium was present.

A new maximum and minimum mercurial thermometer was exhibited by Mr. W. Simmonds. It consisted of a tube bent upon itself so that the two portions were parallel; each end being terminated by a bulb. One of the bulbs and a portion of the tube were filled with mercury, and upon this some sulphuric acid was placed; two little floats were immersed in the sulphuric acid, one of which was placed in contact with the mercury, the other just beneath the surface of the sulphuric acid; the first acted as the maximum, the second as the minimum index; the instrument was arranged so that the two limbs were horizontal.

NOTICES OF PATENTS.

Grants of Provisional Protection for Six Months.

281. Marc Antoine François Mennons, Rue de l'Echiquier, Paris, "Improved processes for the recovery of the oleic acid contained in the residual scouring waters of woollen and other textile materials or fabrics."—A communication from Edmond Lepainteur, Gallard, France.—Petition recorded February 1, 1862.

1222. Lachlan McLachlan, Manchester, "Improvements in governing or regulating light used for taking photographic portraits and other photographic pictures, part of which improvements is also applicable to lighting picture galleries."

1232. Francis Gybbon Spilsbury and Frederick William Emerson, Stratford, Essex, "Improvements in the treatment of fusel oil, and for various applications of the same to useful purposes."

1242. John Fletcher, Farnham Place, Southwark, Surrey, "Improvements in the apparatuses for treating saccharine liquids."—Petition recorded April 28, 1862.

1288. William Bickford Smith, Camborne, and William Bennetts, Tucking Mill, Cornwall, "Improvements in the method of, and apparatus for, preventing the injurious effects occasioned by smoke, sulphur, and the deleterious gases which escape from stacks, chimneys, calcining houses, chemical and other furnaces."—Petition recorded May 1, 1862.

451. Emil Moritz Stoehr, Manchester, "Improvements in the manufacture of manganese, and in the combinations of manganese with other metals."—A communication from Oscar Eugen Prieger and Ferdinand Carl Prieger, Schloss Gereuth, near Ebern, Bavaria.—Petition recorded February 20, 1862.

714. Constantine Nicolaus Kottula, Bell Isle, London, "Improvements in the manufacture of combined soaps."—Petition recorded March 15, 1862.

1038. Andrew Trimen, Adam Street, Adelphi, London, "The protection and solidification of magnesian limestone and other stones, and for the prevention of the passage of water through the same."—Petitions recorded April 10, 1862.

1105. Matthew Cartwright, St. John's Row, Hoxton, London, "Improvements in the manufacture of models, and of plates or pieces for artificial teeth, and in combining or amalgamating india-rubber and gutta-percha with metals for the manufacture of artificial plates or pieces, and for other purposes."

1115. Charles Denton Abel, Southampton Buildings, Chancery Lane, London, "Improvements in the manufacture and production of the chromates and the bichromates of potash and of soda."—A communication from Louis Alexandre Taillandier, Argenteuil, France.

1118. William Hodgson Hutchinson, Bury, Lancashire, "Improvements in the manufacture of ammonia or its salts, and cyanogen or its compounds, from refuse gluten."

1127. Charles Denton Abel, Southampton Buildings, Chancery Lane, London, "Improvements in the manufacture and production of certain alloys containing cadmium."—A communication from Henri Catherine Camille du Ruolz and Anselme Louis Marie Fontenay, Paris.

1137. Edwin Dove, Hunter Street, London, "Improvements in matches and fusees, and apparatus for containing and igniting the same."

50. Louis Wunder, Leignitz, Prussia, "Improvements in the manufacture and composition of soap, applicable especially for shaving."—Petition recorded January 8, 1862.

139. Thomas Roberts and John Dale, Manchester, "Improvements in the manufacture of gunpowder."—Petition recorded January 18, 1862.

1177. William Moir, Manchester, "An improved instrument for ascertaining the specific gravity of liquids."

1195. William Denny Ruck, Duke Street, London Bridge, "The manufacture of grease from coal, tar, coal oil, creosote, or dead oil."

1217. Charles Reed, Kintbury, Berkshire, "A new method of treating the sorghum saccharatum or holcus saccharatus in order to obtain saccharine liquor and pulp therefrom."

1231. Squier Cheavin and George Cheavin, Boston, Lincolnshire, "Improvements in filtering and purifying water, and in apparatus employed therein."

1235. Gustav Bischof, jun., Swansea, "Improvements in treating solutions containing copper and silver, or either of them, to obtain metallic copper and silver."

1304. Alfred Vincent Newton, Chancery Lane, London, "Improved electrical apparatus applicable to the lighting of gas."—A communication from Robert Cornelius, Philadelphia, U.S.—Petitions recorded May 2, 1862.

1332. Christopher Binks, Parliament Street, Westminster, "Improved methods of obtaining hydrogen gas and certain gaseous compounds of hydrogen and of carbon."

1350. John Henry Johnson, Lincoln's Inn Fields, London, "Improvements in the manufacture and production of minium or red lead."—A communication from Charles Louis Pierre Burton, Rue St. Sèbastien, Paris.

1352. John Henry Johnson, Lincoln's Inn Fields, London, "Improvements in the manufacture of soda and potash, and of their carbonates."—A communication from Charles Louis Pierre Burton, Rue St. Sèbastien, Paris.

1366. Richard Archibald Brooman, Fleet Street, London, "An improved box and apparatus for containing and igniting matches."—A communication from Louis Leonard Poulain, Presles, France.

1378. William Southwood, Kensington, Middlesex, "Improvements in machinery for pulverising ores and extracting metals therefrom, part of which is applicable to breaking stones."

1188. William Edward Newton, Chancery Lane, London, "An improved fertilising composition."—A communication from John McAuley Gallacher, Roxbury, Norfolk, Massachusetts, U.S.

Notices to Proceed.

627. William Noy Wilkins, St. John's Wood, Middlesex, "Improvements in the manufacture of pigments for oil and water colours."—Petition recorded March 8, 1862.

28. James Whitton Arundell, Gresham House, Old Broad Street, London, "An improved method and improved apparatus for treating and dressing ores and minerals, particularly applicable to tin, lead, copper, zinc, and iron ores."—A communication from Martin Nueburg, Kalk, Rhenish Prussia.

150. John Stenhouse, Upper Brunswick Terrace, Barnsbury Road, London, "Improvements in the protection of metallic surfaces, and in rendering certain substances less pervious to air and moisture."

281. Marc Antoine François Mennons, Rue de l'Echiquier, Paris, "Improved processes for the recovery of the oleic acid contained in the residual scouring waters of woollen and other textile materials or fabrics."—A communication from Edmond Lepainteur, Gallard, Longeron, France.—Petitions recorded February 1, 1862.

378. Marc Antoine François Mennons, Rue de l'Echiquier, Paris, "Improvements in the disinfection of animal excretions, and in the extraction therefrom of fertilising elements for agricultural purposes."—A communication from Alfred Marie Celestin Edgard Goussard, Rue d'Enghein, Paris.—Petition recorded February 13, 1862.

58. Henry Cook, Manchester, "An improved mode of, and apparatus for, transmitting despatches and small articles by the agency of electricity."—A communication from Gaetano Bonelli, Milan, Italy.—Petitions recorded January 8, 1862.

59. Charles William Siemens, Great George Street, Westminster, "Improvements in the means and apparatus employed for insulating and protecting telegraph conducting wires, and in apparatus for working the same."—Partly a communication from Dr. Werner Siemens, Berlin, Prussia.—Petition recorded January 9, 1862.

123. Thomas Myers, Bloomsbury Square, London, and Edward Myers, Millbank Street, Westminster, "An improved composition for preventing rust on bright steel, iron, brass, or metal."—Petition recorded January 17, 1862.

220. Arthur Herbert Church, Great Portland Street, St. Marylebone, Middlesex, "Improvements in the means of preserving stone, brick, slate, wood, cement, stucco, plaster, whitewash, and colour-wash from the injurious action of atmospheric and other influences, also in the application of colours to the surfaces of stone, brick, slate, wood, cement, stucco, mortar, clay, plaster of Paris, whitewash, and colour-wash, and in the retention of such colours thereon."—Petition recorded January 28, 1862.

235. William Clark, Chancery Lane, London, "Improvements in the disintegration and bleaching of textile materials for the manufacture of paper."—A communication from Jean Baptiste Michel Auguste Siry, Boulevart St. Martin, Paris.

338. Mark Antoine François Mennons, Rue de l'Echiquier, Paris, "Improvements in the treatment of coprolites, and other fossil phosphates of lime."—A communication from Guillaume Louis Edouard Buran, and Louis Goupy, Rue du Grand St. Michel, Paris.—Petition recorded February 10, 1862.

304. Alfred Vincent Newton, Chancery Lane, London, "Improved electrical apparatus applicable to the lighting of gas."—A communication from Robert Cornelius, Philadelphia, U.S.—Petition recorded May 2, 1862.

Invention Protected for Six Months by the Deposit of a Complete Specification.

1244. William Taylor Glidden, Massachusetts, U.S., "A new and useful mode of restoring phosphatic guano."—A communication from Louis Harper, Brooklyn, New York, U.S.—Deposited and recorded April 29, 1862.

CORRESPONDENCE.

Downing v. *Chance.*

To the Editor of the Chemical News.

Sir,—A friend has directed my attention to a passage in the Chemical News of May 17, where, speaking of chemical manufactures and noxious vapours, you say:—"We have seen recently that when they have taken every precaution it leaves them quite at the mercy of wonderful chemists, endowed with marvellous vision, who see bluish vapours rise whenever they place on the ground, within sight of a soda factory, a piece of filtering paper soaked in ammonia." You go on to speak of Messrs. Chance, from which I infer that you allude to the evidence given in the case of Downing *v.* Chance, and as I was one of the witnesses who saw and examined the vapours alluded to, that I am one of your "wonderful chemists."

As regards the insinuation of animosity towards Messrs. Chance, I beg to state that ever since I have been engaged in my present office in Birmingham I have been in friendly communication with the Messrs. Chance and many of their family connections, and have received from them so many marks of courtesy and kindness, that such conduct as you impute to me would have been grossly ungrateful as well as unjust; that, instead of the feeling which your words imply, I entertain the greatest respect and regard for those gentlemen, both as private individuals and as manufacturers, and I consider their chemical and their glass works as about the best conducted in the Kingdom. I hesitated for some days before consenting to give evidence on this trial, and, in the mean time, consulted some mutual friends, and ascertained by very careful investigation what was the character of the plaintiff, and whether there was any possibility of the case being made up for annoyance sake, or with the view of getting heavy damages, or anything of that sort, and it was only when I learned that Mr. Downing was only seeking fair compensation for the amount of damage suffered somehow that I went into the chemistry of the matter, to ascertain how far the damage was traceable to the fumes from Messrs. Chance's works.

As regards the "marvellous vision," every chemist knows the reaction of hydrochloric acid upon ammoniacal vapours.

Thirdly: I need only state that the very important point as to the position in which I stood when making my experiments, its relation to the stacks, the direction of the wind, the deflecting influence of the mound of waste, and the relative positions of other factories, were very properly made the subject of a most rigid cross-examination by the defendant's counsel, Sergeant Pigot. The cross-examination on these points lasted about three-quarters of an hour, and resulted not only in the full and further confirmation of the accuracy of my experiments, but led to some very strongly complimentary remarks being made upon my evidence by Mr. Huddleston in his address to the jury.

I hope, Sir, that since you have so freely and lightly brought my name and my evidence before your readers, and have done me much injustice thereby, you will insert this letter. I shall also be much obliged if you will explain wherein lies the "curiosity" of my evidence on the trial, Downing *v.* Chance.

As regards that on the other trial, I am not at all surprised that you should think it curious if you believe in the accuracy of the report which you published. As you prefaced that report with an expression of your own doubts as to its accuracy, I took no notice of it at the time, especially as I had made some notes for a paper upon an unexpected reaction which I had stumbled upon in the course of my examination of gas water. The paper was intended for your pages, and would have fully explained the curious part of your report of my evidence; but as you made a second attack upon my evidence which could not be explained by a technical error of a newspaper reporter, I refrained from becoming a contributor to a journal in which I had been thus treated.—I am, &c.

W. Mattieu Williams.

Hall Road, Handsworth, near Birmingham.

[We print as a matter of course all that is pertinent in Mr. Williams' letter. He is quite wrong in construing our remarks into a personal attack. The observations we felt bound to make on the evidence given on the trials alluded to were made solely in the interest of true science, and they contained no insinuations against Mr. W.'s perfect good faith; nor does anything we have written convey a hint that he has wilfully sought to injure the Messrs. Chance, to whom we are perfect strangers. The reports, as we said at the time, were taken from local papers, and we could not be answerable for their verbal accuracy; but on one point they have been fully confirmed by the circuit short-hand writer's notes. We shall be glad to be the medium of conveying to our readers any account of the circumstances which determine the formation of *bluish* vapours when ammonia and hydrochloric acid come in contact with each other; and also pleased to publish a description of the unexpected reaction stumbled upon by Mr. W. in his examination of gas water.—Ed. C.N.]

MISCELLANEOUS.

Royal Institution.—Tuesday, June 10, at Four o'clock, the Rev. G. Butler, M.A., "On the Art of the Last Century;" Thursday, June 12, at Three o'clock, Dr. Lyon Playfair, C.B., "On the Progress of Chemical Arts (1851-1862);" Friday, June 13, at Eight o'clock, Major-General Sir H. Rawlinson, "On Cuneiform Writing, and the Way to Read it;" Saturday, June 14, at Three o'clock, Professor Anderson, M.D., F.R.S.E., "On Agricultural Chemistry."

Royal Institution of Great Britain.—General Monthly Meeting, Monday, June 2, 1862.—The Rev. J. Barlow, M.A., F.R.S., Treasurer, in the chair.—Mrs. Henry Bischofsheim, the Rev. W. R. Tilson Marsh, M.A., and Major Roger North, were elected Members of the Royal Institution. The presents received since the last meeting were laid on the table, and the thanks of the members returned for the same.

Potabilisation of Sea-water by the Electric Current.—In *Macmillan's Magazine* for this month is an interesting paper by Dr. Phipson, entitled, "Electricity at Work," in which the author passes in review the useful applications of this wonderful agency. He concludes his paper as follows:—"Reflecting upon the powerful decomposing chemical force with which we are furnished by the electric current, it occurred to me that I might be able to render sea-water potable by decomposing and extracting its salt, by means of a moderately powerful battery. The experiments were made at Ostend a few years ago. My apparatus consisted of three vessels containing sea-water; the centre one contained the water to be operated upon, the two others communicated with the two poles of the battery. The three vessels were connected by two bent Π-tubes filled with sea-water. As the only battery I could procure in Ostend was rather weak, I passed the current through the water for about fourteen hours, after which one of the outside vessels had become acid and the other alkaline. The sea-water was then filtered through charcoal, and was nearly drinkable. It would have been, I doubt not, quite potable had the battery employed been more powerful, as it was I found it difficult to extract the last particles of salt; and the water, after subsequent trials, still presented a slightly brackish taste. I have not had an opportunity of repeating this experiment since, but from the results obtained, I think it probable that sea-water may be rendered potable by means of the electric current."

THE CHEMICAL NEWS.

VOL. V. No. 132.—June 14, 1862.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On some Cyanides of the Platinum Metals, by M. C. A. MARTIUS.*

THE residuums from the treatment of platinum ore are submitted to the following operations to isolate the metals they contain:—

After pulverising and levigating to separate the larger grains of osmide of iridium, slightly calcine the powder in a closed crucible; then mix with it one part of finely granulated lead and one part half of litharge, and melt at red heat in a thick iron crucible. The lump of lead, freed from the adherent scoria, treat with nitric acid diluted with one and a-half times its volume of water. The residuum consists of iridium, rhodium, ruthenium, in the form of black powder, and osmide of iridium in spangles separable by levigation. Melt the osmide of iridium with some zinc in a charcoal crucible, heated until the zinc becomes volatilised; the osmide, remaining as a black powder, heat to redness in a current of oxygen to eliminate the osmium. Collect the osmic acid in a well-cooled receiver.

Mix the residuum of this calcination, together with that from the action on the lead, with their weight of chloride of sodium, and treat with chlorine according to M. Wöhler's method. Add hydrochloric acid to the solution and a quarter its volume of nitric acid, then distil until it is reduced to a third. The osmium disengages itself in the form of osmic acid, and is condensed in ammonia. The osmium is then easily extracted from the solution of osmiate of ammonia by evaporation and calcination with hydrochlorate of ammonia. The osmium remains under the form of a blue-black powder.

Precipitate the solution whilst warm by hydrochlorate of ammonia. After letting it stand for a few days, decant the supernatant liquid; it will be found to contain traces of iridium, rhodium, and gold. Wash the precipitate with a saturated solution of a salt of ammonia until it ceases to impart a red colour. The washing will contain all the rhodium.

The residuum, formed of double chloride of iridium and ammonia, mixed with double ammoniacal chlorides of platinum and ruthenium, melt with one and a-half times its weight of cyanide of potassium in a porcelain crucible. The chlorides become transformed into cyanides in 10 or 15 minutes. Pour the melted mass on to a porcelain plate, and dissolve it in the smallest possible quantity of water. To the warm solution add sufficient hydrochloric acid to decompose all the free cyanide of potassium, and then precipitate with sulphate of copper. Wash the violet precipitate, formed principally of platino-cyanide and iridio-cyanide of copper, in boiling water, and decompose with a boiling solution of caustic baryta.

The double barytic cyanides contained in the solution are easily separated by crystallisation. The platino-cyanide is much more soluble with heat than with cold, and crystallises before the iridio-cyanide, which is white and easily distinguished from the platino-cyanide. Small quantities of ruthenio-cyanide of potassium are found in the mother-waters of the iridium salt.

Cyanide of Osmium.—By heating concentrated hydrochloric acid with one of the osmio-cyanides to be hereafter described, a very stable dark violet precipitate of cyanide of osmium, OsCy, is thrown down, hydrocyanic acid at the same time being disengaged.

Hydro-osmio-cyanic Acid.—Saturated solution of osmio-cyanide of potassium mixed with its volume of fuming hydrochloric acid deposits white flakes of hydro-osmio-cyanic acid, $OsCy_2CyH$. These flakes, collected on a filter, washed in concentrated hydrochloric acid, and then dissolved in alcohol, separate into beautiful anhydrous prismatic crystals, when a layer of ether is poured on the alcohol. When they are dry the air has no effect on them; but when they are moist the air decomposes them, forming cyanide of osmium. They are soluble in water and alcohol. They have a strong acid reaction on test paper, and displace carbonic acid from carbonates.

Osmio-cyanide of Potassium, which has been already described by M. Claus (*Répertoire de Chimie pure*, vol. iii. p. 121), presents a remarkable analogy to ferro-cyanide, even in the particularities of the action of its crystals on polarised light. The most simple method of preparing it consists in treating osmiate of potash by cyanide of potassium, evaporating the solution and heating the residuum without melting it in a porcelain crucible. When the mass is white it should be taken up by boiling water, filtered, and crystallised.

Osmio-cyanide of potassium is almost entirely soluble in boiling water and insoluble in alcohol and ether. Its crystals belong to the right prismatic with square base.

The action of weak nitric acid on this salt occasions a brisk disengagement of gas. The solution seems to contain a nitro-osmio-cyanide.

As yet it has not been found practicable to obtain with osmium a salt corresponding to ferricyanide of potassium.

Further, the author describes osmio-cyanide of barium ($OsCy.2BaCy + 6HO$), double osmio-cyanide of barium and potassium ($2OsCy.2BaCy.2KCy + 6HO$), and a magnificent violet precipitate, which was produced by adding a salt of peroxide of iron to the osmio-cyanide of potassium, and which is probably a ferric osmio-cyanide ($3OsCy.2Fe_2Cy_3 + xHO$). He mentions the existence of various other osmio-cyanides and of an osmio-hydrocyanic ether.

Cyanides of ruthenium are exactly analogous to cyanides of osmium.

* *Annalen der Chemie und Pharmacie*, vol. cxvii. p. 357 (New Series, vol. xli).

Hydro-iridio-cyanic Acid ($Ir_2Cy_3{}_3HCy$).—This body is obtained in crystalline crusts when the iridio-cyanide of barium is precipitated with sulphuric acid, and the residuum is recovered by ether. It is soluble in water and alcohol, but sparingly in ether. The solution with the addition of hydrochloric acid gradually deposits a green precipitate of sesquicyanide of iridium. MM. Wöhler and Booth have obtained the salt of potassium.† It is very soluble in water, but insoluble in alcohol. Its crystals, which are very beautiful, belong to the class of right rhomboidal prisms. They are anhydrous. The barium salt ($Ir_2Cy_3{}_3BaCy + 18HO$) is very soluble in water and insoluble in alcohol.

The iridio-cyanides precipitate salts of copper, blue; those of mercury, zinc, protoxide of iron, white; those of peroxide of iron, yellow.

The composition of rhodio-cyanides is analogous to that of the iridio-cyanides. M. Claus has prepared the potassium salt by melting double chloride of rhodium and ammonia with cyanide of potassium.

Concentrated acetic acid decomposes this salt, disengaging hydrocyanic acid and depositing sesquicyanide of rhodium. Iridio-cyanide of potassium not decomposing under the same circumstances, this reaction can be made useful in separating iridium from rhodium.

Cyanide of Rhodium, Rh_2Cy_3, a beautiful carmine red powder, soluble in cyanide of potassium, and decomposable by heat, leaving a residuum of metallic rhodium.

The author adds a few observations on the platinum cyanides. He describes a double platino-cyanide of potassium and sodium ($KCy.NaCy.2PtCy + 6HO$), which crystallises in oblique rhomboidal prisms of a beautiful orange yellow, a platino-cyanide of cinchonine in white crystalline needles, a platino-cyanide of cadmium, $CdCy.PtCy$, obtained by precipitating the salt of Gmelin by chloride of cadmium. The latter cyanide is crystalline, and of a yellowish colour. It is soluble in ammonia, forming with this base a salt—

$$\left.\begin{matrix}Pt\\H_3\end{matrix}\right\} NCy.CdCy + HO,$$

which crystallises in beautiful white needles.

By adding to a hot saturated solution of platino-cyanide of potassium a hot saturated solution of acetate or nitrate of lead, a precipitate is not immediately formed; but when cold, small, anhydrous, yellowish-white crystals of platino-cyanide of lead are deposited.

Weak nitric acid transforms this salt into platino-sesquicyanide of lead ($Pt_2Cy_3.2PbCy + 5HO$) of a beautiful minium red, with blue shades on the surface.

† *Poggendorff's Annalen der Physik und Chemie*, vol. xxxi. p. 161.

Notes on Benzoylnaphthylamide, by A. H. Church.

When naphthylamine, in fine powder, is warmed gently with its equivalent of chloride of benzoyle, hydrochloric acid gas is evolved, and in accordance with analogy in the case of aniline, benzoylnaphthylamide formed. The crude product is washed with water, then with a solution of carbonate of soda, and finally re-crystallised from boiling absolute alcohol. On cooling, the solution solidifies into a mass of flattened needles. These crystals are odourless and colourless, and, unlike most naphthylamine compounds, are not darkened by exposure to the light and air. When heated they fuse into a colourless oil, which solidifies without crystallising. They gave on analysis numbers closely according with the formula :—

$$\left.\begin{matrix}C_7H_5O\\C_{10}H_7\\H\end{matrix}\right\} N. \quad \text{Benzoylnaphthylamide.}$$

By acting on benzoylnaphthylamide with ammoniacal nitrate of silver and excess of oxide of mercury respectively, the following compounds are formed :—

$$\left.\begin{matrix}C_7H_5O\\C_{10}H_7\\Ag\end{matrix}\right\} N. \quad \text{Benzoylnaphthylargentamide.}$$

$$\left.\begin{matrix}C_7H_5O\\C_{10}H_7\\Hg\end{matrix}\right\} N. \quad \text{Benzoylnaphthylmercuramide.}$$

When benzoylnaphthylamide is acted on by excess of chloride of benzoyle, the last equivalent of hydrogen is replaced thus :—

$$\left.\begin{matrix}C_7H_5O\\C_7H_5O\\C_{10}H_7\end{matrix}\right\} N. \quad \text{Dibenzoylnaphthylamide.}$$

Chloride of cumyle, chloride of succinyle, and chloride of sulphophenyle, act energetically upon naphthylamine. According to analogy, the several products, none of which I have as yet analysed, should be :—

$$\left.\begin{matrix}C_{10}H_{11}O\\C_{10}H_7\\H\end{matrix}\right\} N. \quad \text{Cumylnaphthylamide.}$$

$$\left.\begin{matrix}(C_4H_4O_2)''\\(C_{10}H_7)_2\\H_2\end{matrix}\right\} N_2. \quad \text{Succinyldinaphthyldiamide.}$$

$$\left.\begin{matrix}C_6H_5SO_2\\C_{10}H_7\\H\end{matrix}\right\} N. \quad \text{Sulphophenylnaphthylamide.}$$

TECHNICAL CHEMISTRY.

Contributions towards a Knowledge of the Inorganic Constituents of Plants, by Dr. Charles A. Cameron, *M.R.I.A., F.C.S.L., Corresponding Member of the Agricultural Societies of New York, Belgium, &c.*

(*Concluded from page* 316.)

Experiments with Fluorine.

Fluorine has been found in animal and vegetal bodies, by Berzelius, Erdmann, Marchand, Heintz, G. Wilson, T. J. Herapath, Nickles, and other chemists. In animal bodies it is found in greatest quantity in those parts—such, for instance, as teeth and feathers—in which silicic acid and phosphoric acid abound. It is rarely found in estimable proportions; thus, Wilson found mere traces of it in the ashes of twenty-four pints of blood, of twelve pints of milk, and of twelve pounds of cream cheese. Fluorine enters so sparingly into the composition of animals and plants, that it is difficult to believe in its importance to those organisms. Its presence in them is, in all probability, accidental, and arises from the circumstance of its being almost invariably associated in the mineral kingdom with silicon and phosphorus, which, on being taken up, as they are in large quantities by plants, carry with them a trace of fluorine. In like manner the iron taken up by plants carries with it into the vegetal mechanisms a little of its kindred metal and frequent companion—manganese.

The ashes of the plants which grew in the vessels of groups *a*, *b*, and *c* were carefully examined for fluorine with negative results. The quantities operated upon were, however, too small to enable me to state positively that they did not contain very minute traces of fluorine.

Conclusion.

The investigations, the results of which I have now had the honour of submitting to the Royal Dublin Society, have occupied a large portion of my time for several years past, but particularly during the years 1858 and 1861. To ensure the accuracy absolutely necessary in experiments of this kind, the greatest care had to be exercised in the preparation of the substances employed in which the plants were grown, and in the performance of the analyses of their ashes. The limits to which this paper is restricted by the rules of your Society, oblige me to be so concise in my descriptions that I cannot enter into the details of the methods which I adopted in analysing the ashes of the plants experimented upon; I can, therefore, give but a mere outline of the procedure. Great care was taken in separating the plants from adhering earthy matter, and the cleaning of the seeds was effected according to the plan recommended by H. Rose. As it was of importance that there should not be the smallest loss of the alkaline chlorides (which are so likely to be volatilised if exposed to a high temperature), Cailatt's mode of removing the organic constituents, by the action of dilute nitric acid, was, in most instances, adopted. By this method the incineration of the residual parts of the plants is effected at a comparatively low temperature, and the amount of ashes obtained is greater than when the combustion is effected by the old way,—*i. e.*, by burning the substance in a crucible placed in an oblique position. In a few instances the incineration was effected in a muffle, according to Strecker's modification of Erdmann's plan. The vegetal and animal substances were first merely charred at the lowest possible temperature, and exhausted of all their constituents, which are soluble in water and dilute hydrochloric acid. The incineration was then completed, and the ashes examined by the most recent and improved methods.

It is my intention to prosecute still further inquiries into the nature of the inorganic constituents of plants; and the problems which appear to me to be ripe for solution, and which I shall make an attempt to solve, are the following:—

I. Whether or not magnesia can be wholly substituted for lime.

II. Is iron absolutely necessary to plants,* or may manganese replace it?

III. May chlorine, which is so constantly present in plants, be replaced wholly or partly by bromine or by iodine, or by fluorine?

IV. Are bromine and iodine essential ingredients of marine plants?†

I am not vain enough to suppose that the results of my investigations, however elaborate and extensive they may be, will solve these important problems; their complete and satisfactory solution can, indeed, only be accomplished by the labours of many chemists, and by the careful performance of a large number of experiments. The more I study this subject the more I am disposed to believe that many of the mineral substances found in plants are either wholly useless, or are present in greater quantity than is necessary or useful. It also appears to me that too much importance is given to the difference in the composition of the ashes of various kinds of plants. Excepting those plants, such as the Gramineæ and the Equisetæ, which contain a large quantity of silica for mechanical purposes, there is, after all, but little difference between the composition of the ashes of all our cultivated plants. How often do we not find a greater difference in the composition of two specimens of the same plant than in the composition of two plants belonging to different species? That sea-weeds contain a large amount of sodium compounds is no proof that soda is the alkali they prefer. They are constantly moistened with sea-water, of which sodium is the most abundant constituent. Ought we not rather to consider potassium as the alkali essential to sea-weeds, since, when watered with a fluid containing thirty times more sodium than potassium, they absorb from it both elements in nearly equal proportions? Were potassium the preponderating ingredient of sea-water, I have no doubt the amount of sodium existing in sea-weeds would be exceedingly small. The opinion of the late Dean (Herbert) of Manchester, that plants do not grow naturally in the soils best suited to them, absurd though it may at first sight appear, is, after all, not very far from the truth. "I saw," said the sagacious Dean, "a crocus, a sternebergia, and an ornithogalum, growing in contact with each other aloft on the meagre soil of Mount Œnos; but not a seed-pod of the sternebergia could be discovered, and very few of the crocus. In a more fertile soil they would have been choked by some stronger plant, but they would rejoice in a better soil if protected against the oppressor." The Dean contends that the reason why certain plants are found in peculiar situations is not because they prefer them, but simply because they alone are capable of existing there, or because in more favourable places they would be overcome by more vigorous plants. I am almost disposed to adopt the Dean's theory in its entirety. Do we not see the furze flourishing in places where the wheat, the pea, or the cabbage would speedily perish; yet who can doubt that the furze thrives still better when cultivated in a kindly soil? The bare rocks to which the algæ cling are sterile to the cultivated plants; but what chance of existence would the soft mass of cells which form these vegetal mechanisms have, if surrounded by such firm, hardy plants as the thistle, the sweet-briar, or the cabbage? It is not the partiality of the sea-weeds for soda, which is the cause of their growing only on the coast; it is because their low organisation permits them to live upon the coarse fare supplied by the ocean, and to be developed under other conditions of existence, which would speedily prove fatal to plants higher in the scale of vegetal life.

In concluding this paper, I have to acknowledge my obligation to Mr. Emerson Reynolds for his assistance in carrying out some of the details of the investigations described therein.

[In the foregoing paper, the new unitary system of chemical notation has been employed—a system which, from its simplicity, deserves to be generally adopted.]

* Chlorosis is a disease not confined to animals; for, according to E. Gris, chlorotic plants (*Castanea Americana, Quercus phellos, &c.*) were cured by watering them with a solution of protosulphate of iron.

† It is probable that neither bromine nor iodine is essential to vegetables. That they are constantly present in marine plants, is simply because they are carried up into them by the alkaline metals; but if both of these palogens were completely removed from the ocean, I have no doubt their absence would in nowise affect the vegetation of the coast.

Bicarbonate of Ammonia.—Schrötter found a mass of crystals in a cast-iron pipe through which raw gas passed, which on analysis proved to have the composition $NH_4O,2CO_2+HO$. Before the analysis was made the crystals were cleaned from coal-tar with which they were soiled, and were resublimed. There is no doubt, then, of the existence of a true bicarbonate of ammonia.—*Sitzungsrb. d. Akad. d. Wissenschaf. zu Wien*, Bd. xliv., s. 33.

PHYSICAL SCIENCE.

On the Action of the Voltaic Pile on Salts of Potash and Soda, and Alloys submitted to Igneous Fusion, by M. Gerardin.

M. Gerardin has just completed, in the laboratory and at the expense of the Duc de Luynes, a long series of experiments on the electrolisation of salts and alloys submitted to igneous fusion. We have space only for his principal conclusions, and for some simple and easy experiments in support of them.

1. In the electrolytic decomposition of potash and soda salts submitted to igneous fusion the oxygen alone goes to the positive pole, the two radicals of the acid and of the base going to the negative pole.

By plunging the poles of the battery into a crucible containing fused borax, an abundant disengagement of oxygen takes place at the positive pole, and at the negative pole bubbles of sodium which burn on the surface. After the experiment the negative pole is found surrounded by a considerable deposit of amorphous boron.

2. The presence of excess of alkali does not vitiate the results, and imparts the advantages of conductibility and fluidity, which admit of the operation being performed with batteries of from one to four ordinary elements of Bunsen, and in ordinary furnaces. Platinum poles are much less attacked than in operating at a higher temperature without the addition of alkali.

All the salts of potash and soda can be decomposed in this manner with the greatest ease. M. Gerardin has operated upon borates, silicates, zincates, stannates, chromates, manganates, titanates, molybdates, uranates, aluminates, arseniates, arsenites, antimoniates, phosphates, sulphates, carbonates, and nitrates, and in every instance oxygen only was disengaged at the positive pole.

The decomposition of uranates and phosphates furnish the best experiments. With uranates globules of potassium or sodium appear at the negative pole, and detonate in the midst of a shower of uranium sparks. With phosphates, when the current has ceased to pass there is a brilliant combustion of phosphorus at the negative pole at the termination of the experiment.

Chlorates alone are the exception to the rule. Chlorine and oxygen are disengaged simultaneously at the positive pole. But this anomaly may be attributable to the decomposition of chloride of potassium, which takes place when the heat has begun to decompose the chlorate of potash.

3. Bodies which tend together at the negative pole are in a state of mixture rather than in a state of combination. The proof of this lies in the absence of phosphuretted or siliciuretted hydrogen in the decomposition of phosphates or silicates, to which is added potash, which is always hydrated. The combustion of phosphorus after the decomposition of phosphates is a further incontestable proof of this proposition.

4. The poles are often attacked. Thus, if in decomposing silicates an ingot of aluminium is taken as the negative pole, the aluminium alloys itself to the potassium and sodium set free. In presence of water this alloy yields spontaneously inflammable siliciuretted hydrogen.

5. During the decomposition of chlorides, bromides, iodides, sulphides, &c., the positive pole is energetically attacked, and the compound formed decomposes a short time after, under the influence of the current.

Thus, by placing a positive copper pole in a crucible containing fused sea-salt, it will be observed that, after the passage of the current, half the crucible will take a greenish-blue colour, from the chloride of copper, and the other half red, from the metallic copper.

With iron poles the action is more complex; for the sea-salt attacks the oxide of iron unless hindered by the action of the current. On this point the Duc de Luynes has performed an excellent experiment, hitherto undescribed. He threw some pieces of iron into a crucible containing fused sea-salt, and filled up the crucible with pieces of earthen crucible. Chloride of iron forms, which, under the influence of humid air, is transformed into oxide, the earthen pots at the same time becoming covered with a multitude of crystals of oligist iron, identical with those of the Island of Elba.

6. The electrolytic decomposition of compounds formed at the expense of the poles is not identical in the dry and the wet way. Thus, when copper poles are used, if the same current is passed into fused or dissolved sea-salt, there results in the first place chloride of copper and reduced copper, and in the second place, hydrate of suboxide of copper, of a beautiful yellow colour.

7. When several bodies are fused together, their electrolytic decomposition is not simultaneous, but successive. Thus, in the decomposition of uranates, sparks of uranium appear before the bubbles of potassium. This is attributable to the reducing action of bodies with strong on those with weak affinities. This is easily proved by dissolving oxide of copper in fused borax. When the borax is nearly solidified the bubbles of sodium detonate slowly, and the blue colour imparted to them by the dissolved oxide of copper will be superseded by the red of the reduced copper.

8. All alloys, without exception, lose their homogeneousness when traversed by the current. Thus, fused plumbers' solder when electrolysed becomes brittle at the positive pole and soft and malleable towards the negative pole.

The amalgams and alloys of potassium and sodium can be operated upon when cold. The amalgam of sodium decomposes water when taken at the negative pole, but not at the positive.

Potassium and sodium alloy, under the influence of the current, solidifies at both poles.

9. Whatever the electro-chemical rank of a metal, if present in small quantities in the alloy, it goes always to the negative pole.

The amalgams of gold and bismuth dissolved in mercury may be taken as examples. Whatever the amalgamated metal, it always returns to the negative pole.—*Comptes-Rendus.*

INTERNATIONAL EXHIBITION.

Class III.

Substances used as Food.

(*Continued from page* 319.)

A simple exhibition of substances used as food is by no means a satisfactory or, rather, satisfying display. The special organ which desires to be satisfied has no chance of gratification; and thus the mere sight of the richest bride-cakes under close glass shades, the most tempting of bon-bons in impervious glass cases, the neatest of wines in sealed bottles, and the brightest of ales in untapped barrels (which last, by the way, might as well be filled with water), is little better than a

mockery, if it be not altogether a delusion. We may say, indeed, that the only satisfactory display in this division is the case of Dr. Hassall (788), a sight of the contents of which is enough for a visitor. We are almost tempted to ask how the Doctor could have got possessed of all these abominations. Did he really find those Bath buns coloured with chromate of lead exposed for sale at a pastrycook's? Are all these poisonous delicacies in common use, and intended by their makers to be eaten? We might ask many other questions concerning the contents of this case, but this is not the place; we may, however, inquire whether, if people will buy lozenges and things of that sort at less than the cost of sugar, plaster of Paris, or flour, can reasonably be considered an adulteration?

There are a few novelties in this division which deserve particular notice. There is, for instance, the uncooked preserved meat exhibited by Messrs. Jones and Trovithick (795). Of all the means devised for protecting organic matter from that arch promoter of decomposition, oxygen, Mr. Jones's plan would seem to be one of the best. He places the material to be preserved in air-tight vessels, effects the complete exhaustion of the atmospheric air, and then replaces it with pure nitrogen. How far the plan is successful may be seen by the specimens exhibited. Here are a leg of mutton, a piece of salmon, and some sausages. Warmth and moisture have evidently done their best to bring about decomposition; but the active agent of change being absent, they seem to remain quite unaltered. There can be no doubt of the success of this plan if it be perfectly carried out; but it will be expensive, we imagine, and leave a large field open to Mr. M'Call (802), who exhibits preserved cooked meats close by. Mr. M'Call adopts the old plan of expelling air by boiling, but he adds an ingenious contrivance of his own. All who have been condemned to live on preserved meats are well aware that a little decomposition almost always takes place in them; and few who eat them escape without more or less diarrhœa. Probably the air is never completely replaced by steam in the boiling, or some air may make its way again into the cases before they are soldered down; so Mr. M'Call has a plan by which he expects to effect the absorption of any oxygen remaining in the case. In the top of his cans is a small capsule in which he places a button of fused hyposulphite of soda, which, by a peculiar contrivance, is exposed when the can is soldered, and becoming dissolved is expected, by the decomposition familiar to chemists, to absorb any oxygen left in the vessel. Whether it really does so we will not undertake to say, but, at all events, the case of beef open on the table is quite free from taint, and looks remarkably good.

Among the drinks there are also some novelties in the case of Mr. Garton, at present uncatalogued, but just over those we have mentioned. Mr. Garton has taken out a patent for converting common cane sugar into grape sugar, and using this for the production of beer, wines, and spirits. The conversion of the sugar is effected in a very ingenious way. Some ready-made glucose and a little acid are added to the dissolved cane sugar, and the mixture is then strongly agitated for a considerable time, after which it is found that the whole of the sugar has undergone a change from cane sugar to grape sugar. The great difficulty which Mr. Garton had to overcome was that of getting the sugar back again to a solid form, for our vexatious excise regulations forbid the entrance of anything like molasses to a brewery or distillery. The inventor has, however, succeeded in getting his glucose into an agreeable looking granular condition, very much like honey, and there is now no legal objection to its general introduction. Mr. Garton here exhibits specimens in the various stages of manufacture, and also samples of beverages made with his grape sugar in place of malt. Every practical chemist will see how much these may be made to surpass those produced in the ordinary way, and how large a field is open to the employment of this material. The strongest ales may be brewed having very little more colour than the decoction of the hops; the British brandy maker may now equal his Continental rival at Cognac; and the maker of perfumes will find a spirit quite free from any other odour than its own. British wines, too, will lose some of their objectionable qualities if fermented with this glucose.

We may pass over most of the remaining exhibitors in this class with a very few words. No doubt the ales of Messrs. Salt and Co. (874) and Bass and Co. (853) fully sustain the reputation of these brewers; but the visitor must exercise his faith, for the barrels are untapped and the bottles are sealed.

Messrs. Fortnum and Mason have a very fine display of preserved fruits and other good things for which they are famous. In case 783 Messrs. Fry exhibit all sorts of cocoa and chocolate preparations, from the stump of a cocoa tree to the finished chocolate bon-bons; and in 776 Messrs. Dunn and Hewett display a similar collection to which they scientifically add a specimen of theobromine, the only one we have yet seen in the Exhibition

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Six Lectures on Some of the Chemical Arts, with Reference to their Progress between the Two Great Exhibitions of 1851 *and* 1862, *by Dr.* Lyon Playfair, *C.B., F.R.S., Professor of Chemistry in the University of Edinburgh.*

Lecture II. (*Thursday, May* 15, 1862.)

Distillation of Coal.—Showing how the former Waste Products in the Manufacture of Gas have been Economised. Salts of Ammonia, Benzol, Tar Colours, &c.

I must now make a little recapitulation of our last lecture, and show you the manner in which its waste products are applied to useful purposes. I explained to you that gas was produced by the distillation of coal; that for a long time the thoughts of manufacturers were applied only to the first purposes for which coal was used, namely, the production of the gas; and that all the substances which are accessory products were looked upon in the light of concomitant evils, the tar and water being waste products, which were inconvenient, and to be got rid of by the most ready methods. Long ago, in the seventeenth century, Boyle wrote an Essay entitled, "Man's Great Ignorance of the Uses of Natural Things; or, that there is no one thing in nature whereof the use to human life is thoroughly understood." This truth of the seventeenth century is still a truism in the nineteenth century, the whole progress of manufacture being merely an illustration of it. Substances which to-day are the most useless, to-morrow become embraced within the circle of industrial utilities. It is quite true that there is no one substance in nature of which we know all its properties, or all the uses to which it can be applied for the purposes of common life. I take Boyle's old title as the text for our discourse; but I have only time to give it a very limited application, by describing the utilities

now derived from tar, although time will not allow me to embrace them all in one lecture.

You will recollect what were the waste products of the coal-gas manufacture. You will find the products of the distillation of coal in the first diagram on the gallery. First, gaseous products were produced, part of which were useful—the diluents and illuminants; part were impurities, and were got rid of by certain processes. Even these impurities are in some cases now applied. After that there was the crude coal oil, which is commonly called tar; and then there was a watery portion which contained salts of ammonia. We, therefore, had the gaseous products, the crude oil or tar, and the watery distillate. All except the gaseous products were regarded as impurities—waste substances which were got rid of, and the getting rid of which was a serious undertaking to the gas manufacturer; and I wish now to show you how all these have been utilised.

We begin with the gas water, the badly-smelling, black, ugly gas water of the gas-works, and see what has been obtained from it. The gas water contains salts of ammonia. These salts of ammonia, except in one instance, consist of the base ammonia united with volatile acids, sulphuretted hydrogen, and carbonic acid in the other. A certain quantity of chloride of ammonium, or ammonia in union with hydrochloric acid, is also found in the gas water. The value of these salts of ammonia was long known before they were extracted from the watery waste product of the gas manufacture. In fact, ammonia derives its name from one of the titles given to Jupiter, "Jupiter Ammon," near whose temple in Upper Egypt ammonia was for many generations manufactured from the refuse of camels, which was taken and heated and distilled, and gave off ammonia or some of its salts. Hence its name. Its uses were familiar in this country, and its applications to manufactures were known long before persons thought of extracting it from gas water. After a time chemists found sulphide of ammonium and carbonate of ammonia in the watery portion of the coal gas distillate. This distillate gives a very abundant source of ammoniacal salts, and, in fact, the source from which it is now almost all derived. As, however, the subject of to-day's lecture, when we come to the colours produced from coal-tar, wholly relates to the characters of ammonia and the base which is in these salts, I must be permitted, with the excuse of the many chemists whom I see present, to tell those who are not necessarily chemists what ammonia is, and what are its peculiar characters.

The general character of ammonia is probably known to you all. Here is a vessel containing it. It is, as you will see, a colourless gas. It has a very pungent smell; it has an alkaline character; and it is extremely soluble in water. Mr. McIvor will now agitate a portion of this with water, and then you will see how soluble it is. Its alkaline character I wish to explain to you for a moment. An alkaline character is the character possessed by certain bases, such as soda and potash, and a trivial feature of it, but a very important one, is that it renders reddened infusions blue. I have got here the alkali soda, and if I add it to a reddened vegetable infusion, you see the reddened vegetable infusion becomes blue. This is an ordinary character, and apparently a trivial matter, but still an important one. Now we will agitate this ammonia with water, in which it is partly soluble, and we will admit the water into it, and you will see how it rises. You will observe, at the same time, that this red matter as it rises becomes blue. It is so exceedingly soluble in water, that the water dissolves the ammoniacal gas; and its alkaline character is shown to you very distinctly by the red colour of this red water becoming strongly blue just as this fixed alkali soda or potash rendered it blue. Observe how extremely soluble this alkaline ammonia is. You see the water absorbs it so completely that we are obliged to add more water in order to fill the tube.

It is the character of an alkali to unite with an acid. An acid with which it forms, one of the most economical salts of which I have to speak, is muriatic acid. Here I have some muriatic acid—colourless, like the ammonia, but yet possessing very different properties. You see in this case we have got this water blue instead of red, and we will now remove our vessel and agitate it in the same way. We introduce a little of the water and shake it up, so as to dissolve some of the muriatic acid, which is of a different character altogether from ammonia. And now we pass it back into the basin of water coloured blue. The other was red and became blue; but now the blue becomes red, from this gas being an acid—having an acid instead of an alkaline character.

Now, I wish to show the effect when these two gases are mixed. We must allow a little time for the completion of the experiment. I have here some ammonia, and I will place a flame below it; and in the other retort I have an acid, and I will place a light below that also. This is muriatic acid. When both of these are heated we will bring the vapours into contact. You will see then that the muriatic acid will unite with the ammonia, and produce a substance which is exactly the same, although not in such a solid form, as this muriate of ammonia [referring to a large white block of that substance on the lecture table]. It is hydrochloric acid and ammonia which form this solid cake in the manner in which it occurs in commerce.

How completely, you will see, this shows the deductive character of chemistry. Chemistry, in its present state, is not an inductive science; it is a deductive science. It is a science taught to us by experiment.

These liquids are now nearly boiling, and we will pass the two gases into this large tube. [The vapours of the ammonia and the hydrochloric acid were passed through separate tubes up into a large glass globe, and there allowed to mix]. They are now joining one another, and they are forming this solid white muriate of ammonia by their union. You see how this solid body is formed from two gases, a result which could not have been predicated by any science, and is only taught to us by experience.

Having explained the preliminary points to you, I now desire to show how salts of ammonia are manufactured in the arts. When a ton of coal is distilled, above ten gallons of the watery portion comes over from it—ten gallons from Newcastle coal. This contains sulphide of ammonium and carbonate of ammonia. Now, sulphuretted hydrogen and carbonic acid are both volatile substances. It is, therefore, only necessary to add a strong acid to obtain whatever salt we please from these compounds of ammonia. Muriate of ammonia is manufactured in this way:—The gas liquor is run into a deep cistern. This cistern is connected with a chimney, and there is poured into it muriatic acid. That muriatic or hydrochloric acid expels the sulphuretted hydrogen and the carbonic acid, and forms muriate of ammonia in solution. The bad-smelling gas, sulphuretted hydrogen, which smells like rotten eggs, is passed up the chimney, and removed from the locality of the works, to be given to people living at a distance. The muriate of ammonia is placed in a pan containing about 1500 gallons, and evaporated till strong enough to crystallise. The muriate of ammonia obtained in this way is impure, and has to be sublimed in order to be obtained in this state. This is a piece taken from the top of the retort. After that it is removed to a still of this kind—an iron pot surrounded by a leaden dome; and here a fire is placed below it, and the muriate of ammonia vaporises from its impurities, and condenses at the top as a crystalline solid. About 4000 tons of this muriate of ammonia are made annually in this country from gas water. It is used extensively in making alum,

and it is used largely in the process of soldering. For instance, it is employed for preparing tin plates when you are obliged to get the surface of the iron which you are about to tin in a perfectly clean state. You put it in a bath of muriate of ammonia, which dissolves off the oxides which are on the surface, and leaves the iron in a state for soldering. It is also employed extensively in making the more common salts of ammonia.

There is a point in connection with this to which I would direct your attention. I want to show you the peculiar character of ammonium as a metal. Ammonia consists of one equivalent of nitrogen and three equivalents of hydrogen. There seems to be little analogy between this substance and chloride of sodium or chloride of potassium. Chloride of sodium, which is common salt, contains the silvery metal sodium; and chloride of potassium contains also the silvery metal potassium. There seems to be little analogy between a gaseous body consisting of one of hydrogen and three of nitrogen; but I am going to attempt to imprison this body, which consists of four equivalents of hydrogen and one of nitrogen, by amalgamating it with mercury. Here I have a saturated solution of this salt, chloride of ammonium, which chemists are compelled to think contains a substance having metallic characters, although it consists of these gaseous bodies, nitrogen and hydrogen. Here I have an amalgam, or a compound of mercury with sodium. Now, if I pour this amalgam of sodium and mercury into the chloride of ammonium, the sodium takes away the chlorine from that compound, and leaves the ammonium to combine with the mercury. This metal is NH_4. It is one of an evanescent character, and I must imprison it by holding it in the mercury in order to show you its presence. As the ammonium acts upon the mercury it will swell up. It is now swelling. You see it growing in bulk before your eyes. We have the ammonium imprisoned by the mercury, and enabling me to show you for awhile that this substance really has metallic properties, although it will soon dissipate again into the gases of which it consists. You see that it has formed an amalgam, as the sodium did, but, from its gaseous character, one of much larger bulk. It is a semi-solid or butyraceous substance. It can be handled, but it soon breaks up into running mercury and the gases. It is now obvious how the salts of ammonium may be readily made analogous to the salts of sodium and potassium. This body, NH_4, or one of nitrogen and four of hydrogen, is in reality a metal which unites with halogens and forms salts.

I must run quickly over the other salts of ammonia, and I will not enter into the details of the manufacture. For instance, this muriate of ammonia is not manufactured only in the way I have told you. It is manufactured in many other ways which it would tire you to describe. One of them is to take the gas water, and, instead of saturating it with strong acids like muriatic acid, to distil it with lime. The ammonia gas goes over, and is very readily condensed in water. It may be condensed in water or acids, and forms various salts. This process is much the best, as the badly smelling sulphuretted hydrogen is retained by the lime. There is another way of manufacturing this muriate of ammonia by acting upon sulphate of ammonia with common salt; but I will not tire you with all these details and modifications of the manufacture. You must ascribe it not to ignorance, but to the fact that I do not think it necessary to enter into them. I now pass to sulphate of ammonium, which is another salt very much manufactured from gas water. About 5000 tons of it are annually made in this country from gas water. It is made in the same way, by adding oil of vitriol to the ammonia of the gas liquid. It is used largely for manure. It is used largely for making alum; and it is employed also for making ammonia, or rather solutions of ammonia in water, by distilling it with lime, which keeps back the sulphuric acid. Carbonate of ammonia is another salt, and one which ladies use very much in their scent-bottles, and as a diffusive stimulant. It is made by distilling with sulphate of ammonia and chalk. Chalk is carbonate of lime. The carbonic acid goes over to the ammonia and forms carbonate of ammonia. The way this is done in the arts is represented here. I have here a still, or a retort, not at all unlike the retorts which are used in gas making. Here the sulphate of ammonia, or the muriate of ammonia and carbonate of lime are placed, and they are heated together with fires placed under them, and the carbonate of ammonia being a volatile salt is sublimed and condenses in these chambers. It is afterwards distilled again. It sublimes at 177°, which is below the boiling temperature of water. The stills have got leaden caps, and the water heats the impure salt and sublimes the carbonate of ammonia which is afterwards taken out of the cap. This is also very largely manufactured. About 2000 tons are made annually of this salt. Various modifications of these plans are also used. For instance, the gaseous ammonia is led into a chamber of carbonic acid. The chamber has water at the bottom. The carbonate of ammonia is formed and crystallised, and afterwards sublimed. The aqua-ammonia of pharmacy, or ammonia water, or liquid ammonia, or hartshorn, is made by introducing a base to keep back the acids, and the ammonia is distilled over. This ammonia is used for a great many purposes—as a diffusive stimulant in medicine. It is also used as an antacid in medicine; and largely employed to saturate carbonate of ammonia in ladies' scent-bottles, some aromatic substance being generally mixed with it. Now, look what a transformation is effected by the application of chemical agency: the refuse of camels, the offal of the streets, the fetid water of the gas-works, have become so transformed under the influence of chemistry that ladies preserve them in their scent-bottles as a cherished luxury. You see how these waste products may be used to furnish even luxurious utilities.

But I must go faster with my subject. I now pass to coal-tar.

Now, coal-tar is a very complex body. It contains a large number of substances, some of which are volatile, others more difficultly volatile, and others not at all volatile in the ordinary sense of the word.

I have here a retort filled with tar, and I am now going to pass through that a current of steam; and you will see that after a little when it passes through freely it will distil over along with the water, and that this water will contain, swimming on its top, a certain quantity of naphtha. The steam which passes through the tar will take away the more volatile portions of the tar and condense it upon the top under the name of naphtha. What remains behind is a mixture of what is called dead oil and pitch. This dead oil is afterwards distilled off, and what remains behind in the retort is finally pitch.

In distilling it in this way we obtain from 100 parts of coal-tar, of naphtha 9 parts, of dead oil 60 parts, and of pitch 31 parts, so that there are various substances obtained. I have only time, however, to deal with the naphtha. Now, naphtha itself, or the substance which we get over by distilling the tar with steam, is a general word also. I have placed on this diagram the products of the tar. Crude naphtha contains all these substances which are written down there; but I will refer you at present to the upper division. The crude naphtha contains, first, basic oils, or oils acting as bases; secondly, acid oils, or oils acting as acids; and thirdly, neutral hydrocarbons. You will notice in the diagram, as in all the diagrams which we shall use, that whenever we have a body acting as a base we colour it blue to show that it is a base, like this soda which coloured red water blue; when it is an acid we colour it red; and when it is neutral we colour it green; so that when you find a body written red it is an acid body; and when it is green it is a neutral hydrocarbon.

This naphtha is now taken and purified and clarified. There is added to it sulphuric acid. The sulphuric acid takes up the basic oils which are at the top, and unites with them and forms salts—sulphates of these bases. (We will complete our distillation of the tar afterwards. It is making too much noise for me to have my lecture accompanied by it. We will finish it after the lecture, and you shall see the products in the next lecture.) The sulphuric acid unites with the basic oils and produces this "sludge," as it is termed by manufacturers—the bases united with the acid.

Now these are extremely valuable, and it is from them that these coal-tar colours, which I am going to speak of presently, are obtained; but they are entirely lost by the manufacturer. They will probably be saved afterwards, but at present they are thrown away as a sort of tar. The first things that we obtain of any advantage are the acid oil and the naphtha. The naphtha itself—the crude naphtha of which there is a specimen there, is employed at once, without any purification, for the purpose of making india-rubber waterproof coats and similar articles. But it is purified for various very important purposes. When the most volatile portions are collected, what comes over are the acid oils. Now these acid oils consist of two acids—carbolic acid and cressylic acid. Carbolic acid has the formula $C_{12}H_6O_2$; and the cressylic acid is what is called a homologue of the other, or contains C_2H_2 more. It consists of $C_{14}H_8O_2$. Common creosote is a mixture of these two acids. This carbolic acid which forms common creosote, is after purification, and when perfectly dry, a solid; and it is this beautiful acid which I have present here. I see the manufacturer of this very specimen in the room, and I wish he was lecturing here to tell you more about it than I can. Before he sent me this beautiful specimen I had never seen it in commerce solid; it is generally liquid. This carbolic acid when united with lime forms one of the most powerful disinfectants we have, which I will show you in my last lecture, when we come to the subject of sanitary chemistry. When this acid is treated with nitric acid it loses part of its hydrogen, and that hydrogen becomes replaced by peroxide of nitrogen, a lower oxide of nitrogen than nitric acid. When it is treated with nitrogen three of these go away, and the hydrogen is replaced by what is termed a compound radicle—a body which plays the part of hydrogen, and which forms this yellow substance called carbazotic acid. Carbazotic acid is carbolic acid, three of whose equivalents of hydrogen have been substituted by three equivalents of an oxide of nitrogen.

Now, this carbazotic acid can be prepared in large quantity from creosote by the action of nitric acid, and it can be employed at once for dyeing. If I take a skein of silk and agitate it for a little in this carbazotic acid, it will take on the dye without any previous preparation, and it is dyed a beautiful yellow colour. You see how it has already taken on the colour, and in this way you can dye silks of a beautiful colour with this substance obtained from the former waste product of coal-tar. This material has also been lately employed, as almost everything is employed, for various other useful purposes. It is an excellent antiperiodic, like quinine, only when employed it dyes the skin of the patients yellow, and they, therefore, have a sort of artificial jaundice. But it has also been suggested for another purpose. It may be mixed with arsenic and other poisons for the purpose of rendering them more ready of detection. It imparts to the arsenic a bitter taste, and it also turns the person to whom it is administered yellow, and in a case of slow poisoning this yellow appearance would be an indication that there was something wrong.

Cressylic acid, another of the compounds of crude coal oil, is not much employed in the separate state.

I now pass to the neutral hydrocarbons. The neutral hydrocarbons are also various. They are called benzol, toluol, xylole, cumol, cymol, and a great many other names with which I will not trouble you. They are compounds of hydrogen and carbon, and possess many degrees of volatility. For instance, benzol boils at 177°. This is one of the most useful of the substances. It is made from crude naphtha by a simple operation, taking advantage of its low temperature of ebullition. Here is a benzol still. The crude naphtha is placed in this still. It is a double still, into which steam is sent from this steam-boiler in order to heat the crude naphtha. The top of the still, you will observe, passes through a cistern of water. That cistern of water is kept at the boiling point of benzol, 177 degrees, and the vapour of the naphtha passes through the heated vessel, which is heated to 177 degrees. Benzol distils over at 177°; but toluol, cumol, cymol, and the others boil at a much higher temperature. Therefore they are condensed at that temperature, and fall back into the still. The separation is, therefore, effected simply by means of keeping the benzol at its own boiling temperature, and cooling the others below theirs. It is a very volatile substance. It, no doubt, adds much to the illumination of our coal gas. We will show you this. I have here the means of showing you the gas, first, not passed through benzol, and then passed through benzol. I first take the gas not passed through benzol, and if I light it you see that there is little illumination. You can scarcely see it at all at a distance. Now, I will pass some gas through this benzol. It is now passing through, and you see how the gas has licked up this volatile body, and given us a stronger illumination. Benzol is, no doubt, one of the illuminating vapours which exist in common coal gas.

Now, this body, when acted upon by nitric acid, produces what you will find in that big bottle—nitro-benzol. I must call your attention to nitro-benzol a little scientifically. Benzol has the formula $C_{12}H_6$,—that is, it contains twelve equivalents of carbon and six of hydrogen. In nitro-benzol one of these equivalents of hydrogen goes out, and one equivalent of oxide of nitrogen, NO_4, goes in and substitutes it, and then forms nitro-benzol, a substance which by itself possesses some peculiar characters. It smells strongly of bitter almonds, and it is employed now instead of bitter almonds, which is poisonous, for making common almond soap. That common almond soap which we buy is now perfumed with this nitro-benzol. It is also employed in confectionery as a substitute for bitter almonds. It is much better for that purpose, because the bitter almonds contain prussic acid, and by the use of too large a quantity by our cooks we may poison our friends. There is no chance of that taking place when nitro-benzol is used. It is the basis from which we derive our tar colours, and the mode in which it is used for this purpose will require a little close attention to a chemical formula; but it is very interesting.

If nitro-benzol is acted upon by water and by iron, of which I have put down the symbols here—nitro-benzol + water + iron = $C_{12}H_5(NO_4) + 2\,HO + 4\,Fe$—the iron takes away all the oxygen from the water, and the oxygen from the oxide of nitrogen. There are six equivalents of oxygen, which the iron takes to itself and forms iron rust with it. This rust remains, and the two of hydrogen of the water now joins itself to the $C_{12}H_5N$, and produces this body here, $C_{12}H_7N$, aniline. That is to say, the iron takes away the oxygen and leaves oxide of iron and aniline as the result.

Now, this aniline is a most important body. It was first investigated by Dr. Hofmann, who has made with regard to it a series of the most brilliant researches, out of which have arisen these coal-tar colours with which we are now acquainted. Aniline is an ammonia. It is a body exactly resembling the base ammonia, but it is what is termed a compound ammonia. Here is the constitution of ammonia:—

$$N\begin{Bmatrix}H\\H\\H\end{Bmatrix}$$

I put down the three atoms of hydrogen separately. Now, if I take away one of these atoms of hydrogen, and substitute it by one of something else which plays the part of hydrogen, I form a compound ammonia. I will do so in this case.

$$N\begin{Bmatrix}C_{12}H_5\\H\\H\end{Bmatrix}$$

I have replaced one atom of hydrogen with a compound radicle which chemists call phenyle, and I obtain what is termed aniline. This aniline is therefore a compound ammonia in which the radicle phenyle replaces one of hydrogen.

When Hofmann began his researches upon this subject, aniline was made by a laborious process by distilling indigo with potash; and the possession of a pound of aniline in any chemical laboratory would have been looked upon as a wonder. Now you see that out of coal-tar we can present to you upon the lecture table whole gallons of aniline, and it is now sold for a few shillings a pound. I have here a series of the substances formed in the production of Magenta. I am indebted for them to the discoverer of rosaniline. Here is a block of coal weighing 100 lbs. This block of coal produces this amount of tar when distilled. Here is the amount of aniline which, with the most economical manufacture, can be extracted from that block of coal; but still, although it appears to you only a small quantity, it is, in fact, a most economical quantity compared with the processes which were formerly employed. It is out of this aniline that the peculiar dyes are obtained, and this is the quantity of the Magenta dye which can be obtained from that large quantity of coal. If you will examine these products after the lecture, you will find this a very instructive proportional series.

Now, it is out of this aniline that we produce mauve, Magenta, roseine, azuline, bleu de Paris, and the various colours which have received arbitrary names. It was known for a long time that the products of distillation of coal had a strong tinctorial power. Here, for instance, I have one of them—a body called pyrrole. I have here a piece of pine-wood, which I see Mr. McIvor has made, for a theatrical purpose, in the shape of a dagger. I will now moisten this with muriatic acid, and then place in a deep vessel which contains a few drops of pyrrole. You see that it suddenly gets as it were covered with blood. This muriatic acid is mixing with it, and the dagger comes out in a sanguineous state. You observe the strong tinctorial power which this substance has by the deep colour which it produces. Now this tinctorial power has been known, in fact, for a long time, but the mode of manufacturing the substance readily and economically was not known. Here I have a small quantity of aniline, and I agitate it with water; and now, if I add to that a solution of bleaching powder, you will see the effect it produces. It was long known that this aniline gave a purple colour with bleaching powder. The colour comes after a little while; it does not come immediately, but you see, as I add it, that the aniline produces a mauve or a purple colour; and this was known for many years, before persons knew now to make it for commercial purposes. This is now a colour used in the arts. The first person who introduced this, and to whom the greatest credit is due for its production, was Mr. Perkin, a pupil of Dr. Hofmann. Mr. Perkin had seen and admired the tinctorial power of aniline, and he had an ambition to render this fugitive colour permanent, and to introduce it into the arts as a dye, and he succeeded admirably. The mode is this: this aniline is a base, and unites with sulphuric acid as ammonia does, and it forms sulphate of aniline. He takes equivalent quantities of aniline and bichromate of potash, and mixes them together, and in a little while, after standing together, they form this very unpromising-looking black powder. You see this black powder here; it looks extremely unlike a dye. Now, when this colour is washed with coal naphtha, this nasty-looking brown resinous substance is dissolved out of it by the coal naphtha, and then there remains a still unpromising substance, but which is rather purple in colour. When you treat with alcohol this brown powder, which has been washed with naphtha and had the resin taken out of it, it forms with the spirit a strong solution of mauve. This beautiful purple colour is obtained in this way,—by dissolving out of the brown powder the purple colour by means of alcohol; and it is this purple colour which is used largely in dyeing. It is readily soluble in alcohol. If I take a washing-bottle of alcohol, and throw a little of it upon the substance, you see that the beautiful purple colour of this substance is readily manifested. [The substance had been previously spread on a white paper screen, and there remained unapparent; but upon the projection of the stream of alcohol upon the screen the purple colour was produced where the alcohol came in contact.] It is a substance which you can easily detect and find out whether you are dealing with this colour, or dealing with some colours derived from lichens, which have a similar character. I have here aniline purple, and I now add to it a little sulphuric acid, and I will show you that it is very easy to detect which colour you are dealing with. The sulphuric acid turns it first to a dirty green. If now I add to this a little water, this dirty green becomes a beautiful blue. The light has become bad. The day is not a good one for showing you these colours. I am afraid this day-light, or want of day-light, will render it necessary for you to take what I say on trust. The addition of water makes it a beautiful blue. This is a second test for it; but if now I add a little more water to it, it is restored again to its purple state. You see on pouring it into this large jar that it resumes its beautiful purple, and in this way you can easily detect its presence. The sulphuric acid turns it a green, a little water turns it a deep blue, and a large quantity of water brings it back to its original purple condition.

It is easy to dye with this aniline purple; in fact, ladies can dye with it perfectly themselves. It is only necessary to use for this purpose hot water—water so hot that you cannot bear it with your hand, but not boiling. The best temperature is about 150°. If you take this hot water and add to it a little tartaric acid and a little of this aniline purple, and then place the silk or woollen in it, it becomes dyed. It is easy to attach the colour to animal fibre, but not to cotton. In the next lecture I have to explain to you its application with regard to cotton. I have here the colouring matter, and now I will add to it a solution of tartaric acid, which is necessary to produce the colour. After that all I require is to place my silk in this solution, and to rinse it for a little time in it, and you see that it quickly takes up the colour and produces that beautiful mauve which is now so familiarly known. It is, therefore, a substance which is extremely easily applied,—almost as easily as the carbazotic acid.

Although I am within a minute of the hour, I must ask your attention for five minutes more. The next dye to which I have to direct your attention is Magenta; or, as it is more properly called by Dr. Hofmann when it is in the state of purity, rosaniline. I should like also to prepare this before you, and show you the method. Any weak oxidising agent less strong than bichromate of potash, produces this substance. I will take here bichloride of tin for my purpose. I add to this anhydrous bichloride of tin an excess of aniline. It must be done cautiously, for the action is energetic. As soon as the action subsides we will add a little more, and finally heat it until I drive off all

the aniline. I must have an excess of aniline and then drive it off, after which the Magenta will be seen to appear. This will take a little time to perform. Mr. McIvor will now heat this gently, passing it slowly over the flame at first as the action is violent, and will continue the heat until our Magenta begins to appear. After a little time we shall have a very good imitation of Magenta, but not nearly so good as is produced by the manufacturers, who now produce it on the most magnificent scale; I allude to Messrs. Simpson, Maule, and Nicholson, to whom I am indebted for a number of the illustrations that are exhibited here, and for this crown, which is made of the substance in the ordinary condition in which it is sold—acetate of rosaniline. You see what a magnificent crown it is. The formula of rosaniline I have written here.

$$N_3C_{40}H_9$$

It is what is called a triamine, or it is three atoms of ammonia which have coalesced into one; and this rosaniline forms, with acetic acid and other acids, deeply-coloured salts, of which this ordinary Magenta is one. You will see a crown in the Exhibition which I think is one of the most remarkable of the scientific exhibits which we possess. The crown is composed of the acetate of rosaniline. You see the difference between this rosaniline and mauve. It is of a much redder colour than mauve, and is more definite in its character. Mauve is a neutral body, not possessing either basic or acid properties; while, on the other hand, rosaniline is a true ammonia—a true base.

Now we have formed our Magenta by this experiment. I think I can show you the colour better if I pour it into water and add an acid. You will then also see its tinctorial power. After a little time it will dissolve and form a solution of this substance. It is a powerful tinctorial body. There [referring to a bale on the lecture table] is the quantity of woollen which is dyed from the amount of Magenta produced from 100lbs. of coal. There is the 100lbs. of coal which produces this small quantity of Magenta, and that bulk of wool is dyed from it, so that you see its tinctorial power is great.

As I have only given you an introduction to the subject, and you will afterwards have the application of this to calico printing, I will only say one word as to the blue colour which is obtained, and in the next lecture, and the lecture afterwards, we shall have an abundant opportunity of following up this subject. Not only have reds and purples been obtained in this way, but a yellow has recently been procured by Mr. Nicholson, to whom we are so much indebted; so that yellow, red, and blue, the three primitive colours, are now to be obtained from the coal-tar. Here is one called *bleu de Paris*, or *bleu de Lyons*, or azuline. It is obtained sometimes from aniline by the action of oxidising agents, such as bichloride of tin, at a high temperature under pressure—a temperature of 350°. But most of these blue colours are made from carbolic acid, and not from aniline. I think I can show you here the blue colour. I have put some of this blue upon this paper. There is one which is obtained from carbolic acid or from creosote. The process, however, is not known; it is still kept a secret in the arts, but you see what a beautiful colour it is; and what a power we have in possessing the three primitive colours, by the mixture of which we can obtain so many others.

As a new art, the manufacture of these colours is of great importance. Hitherto, England has been dependent upon foreign countries for its dyes. We have imported madder from Holland, from Turkey, and from France, and blue colours from India, in order to produce our calico prints; but you see now that we are likely to reverse this. We find in this waste product, coal-tar, the three primitive colours out of the mixture of which we can produce almost any shade we desire; and without taking upon myself the character of a prophet, I think I may easily predict that, in a few years, England will be a colour-exporting instead of an importing country. In this country, even now, coal-tar, notwithstanding all these applications of it, is worth only from a penny to three halfpence a gallon. These discoveries will probably alter the whole character of calico printing, and make this country an export market of colours.

As this is a highly important industry, I have solicited your attention to two other lectures on it.

CHEMICAL SOCIETY.

Thursday, June 5, 1862.

Dr. A. W. Hofmann, *F.R.S., President, in the Chair.*

Professor Wurtz delivered a lecture "*On Oxide of Ethylene considered as a Link between Mineral and Organic Chemistry.*" It had long been known that a mixture of olefiant gas and chlorine gave rise to the production of an oil commonly called Dutch liquid. Various views had been entertained as to the constitution of this body; on account of the decomposition which it undergoes when acted on by caustic potash it had been considered a compound of hydrochloric acid and a body of the composition, C_2H_3Cl; * the Dutch liquid itself having the empirical formula $C_2H_4Cl_2$. From his own experiments, however, he was led to conclude that it should be viewed as a bichloride presenting some analogy to inorganic chlorides. On heating this body with acetate of silver a decomposition took place, the silver combined with the chlorine, and an organic body was formed containing oxide of ethylene and acetic acid; the bromide of ethylene was actually employed instead of the chloride, but the action was, no doubt, similar; the decomposition might be expressed as follows:—

$$\left.\begin{matrix} C_2H_4Br_2 + C_2H_3O \\ Ag_2 \\ C_2H_3O \end{matrix}\right\} O_2 = 2\,AgBr + \left.\begin{matrix} (C_2H_4)'' \\ C_2H_3O \\ C_2H_3O \end{matrix}\right\} O_2$$

On exposing this substance to the action of caustic potash the acetic acid was removed, and glycol, the hydrated oxide of ethylene, set free. To obtain the oxide of ethylene itself it was necessary to expose this body to the action of hydrochloric acid, when a substance, intermediate between Dutch liquid and glycol, and having the composition—

$$\left.\begin{matrix} (C_2H_4)'' \\ H \\ Cl \end{matrix}\right\} O$$

or $(C_2H_4)''O,HCl$, was formed, which was decomposed by caustic potash and the oxide of ethylene set free. The action of oxidising agents on this substance gave rise to two series of compounds, the first of which contained three bodies, represented by the following formulæ:—$C_2H_4O_2$; $C_2H_4O_3$; $C_2H_4O_4$. These were analogous to the series produced by the oxidation of hydrochloric acid, namely, $HClO$; $HClO_2$; $HClO_3$; $HClO_4$. The other series were as follows:—$C_2H_2O_2$; $C_2H_2O_3$; $C_2H_2O_4$; which were comparable with the successive products of oxidation of phosphoretted hydrogen, namely, PHO; PHO_2; PHO_3.

The oxide of ethylene seemed in many of its reactions to bear a remarkable analogy to an oxide of a metal; it was capable of forming definite compounds with acids, and would even displace some mineral bases from their combinations with acids, its salts also formed double compounds with salts of barium, calcium, iron, zinc, copper, lead and mercury, and in these double salts there were always two equivalents of the metal; from various considerations, however, it would appear that the equivalents of these metals should be doubled; an argument in favour of this supposition might be drawn from their specific heat,

* Equivalent of carbon = 12.

which was half that of many of the other elements. If the equivalents were doubled, these metals would occupy a position in the salts precisely similar to that of oxide of ethylene. Oxide of ethylene was capable of combining directly with water by the assistance of heat in the same manner as some metallic oxides; if the water were in excess glycol was formed, but inferior hydrates containing two, three, or four equivalents of the oxide to one of water could be prepared; these were analogous to certain hydrates of stannic and silicic acids; the oxide also combined directly with hydrochloric acid, forming a basic salt.

In addition to this great variety of saline compounds, oxide of ethylene was capable of combining with ammonia, forming compounds containing one, two, or three equivalents of oxide to one of ammonia; of these there were several analogues in the ammonio compounds of copper, mercury, and other metals.

On the whole it would appear that the oxide of ethylene in the compounds it was capable of forming with acids, &c., bore a great resemblance to the mineral oxides, and might serve as a link to connect together the two branches of chemistry by showing the analogies that existed between them.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Annual Meeting,—April 29, 1862.

J. P. Joule, *LL.D., F.R.S., President, in the Chair.*

Mr. Andrew Knowles was elected an Ordinary Member of the Society.

A Paper was read "*On Non-Modular Groups*," by the Rev. T. P. Kirkman, A.M., F.R.S., and Hon. Mem. of the Literary and Philosophical Societies of Manchester and Liverpool.

The following Report of the Council was then read by one of the Secretaries:—

In presenting the Annual Report, the Council congratulate the members on the improved condition of the Society, especially as regards its financial position. The balance in the Treasurer's hands, as seen in his Report annexed, was on March 31, 1861, 58*l.* 2s.; whilst on March 31 last it amounted to 248*l.* 6s. 7d.; and this in spite of necessarily heavy expenditure incurred in the printing of the "Proceedings" and of the Volume I., 3rd series, of "Memoirs," which is now ready for distribution. This improvement is mainly owing to the fact that the members have almost unanimously supported the Council in their proposal to raise the subscription to 2*l.* 2s. per annum, as the only available means of enabling the Society successfully to carry out its objects. The number of ordinary members on the books last year was 207; it is now 204. Since the last Annual Meeting seven members have resigned, eight new members have been elected, and four ordinary members have died, viz.: Professor Eaton Hodgkinson, F.R.S.; Dr. M. Satterthwaite; Mr. Absalom Watkin; and Mr. George Woodhead.

Professor Hodgkinson's high scientific eminence as an experimentalist and as the founder of the principles of many branches of mechanical science, is now universally acknowledged; and the members will be proud to recollect that our illustrious townsman was for forty-one years connected with the Society, and that it was through the medium of our "Memoirs" that the greater part of Hodgkinson's important researches were made known.

The following is the list of Papers published by Professor Hodgkinson in the Society's "Memoirs":—

1. Vol. iv., second series, p. 225.—"On the Transverse Strain and Strength of Materials," read March 22, 1822.

2. Vol. v., second series, p. 354.—"On the Forms of the Catenary in Suspension Bridges," read February 8, 1828.

3. Vol. v., second series, p. 384.—"On the Chain Bridge at Broughton," read February 8, 1828.

4. Vol. v., second series, p. 398.—"A few Remarks on the Menai Bridge," read December 12, 1828.

5. Vol. v., second series, p. 407.—"On the Strength and best Forms of Iron Beams," read April 2, 1830.

6. Vol. vii., second series.—"On the Measure of Moving Force," read April 30, 1844.

The Council congratulate the members on the possession of the life-like bust of the late Professor Hodgkinson, which, thanks to the liberality of a few gentlemen, now adorns our rooms; and they also notice that Mr. Robert Rawson is engaged in preparing a valuable memoir of our deeply lamented friend, the first half of which has already been read before the Society.

Of the Honorary and Corresponding Members, the Council have to notice the deaths of the celebrated French philosopher, M. Biot, and Dr. Peter Barlow, F.R.S.

Notwithstanding the natural claims which the late very successful meeting of the British Association for the Advancement of Science, in Manchester, made upon the scientific resources of our town, it is gratifying to observe no falling off either in the quality or quantity of the original communications presented to the Society during the past Session.

The following is the list of Papers and Communications laid before the Society in the Session 1861-62:—

October 1, 1861.—"Observations of Comet I., 1861," by J. Baxendell, F.R.A.S.

October 15, 1861.—"On the Irregular Barometric Oscillations at Geneva and on the Great St. Bernard, and their relations to the Mean Temperature and the Fall of Rain," by G. V. Vernon, F.R.A.S.

October 29, 1861.—"On the Putrefaction of Blood," No. 1, by Dr. R. Angus Smith, F.R.S.

November 26, 1861.—"Additional Observations on the Permian Beds of South Lancashire," by E. W. Binney, F.R.S., &c. "On certain Scales of some Diurnal Lepidoptera," by Mr. John Watson.

December 10, 1861.—"Nouveau Système de Communication Télégraphique, rendant impossible toute collision de trains sur les chemins de fer," by Professor Baulet, of Persignan, communicated by W. Fairbairn, LL.D., &c.

December 24, 1861.—"On the Influence of the Seasons on the Rate of Decrease of the Temperature of the Atmosphere, with the Increase of Height in Different Latitudes of Europe and Asia," by J. Baxendell, F.R.A.S.

January 7, 1862.—"Experiments on some Amalgams," by the President, J. P. Joule, LL.D., &c. "On the Conductibility of Heat by Amalgams," by Dr. F. Crace Calvert and Mr. Richard Johnson.

January 21, 1862.—On the Action of Nitrate of Sodium on Sulphide of Sodium at different temperatures," by Dr. Ph. Pauli, communicated by Professor Roscoe. "On the Convective Equilibrium of Temperature in the Atmosphere," by Professor William Thomson, LL.D., &c.

February 4, 1862.—"On the Theory of the Transcendental Solution of Algebraic Equations," No. 1, by the Rev. Robert Harley, F.R.A.S. "On the Causes of Sickness and Mortality in the Manufacturing Towns of the North-West of England," by Dr. C. J. Shearman, communicated by Dr. R. Angus Smith.

February 18, 1862.—"Note on a Differential Equation," by A. Cayley, F.R.S. "On the Present State of Meteorology," by T. Hopkins, M.B.M.S.

March 4, 1862.—On the Direction of the Wind at Manchester during the years 1849-61, at 8h. a.m." by G. V. Vernon, F.R.A.S. "On the Theory of the Transcendental Solution of Algebraic Equations," No. 2, by the Rev. Robert Harley, F.R.A.S. "Memoir of the late Professor Eaton Hodgkinson. F.R.S., &c., Part 1st," by Robert Rawson, Honorary Member of the Society. "Observations on Atmospheric Electricity," by Professor Wm. Thomson, LL.D., &c.

March 18, 1862.—"On the Probable Cause of Electrical

Storms," by the President, J. P. Joule, LL.D., &c. "On the Tongues of Mollusca," by Dr. Thomas Alcock. "On the Employment of Galvanised Iron for Armour-plated Ships," by Dr. F. Crace Calvert, F.R.S.

April 1, 1862.—"On the Effect of Increased Temperature upon the Nature of the Light emitted by the Vapour of certain Metals or Metallic Compounds," by Professors Clifton and Roscoe. "Notes on Calorific Phenomena," by J. C. Dyer, V.P.

April 15, 1862.—"On the Theory of the Transcendental Solution of Algebraic Equations," No. 3, by the Rev. Robert Harley, F.R.A.S. "On the Putrefaction of Blood," No. 2, by Dr. R. Angus Smith, F.R.S. "On the Relations between the Decrement of Temperature on ascending in the Atmosphere, and other Meteorological Elements," by J. Baxendell, F.R.A.S.

April 29, 1862.—"On Non-Modular Groups," by the Rev. T. P. Kirkman, A.M., F.R.S., &c.

The Council have decided to print several of the above papers in the next volume of the "Memoirs."

The Report of the Librarian shows that a very large increase has this year taken place in the stock of valuable scientific books of reference in the library. The Society is now (through the unwearied exertions of the Librarian) in regular communication and exchanging publications with all the most important Academies and learned Societies throughout the world; and a scientific library of reference is being gradually collected, the importance of which, especially to those engaged in scientific research, cannot be over-estimated. In order to complete the sets of the series of "Transactions," the current and future numbers of which are obtained in exchange for the Society's "Memoirs," a much larger sum of money is requisite than the ordinary income of the Society can supply. A sum of 85*l.* 7s., being the balances due to the subscribers of the British Association local fund, has been kindly placed by them in the hands of the Treasurer, for the purpose of assisting our Librarian to carry out this important work.

In conclusion, the Council report with satisfaction the increased activity and usefulness of the Sections, as shown by their published proceedings.

MICROSCOPICAL SECTION.

April 27, 1862.

E. W. Binney, *F.R.S., F.G.S., in the Chair.*

Contributions were acknowledged from Captain James Clarke, of the ship *Lightning*, consisting of a specimen of mud from Hobson's Bay, Australia; a specimen of *Fucus natans*, or gulf-weed, from the Sargasso Sea; and sand, &c., from a sounding off the south coast of Ireland. Captain Contente, of the Portuguese steamer *Lusitania*, forwarded a sounding taken between Cape Carvoëiro and the Berling Islands, off the Coast of Portugal.

Professor Calvert presented to the members of the Section a number of bottles containing carbolic acid in crystals, for the purpose of experimenting upon its utility as a preservative fluid for microscopical objects, as well as for specimens of natural history.

Mr. Thomas D. Toase, of Jamaica, presented, through Professor Calvert, specimens of *Diatomacea*, from Kingston Harbour; pollen of a West Indian lily; a portion of a plantain leaf, with two mounted slides of the same, showing cells, raphides, &c. Mr. Toase also sent drawings and description of a rotifer, found upon Conferva, at Jamaica, which is not known to any of the members present. It consists of an oval body or outer case of a brownish colour, $\frac{1}{100}$th of an inch in length; from near one end of the oval is protruded a transparent neck or contractile body, furnished when protruded with four hairs or feelers, a lip, and a kind of operculum, around which Mr. Toase recognised the presence of cilia by the current of water, but he failed to discover the cilia for want of defining power in his microscope. There is not sufficient detail either in the description or the drawings to be of much use to the microscopist, but further information has been written for.

Mr. Brothers presented to the Section photographs of the four drawings by Dr. Alcock, illustrating his paper on the "*Tongues of the Mollusca.*"

Mr. Sidebotham exhibited a drawing of an undescribed species of *Zygnema*, found by Mr. Watson and himself, at Southport, in brackish water. It exhibited no appearance of conjugation, and the spores were like balls covered with spines, which when released from the cells move rapidly through the water like volvox.

Mr. Mosley reported upon the specimen of the outer coating of a bulb, received through Dr. Fairbairn from Mr. Niven, of Jeffrey's Bay, Cape of Good Hope. On examination with the microscope, he found that the leaf is about $\frac{1}{100}$th of an inch in thickness; that between the outer and the inner cuticle a number of tubes or vessels run longitudinally through the structure of the leaf; and that these tubes are composed of very delicate fibres, coiled up so as to form spiral vessels. On breaking the leaf, the fibres may be uncoiled and drawn out to an almost indefinite extent. From the thicker middle portion Mr. Mosley had drawn out fibres to the extent of eighteen or twenty inches without breaking; they are beautifully fine, but are, in his opinion, too weak and delicate, as well as too long, to be used as a substitute for cotton. He considers it possible some application may be found for the fibre, if a sufficient sample were sent for experimental trials. Desirous of knowing the botanical history of the plant, he wrote to Sir W. J. Hooker, Director of the Royal Gardens at Kew; but it was not possible to classify the plant with certainty from a specimen so imperfect. Sir William observed that in all probability it is one of the *Amaryllidaceæ*, and possibly of the genus *Buphane*.

Mr. Mosley also exhibited the leaf of a new species of Digitalis, brought by Mr. Hurst from the Hazaree-bagh, Bengal, upon the surface of which the poison-glands are closely set in groups of four glands, placed in a lozenge form.

Mr. Nevill exhibited a new form of microscope, by Matthews, of London, for field use or class instruction, which can be used with either transmitted or reflected light. Mr. Nevill proposed that the subject for discussion at the next meeting should be, "*On the Motion of the Naviculæ*," which was agreed to.

NOTICES OF BOOKS.

A Manual of Domestic Economy. By W. B. Tegetmeier. Sixth Edition. Home and Colonial School Society, Gray's Inn Road, London. 1862.

In noticing the sixth edition of Mr. Tegetmeier's "Domestic Economy," it is scarcely necessary for us to do more than state our conviction that the success and general appreciation of the book are well merited. The way in which the more important applications of chemical principles to the cooking of meat, and many other household operations are explained, must effect much benefit. The author does not hesitate to express in decided terms the conclusions of sound science, even where they militate against popular notions. His remarks on the injurious results of the salting process on meat are very just; whilst in many matters which scarcely come within our scope, his advice is not less admirable: where, for instance, he guards the novice in dusting from the two extremes of "flapping the dust from one end of a room to the other," and those more violent measures which produce dust rather than get rid of it; the dust being in such cases nothing more than furniture in a fine state of division.

Royal Institution.—Friday, June 20, at Eight o'clock, Professor Faraday, F.R.S., "On Gas, Glass Furnaces, etc."

NOTICES OF PATENTS.

1481. *Treating Skins.* J. Steart, Blue Anchor Road, Bermondsey, London. Dated June 10, 1861.

The inventor employs in the preparatory stage of treating skins a gelatinous product obtained by digesting fish with water, under heat and pressure, until it is reduced to a pulpy mass. By the use of this animal product, the depilated skins are rendered fit to undergo the tanning process in the usual manner.

1489. *Impermeable Varnish for Leather.* C. Stevens, Charing Cross, London. (A communication.) Dated June 11, 1861.

For the purpose of coating leather, and conferring upon it waterproofing qualities, the patentee employs a varnish composed of india-rubber dissolved in bisulphide of carbon, and he prefers to use this in conjunction with another varnish made with gutta-percha instead of india-rubber, and applied as a second coating. In the preparation of these varnishes one part of either of the natural gums is dissolved in a closed vessel in four parts of bisulphide of carbon.

1566. *Cements or Adhesive Solutions for Joining or Connecting together Surfaces or Articles of Leather, Wood, Paper, &c.* J. McKay, Birmingham. Dated June 18, 1861.

The patentee employs in the manufacture of these adhesive solutions the bisulphide of carbon, in which he dissolves purified gutta-percha, leaving the vessel open in order *to allow the bisulphide of carbon to take up a sufficient amount of oxygen!*

Is there not something anomalous in the state of the law which permits of the sealing of two patent claims within the period of a week for the selfsame material and application? The chief difference in the wording of the preceding specifications arises from an erroneous conception in regard to the nature and chemical properties of the bisulphide of carbon, so that while one patentee makes his varnish in a closed vessel to avoid loss of solvent, the other specifically directs the vessel to be left open. Furthermore, it is impossible that either of these patent claims can be supported on the score of novelty, since the same suggestion was made the subject of a provisional specification more than a year earlier, but was not proceeded with at the expiration of six months.*

1507. *Converting Vegetable Fibrous Substances into Pulp.* J. Watt, Camberwell, Surrey. Dated June 12, 1861.

This invention relates to a mode of treatment for converting fibrous vegetable structures into pulp, and consists in submitting them to the action of carbonate of soda or potassa, at the boiling point of these solutions, wherein they are left until the fibres of the vegetable substance have become loosened and separated. They are then washed and bleached either with chloride of lime, or chloride of soda, and are then ready to be made into pulp.

These operations, in the same order of succession, have frequently been applied to the disintegration of soft vegetable structures, the patent claim of Mr. J. H. Johnson † may be quoted as an illustration of a similar mode of treatment.

1543. *Bleaching Coloured Rags and Vegetable Fibres.* T. Gray, Union Road, Wandsworth. Dated June 17, 1861.

The patentee claims the steeping of the substances to be bleached in diluted muriatic acid prior to their being submitted to the action of chloride of lime or other bleaching liquor.

This expedient has long been known, and frequently been put in practice in bleaching and dye works as a means of liberating more quickly the active chlorine.

* Chemical News, vol. iv. p. 82.

† *Ibid.* vol. iv, p. 224.

CORRESPONDENCE.

Bluish Vapours of Chloride of Ammonium.

To the Editor of the Chemical News.

Sir,—I am very glad to learn, by the remarks appended to my letter in your last Number, that I was mistaken in supposing that any part of your comments on the chemical evidence given on the trials of "Downing *v.* Chance," and "Timmins *v.* the Staffordshire Gas Company" was intended as a personal attack upon myself. This being the case, I shall have much pleasure in explaining those parts of my evidence which, as reported in the newspapers, have appeared so "curious" to yourself, and, probably, to your readers.

Firstly, as regards the bluish vapours or rather fumes. My evidence on the point was correctly enough reported so far as the report went; but being only a short abstract, the explanation which I gave of the circumstances under which the blue colour was exhibited was not included. This explanation was brought out by the cross-examination, and had it been fully reported there would have been no need for that which I am now about to supply.

The experiments were made as follows:—After an hour's driving round about the works, and climbing over the ashy mounds of the black wilderness of their vicinity, I fixed upon a retired acre of cinders, along which the "chemical smoke," or "chemical," as it is called by the natives, was occasionally drifting whenever the variable gusts of wind then blowing beat it downwards. I poured about two ounces of strong solution of ammonia on one cone of cinders, and two ounces more on another at some distance from it, and on a cord stretched between two walking-sticks hung some filtering paper saturated with ammonia, both of which were previously tested and found free from any trace of hydrochloric acid. This paper was afterwards taken to the laboratory of the Midland Institution, and tested in the usual manner for chloride. Whenever a waif of "chemical" (which is a readily distinguishable white vapour) drifted over the ground, bluish fumes hung around and drifted from the mounds and the paper, most notably from the cinder cones. The colour of these fumes was identical with that which reeks from the tip of a good cigar, and is so readily distinguishable in tint from that which comes from the mouth of the smoker. To the chemist who is only familiar with the usual laboratory experiment of bringing a stirring rod moistened with strong hydrochloric acid into the midst of ammoniacal gas, this bluish colour may be a novelty, but to the farmers whose dunghills are occasionally visited by the fumes from alkali works it is a very familiar phenomenon. The primary condition determining this bluish colour is that the hydrochloric acid fumes shall be much diffused and spread over a large space. The tint is heightened if the fumes are projected against a dark background, and the spectator stands with his back to the light.

These conditions may easily be fulfilled in a laboratory, by pouring a little solution of ammonia on the ground, or on a table, &c., and standing with back to the window, at a distance of a yard or two from the ammonia, blowing the fumes from the mouth of a hydrochloric acid bottle towards the ammonia. The bluish colour of the chloride of ammonium fumes will then be sufficiently apparent.

This is a very simple experiment to establish a very simple fact; but it appears to be really of some importance in judicial inquiries of this kind, for the chemist who has only seen the white fumes formed in the usual way of

testing for ammonia, or in the lecture-table experiment of mixing the two gases in a more concentrated form, would question the accuracy of the statements concerning these blue vapours over the dunghills, which were made by all the unlearned agricultural witnesses in the trial of "Downing *v.* Chance."

If I might venture upon an explanation of the differences of colour of such fumes under such circumstances, it would be this. These fumes consist, of course, of minute crystals of chloride of ammonium. A clear crystal, or a clear homogeneous mass of this salt, has a rather strong bluish tint, while a mass of detached or lightly cohering crystals is snowy white, and semi-opaque. This is a very common, I might say a general variation in the appearance of such bodies; the difference between ice and snow, rock salt, and granular table salt, lump sugar, and single sugar crystals, &c., are familiar illustrations.

The fumes of hydrochloric acid consist of particles of the gas combined with water in the vesicular state or otherwise. When these particles are comparatively far apart, as in the more diffused fumes, they would form isolated crystals; but when close together, as in the dense fumes, it is very probable that many crystals would coalesce at the moment of formation, and form flocculi-like snow flakes, or granular hail, and these flakes, of course, would be white, and the isolated crystals of the more diffused fumes bluish.

This explanation is further confirmed by the fact that the bluish fumes remain suspended in the air a far longer time than the white fumes, indicating that the blue fumes consist of smaller particles than the white fumes.

There are other cases of a similar kind that may be explained in the same manner, but regard for your space and my own time prevents me from citing further illustrations at present. For the same reasons I must postpone until next week an account of the unexpected Magenta-coloured reaction I obtained by the direct action of dilute nitric acid upon certain coal products.—I am, &c.

W. Mattieu Williams.

Hall Road, Handsworth, near Birmingham, June 10.

MISCELLANEOUS.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

The thirty-second meeting will commence at Cambridge on Wednesday, October 1, 1862, under the direction of the following officers:—*President*, Rev. R. Willis, M.A., F.R.S., Jacksonian Professor of Natural and Experimental Philosophy in the University of Cambridge. *Vice-Presidents*—The Very Rev. Harvey Goodwin, D.D., Dean of Ely; the Rev. W. Whewell, D.D., F.R.S., Master of Trinity College, Cambridge; the Rev. A. Sedgwick, M.A., D.C.L., F.R.S., Woodwardian Professor of Geology in the University of Cambridge; G. B. Airy, Esq., M.A., D.C.L., F.R.S., Astronomer Royal; J. C. Adams, Esq., M.A., D.C.L., F.R.S., Pres. C.P.S., Lowndean Professor of Astronomy and Geometry in the University of Cambridge; G. G. Stokes, Esq., M.A., D.C.L., Sec. R.S., Lucasian Professor of Mathematics in the University of Cambridge. *General Secretary*—William Hopkins, Esq., M.A., LL.D., F.R.S., F.G.S., Cambridge. *Assistant-General Secretary*—John Phillips, Esq., M.A., LL.D., F.R.S., F.G.S., Professor of Geology in the University of Oxford. *General Treasurer*—William Spottiswoode, Esq., M.A., F.R.S., F.G.S., F.R.A.S., &c., 19, Chester-street, Belgrave-square, London, S.W. *Local Secretaries for the Meeting at Cambridge*—Charles C. Babington, Esq., M.A., F.R.S., F.L.S., Professor of Botany in the University of Cambridge. G. D. Liveing, Esq., M.A., Professor of Chemistry in the University of Cambridge. The Rev. N. M. Ferrers, M.A., Gonville and Caius College. *Local Treasurer for the Meeting at Cambridge*—The Rev. W. M. Campion, M.A., Queen's College.

The General Committee will meet on Wednesday, October 1, at 1 p.m., for the election of sectional officers, and the despatch of business usually brought before that body. On this occasion there will be presented the report of the council, embodying their proceedings during the past year. The general committee will meet afterwards by adjournment.

The first general meeting will be held on Wednesday, October 1, at 8 p.m., when the President will deliver an address; the concluding meeting on Wednesday, October 8, at 3 p.m., when the Association will be adjourned to its next place of meeting.

At two evening meetings, which will take place at 8 p.m., discourses on certain branches of science will be delivered.

There will also be other evening meetings, at which opportunity will be afforded for general conversation among the members.

The Committee of Sections will meet daily, from Thursday, October 2, to Wednesday, October 8, inclusive, at 10 a.m. precisely.

The Sections will meet daily, from Thursday, October 2, to Tuesday, October 7, inclusive, at 11 a.m. precisely.

Reports on the Progress of Science, and of researches entrusted to individuals and committees, and other communications intended for presentation to the Sections, are expected to be forwarded in letters addressed to the Assistant General Secretary, at Cambridge, previously to the meeting, accompanied by a statement whether the author will be present, and on what day, so that the business of the Sections may be satisfactorily arranged.

The reports complete, and concise abstracts of other communications, are to be delivered to the Secretaries of the Sections before which they are read, previously to the close of the meeting, for publication in the *Transactions*. As the Reports on Science may be more interesting to more Sections than the one which originally called for them, it is desirable that the authors should be prepared to furnish the means of reading them in any other Section at the request of the President and Secretaries of that Section.

The following are the titles of the Sections to which communications may be presented:—Section A. Mathematics and Physics. B. Chemistry and Mineralogy, including their applications to Agriculture and the Arts. C. Geology. D. Zoology and Botany, including Physiology, Sub-Section D. E. Geography and Ethnology. F. Economic Science and Statistics. G. Mechanical Science.

Members will receive their tickets in the reception room according to their class of membership, separate registers being appointed for old life members, old annual subscribers, new annual subscribers, and associates.

Gentlemen desirous of attending the meeting will find in the reception room blank forms of proposal, and may make their choice of being proposed as life members, paying ten pounds as a composition, or annual subscribers, paying one pound annually and an admission fee of one pound (making together two pounds on admission), or associates for the meeting, paying one pound.

Ladies may obtain tickets, through the application of a member, in the reception room, price one pound each ticket. These tickets are transferable to other ladies only.

Compositions, subscriptions, and arrears are received during the meeting in the reception room, and at all times by the Local and General Treasurers.

Chemical Society.—The next meeting of this Society, being also the last of the season, will be held on Thursday next, June 19, at 8 p.m., when a lecture will be delivered by Dr. Marcet, F.R.S., "On the Chemistry of Digestion."

THE CHEMICAL NEWS.

VOL. V. No. 133.—June 21, 1862.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Preparation of Oxalic Ether, by M. KOLBE.*

MIX 180 grs. of oxalic acid, dried at 100°, with 100 grs. of acid sulphate of potash, and submit them, in a retort, to the action of a temperature of 150° or 180° C. Then drop gradually into the tubulure of the retort a mixture of 250 grs. of absolute alcohol and 25 grs. of concentrated sulphuric acid; cohobate and distil at a temperature not below 150° C. After shaking up the product of the distillation with some water, dry over chloride of calcium and rectify. The product is about 70 per cent. of the quantity indicated by theory, basing it on the oxalic acid used. By adding ammonia to the mother-waters, a considerable quantity of oxamide is also obtained.

On the Arseniferous Sulphur from the Solfatara of Naples, and Preparation of Selenium, by Dr. T. L. PHIPSON, *F.C.S. Lond., Member of the Chemical Society of Paris, &c.*

THIS sulphur is of an orange colour, only partially soluble in sulphide of carbon, differing in this respect from the crystallised sulphur of Sicily, which I find to be totally soluble in sulphide of carbon. It contains both selenium and arsenic; the latter in considerable quantity. The following is the analysis of an average sample:—

Sulphur	87·600
Arsenic	11·162
Selenium	0·264
	99·026

Or,—

Sulphur	80·458
Sulphide of arsenic, AsS_3 . .	18·304
Selenium	0·264
	99·026

When heated in a platinum capsule it almost entirely disappears, leaving only a slight trace of a black substance, insoluble in nitric acid, and giving the reaction of silica before the blow-pipe.

Of the 87·600 of total sulphur contained in this substance, 64·26 dissolve readily in *aqua regia*, but the remaining 23·34 per cent. resist the action of *aqua regia*, and were not dissolved after boiling with excess of the latter for upwards of two hours.

The preparation of selenium from this impure sulphur is not difficult. The substance being finely pulverised, is oxidised by *aqua regia*, the solution diluted and filtered, thus separating the sulphur which remains undissolved. A few crystals of sulphite of soda are then dissolved in the liquid, until the latter acquires a permanent odour of sulphurous acid, and the whole allowed to repose for about forty-eight hours, at the expiration of which the whole of the selenium is deposited as a rose-red powder. In this manner from 0·3 to 0·4 per cent. of selenium can be obtained*; but if, instead of using *aqua regia* to oxidise the sulphur, the latter be treated with carbonate of soda and nitre, in the dry way, no selenium at all is obtained.

Reduction of Sulphuric Acid by Nascent Hydrogen, by M. KOLBE.†

IT is an established fact that when sulphurous acid is reduced by nascent hydrogen sulphuretted hydrogen is produced. MM. Fordos and Gélis have founded upon this reaction a very simple process for recognising the presence of sulphurous acid; but it has hitherto been unknown that sulphuric itself, under these circumstances, undergoes a similar reduction. Hydrosulphuric gas, which always contaminates hydrogen prepared with zinc, water, and sulphuric acid, has probably no other origin. M. Kolbe has remarked that this gas is developed in increasing quantity proportioned to the degree of concentration and high temperature of the acid.

This is not an unimportant phenomenon. It can be prevented by employing sulphuric acid previously diluted with twice its volume of water, in which case pure hydrogen is produced; but if concentrated acid is then poured into the mixture, hydrosulphuric gas speedily appears.

This circumstance especially deserves attention when Marsh's apparatus is employed; in fact, under these circumstances the sulphuretted hydrogen may convert the arsenious acid either wholly or partially into unalterable sulphide of arsenic, and thus from the presumed absence of arsenic in the suspected matter lead to a wrong conclusion.‡

Preparation of Iodide of Calcium.—Hesse (*Chemisches Central-Blatt*, 1862, p. 174) prepares iodide of calcium by adding milk of lime in excess to a solution of the iodide of iron. Any excess of lime in solution with the iodide of calcium is separated by evaporating the solution, or better, by passing carbonic acid through the hot solution.

* *Annalen der Chemie und Pharmacie*, vol. cxix. p. 172.

* That is the amount yielded by the specimens I have examined; but I believe the environs of Naples furnish sulphur which, treated as above, would yield considerably more.

† *Annalen der Chemie und Pharmacie*, vol. cxix. p. 174.

‡ This fact connects itself with the other facts ascertained by M. Blondlot, whilst treating organic matters with sulphuric acid in order to isolate arsenic. The error can doubtless be rectified by the means this chemist indicates. (See *Journal de Pharmacie et de Chemie*, vol. xxxi. p. 117.)

TECHNICAL CHEMISTRY.

On Aluminium, by J. W. M'Gauley.

There is no subject connected with chemistry of more importance than that of the metals; nor is it an exaggeration to assert that they have been the principal agents in civilisation. We are chiefly indebted for the latter, however, not to those which are costly and rare, but to that which, by a beneficent dispensation of Providence, is the most common of them all; since iron has been of far more value to mankind than gold.

The ancients were acquainted with but few metals. In very remote antiquity, metallurgical knowledge was almost confined to copper and tin, which were more easily reduced than iron. And, as it was soon discovered that some of their alloys were extremely hard, they were employed in the manufacture of axes, swords, and other weapons, after still ruder materials were discarded; hence, warlike and domestic instruments of bronze are found among the oldest relics of distant ages. But iron at length obtained its fitting place among the substances useful to man; and its abundance, its extreme softness when softness is required, its extreme hardness when that quality is desirable, its elasticity, its malleability, ductility, tenacity, and other admirable qualities, have caused it to acquire and maintain the very first rank among the useful metals. Gold and silver, it is true, have been known from the earliest times; but their use, like their supply, has always been limited; and much of the value attached to them is due to their scarcity. Lead, also, and zinc—at least as an ore, and as one of the ingredients of brass—were familiar to the ancients. But we have now enumerated all the metals with which they were acquainted; and, in truth, all that to any great extent have yet been utilised.

We are, on this occasion, specially to treat of a metal which has been a source of great expectations; and, fortunately, there is no reason to consider that these have been disappointed; their complete realisation is only deferred, and most probably for but a short period; and one of our objects in directing attention to it, is to excite a more general inquiry regarding it. The establishment of aluminium among the most important of the metals is a mere question of the cheapness of its production; and as, up to this time at least, it is most conveniently obtained by means of sodium, investigations regarding it resolve themselves into a determination of the most economical method of obtaining that metal. On this point our knowledge has also progressed considerably, and hence the price of aluminium has greatly fallen. Not long ago it was 3*l.* per ounce, it is now only about 5*s.*; and it will, no doubt, be far less, if we are to judge by the extraordinary improvements always made, after a time, in chemical processes. How much lower in price are the most useful substances at present than they were a few years ago, because the methods of manufacturing them have been simplified. But even at its present cost, which, by weight, is the same as that of silver, aluminium is really only one-fourth as dear, bulk for bulk; and this, after all, is the test, since bulk for bulk, it is as strong, and even stronger than silver. When there is question, however, of its application to domestic purposes, we must compare its cost with that of pewter or copper; it would chiefly supersede these, which, among other disadvantages, are productive of very noxious compounds, particularly the copper.

The qualities of the precious metals are quite distinct from those of the more common; nor have the two classes hitherto been connected by any intermediate metal—that is, by one possessing the most characteristic properties of each; but it is hoped that aluminium may supply such a connection. Like the precious metals, it is brilliant, and little alterable by chemical agents—scarcely at all, under ordinary circumstances. Like the common metals it is very abundant, constituting one-fourth, by weight, of the most widely diffused bodies. It is malleable, ductile, hard, and tenacious; its compounds are harmless—which is true of scarcely any other metal but iron; and, unlike both the precious and the common metals, it has the advantage of being extremely light. It is admirably suited to all ordinary purposes, and is one of the best that can be used for those which are artistic and ornamental. M. Christofle, in 1858, exhibited before the Academy of Sciences a group in aluminium, which had been cast and chiseled, and which afforded an excellent example of its capabilities, though it was its first application to such a purpose.

Davy, soon after he had discovered the metallic bases of the alkaline earths, in 1807, proved the existence of aluminium, from potassa being produced when the vapour of potassium was brought in contact with alumina heated to whiteness; and he even obtained it in combination with potassium. It was procured by Wöhler, in 1827, as a grey powder, and, in 1845, in the form of very minute globules; but probably from being more or less impure, it did not exhibit the same properties as when in a massive state. On account of the high price of potassium at the time he made his experiments, and other obstacles, he did not obtain it in particles larger than a pin's head; and he succeeded in uniting these only by great chance, and after many trials; since the presence of minute quantities of other substances, or a slight coating of oxide, would prevent their coalescing. M. Degousse, a gold-beater, of Paris, succeeded in preparing it in the form of very thin plates; and he found that in beating them out, it was necessary to re-heat them more frequently than other metals in similar circumstances. M. Deville has been the most successful of all those who have made experiments upon it. We shall first describe the most convenient methods of obtaining aluminium, particularly on the small scale, and shall then examine its properties and combinations—omitting nothing of any importance that has yet been discovered regarding it. As to the mode of procuring it on the large scale, it does not concern the object we have in view; but it may, in a great degree, be conceived from what we shall say.

When we attempt to get aluminium directly from alumina, with potassium, or sodium, we do not succeed; most likely from its being necessary that the potash or soda, which would then be formed, should unite with some of the undecomposed oxide, which does not seem to occur, though aluminates of the alkalies are very easily made. But M. Chapelle, in 1854, procured it by introducing pulverised clay, sea-salt, and powdered charcoal into a common crucible, and heating the mixture with coke, though not to whiteness, in a reverberatory furnace. When the crucible was cold, a considerable quantity of minute globules of aluminium were found at the bottom. It must be admitted that the simplicity of this method, if it could be rendered economical, would make it deserving of preference; and it is not improbable that it may hereafter be so improved as to supersede all others. To obtain aluminium through the medium of a troublesome metal seems at best a clumsy process. It is, however, the most successful that has been yet devised; and

we are indebted for it in its present improved state to the ingenuity and researches of Deville, whose method is a modification of Wöhler's. He received from the present Emperor Napoleon the funds necessary for making his experiments on a large scale, and in a satisfactory manner, and he first published an account of them in 1854.

It occurred to him that, on account of its smaller equivalent, and the commercial value of its salts, sodium would be better for the purpose of obtaining aluminium than potassium, which had been employed by Wöhler. Other advantages, besides, were found to follow from its adoption. The manufacture of sodium is easier, and even safer, than that of potassium; and when the process goes on well, those carbon compounds which are so annoying with potassium, do not make their appearance, nor is its reduction accompanied by the explosive substances—probably compounds of hydrogen—which are so dangerous in the reduction of potassium. Moreover, the use of potassium in obtaining aluminium is not very safe,—it inflames so easily, and often produces such violent explosions; while sodium can be employed without fear, since it may be raised in the atmosphere to a higher temperature than its point of fusion. Indeed, we have reason to believe that it is inflammable only in a state of vapour, though still at a temperature below its boiling-point; and if it is kept very carefully from water, there will be little likelihood of its taking fire.

To get pure aluminium by Deville's method, we require pure alumina, pure chloride of aluminium, and metallic sodium; for any impurities present in these will be concentrated in the aluminium, and affect its properties very much, nor, if once combined with it, can they ever be entirely removed. We shall first, therefore, describe how these are to be had.

To Obtain Pure Alumina.—Eight and a-half parts, by weight, of the sulphate of alumina of commerce for every required part, by weight, of pure alumina, are dissolved in an equal weight of water, and precipitated by a concentrated and boiling solution of acetate of lead in slight excess, and the smallest possible quantity of tartaric acid is added to the liquor, which is separated by decantation, to prevent the precipitation of alumina. The acetate of alumina is then supersaturated with ammonia, and the ammoniacal solution, after being treated with hydrosulphuret of ammonia in a closed vessel, is placed in a stove having a temperature of from 122° to 140° F. This determines the precipitation of the sulphurets of iron and lead, which are removed first by decantation, and then by filtering, but without washing the filters. The clear and slightly-yellow liquor, which consists of acetate and tartrate of alumina combined with ammonia, and some hydrosulphuret of ammonia, is rapidly evaporated and carbonised in an earthen crucible. The residual mixture of alumina and carbon is made into a paste with oil, and strongly calcined to expel the sulphur, due to a little sulphuric acid which remains in the alumina, the whole of it not having been separated by the acetate of lead.

To Obtain Pure Chloride of Aluminium.—Some of the mixture of alumina and carbon, just mentioned, is introduced into a porcelain tube that has been fitted with another tube, and is heated to redness in a current of dry chlorine. Chloride of aluminium sublimes, and is removed from the tubes in compact masses, which are composed of very beautiful crystals, that are either colourless or slightly tinged with yellow. If, however, from the impurity of the materials, this chloride is not found to be quite pure, it is heated with nails or iron-turnings, in an earthen or cast-iron vessel, which, when the permanent gases have passed off, is closed; after which, the heat being continued, a slight pressure results that causes the chloride of aluminium to melt and come in contact with the iron. This changes the volatile perchloride of that metal into the protochloride, which is comparatively fixed, and the chloride of aluminium, completely purified, crystallises in the vessel itself in large transparent and colourless prisms, and a distillation in hydrogen finishes the process.

To Obtain the Sodium.—Its preparation is founded on the reaction of an alkaline carbonate on carbon; and carbonate of soda, wood charcoal, and carbonate of lime are required in the following proportions:—

Carbonate of soda . . .	717
Wood charcoal	175
Chalk	108
	1000

The carbonate of soda should be obtained from crystals dried and pulverised fine; the carbon and chalk should also be reduced to powder, and the whole, as soon as possible after having been mixed, should be made into a paste with very dry oil, and then calcined at a red heat in an iron mercury bottle, that it may occupy a small space, and thus a larger quantity of potassium be obtained by the subsequent process. The calcined mass is subjected to a high heat in an iron mercury bottle, which is not so rapidly destroyed as might be expected, and ought to last for three or four operations. It is kept comparatively cool by the resulting oxide of carbon, and by the sodium assuming an aëriform state, and the heat required is not near so great as might be supposed. An iron tube leads from the bottle, which is inside the furnace, to a receiver, which is outside, and has an aperture for the escape of the gases. The carbonic oxide formed from the chalk assists in carrying the vapour of sodium rapidly into the receiver, and thus prevents it from decomposing any of the gas by which it is necessarily surrounded,—an effect that would be facilitated by its finely divided state as vapour. The receiver, also, is thus kept hot enough to unite the metallic globules without a wasteful after process. One-seventh of the weight of the mixture which has been used, or one-fourth of the weight of the carbonate of soda, should be obtained in sodium. If the mixture employed has been such as to melt, it will have prevented a free disengagement of the gases.

To Obtain the Aluminium.—From 3000 to 5000 grains of chloride of aluminium are placed in a tube of glass or porcelain, about one and a-half inches interior diameter, and are insulated by two plugs of asbestos. Hydrogen, purified and dried by being transmitted through sulphuric acid and chloride of calcium, is sent through the tube; and while it is passing, the chloride of aluminium is gently heated by a few coals, to drive away any hydrochloric acid which may have been formed by the action of the air on the chloride, and also the chlorides of sulphur and silicium which are invariably present in small quantities. Sodium, previously crushed between two pieces of dry filtering paper, and placed in a boat, is then introduced into one end of the tube while it is still full of hydrogen, and is melted. The chloride is at the same time heated so as to make it rise in vapour, that it may come in contact with the sodium, and be decomposed; and when the sodium has disappeared, and the chloride of sodium that has been formed is saturated with chloride of aluminium, the process is complete.

An incandescence which occurs is easily regulated. The boat being taken from the tube, the mixed chlorides, in which the globules of aluminium are suspended, are removed by dissolving in water, and the globules, covered up in a porcelain crucible either with mixed chlorides of aluminium and sodium or with common salt, are fused together by a strong heat.

This process answers still better on the large scale; but, instead of the porcelain tube and boat, two cast iron cylinders connected by a smaller tube of iron are employed. The anterior cylinder contains the chloride of aluminium; the posterior, sodium in a tray; and the iron tube, kept at a temperature of from 400° to 500° F., scraps of iron to separate any of that metal which may rise with the vapour of chloride of aluminium, by changing it from volatile per- to fixed proto-chloride.

Œrsted, who was the first to form chloride of aluminium, is said to have obtained that metal by heating the chloride with an amalgam of potassium, rich in the latter, and driving off the mercury from the resulting amalgam of aluminium by heat.

Aluminium may also be procured from cryolite, a mineral which exists abundantly in Greenland, though it is found only in small quantities elsewhere. It is a double fluoride of aluminium and sodium, and may be produced artificially by adding hydrofluoric acid in excess to calcined aluminium and carbonate of soda, so as to produce—

Fluorine	54·5
Aluminium	13·0
Sodium	32·5
	100·0

Then evaporating, and fusing the result. Both the native and factitious cryolite give aluminium with sodium, and with the galvanic current. The latter, with a mere mixture of aluminium and fluoride of sodium, would afford only sodium and fluorine. It occurred to Rose that, on account of the deliquescence and volatility of the chlorides of the alkaline metals, and the necessity, when they are employed, of preventing any excess of atmospheric air, it would be better in the reduction of aluminium to use a fluoride of that metal combined with an alkaline fluoride; and he proposed to use cryolite, but was deterred by its scarcity at that period. To obtain aluminium in this way, finely powdered cryolite and sodium are placed alternately in layers in cast iron crucibles, the whole being covered with a good thickness of chloride of potassium as a flux. The crucible is then carefully closed with a porcelain cover, and raised to a red heat for half an hour; after which, the calcined matter having been softened with water, it is broken down in a porcelain mortar. The larger globules of aluminium are easily separated mechanically; the smaller, by dissolving away the mass in which they are imbedded with nitric acid without heat. The globules are fused, as before, under the mixed chlorides or common salt. Without this the slight coating of oxide on their surface would prevent their union. When common salt alone is used a higher temperature is required. The aluminium obtained from cryolite almost always contains silicium, and even iron; and the product is not abundant, since 10 of cryolite and 4 of sodium give only 0·5 of aluminium. Rose attempted also to procure aluminium by placing the mixed chloride of aluminium and sodium in alternate layers with sodium; but the results were not satisfactory.

(To be continued.)

On Aluminate of Baryta and Pure Alumina Salts for Industrial Purposes, by M. Gaudin.

On commencing my researches I believed, with all other chemists, that aluminate of baryta was insoluble like the aluminates of lime, magnesia, or zinc. The circumstances which led to the rectification of this erroneous impression are sufficiently interesting to deserve being made known.

A manufacturer, ignorant of the most simple rules of chemistry, had a fixed idea that chloride of barium could be transformed into baryta simply by the action of aqueous vapour. He engaged my services to put his idea to the proof. This task appeared to me all the more difficult from the fact that, chloride of barium being fusible at red heat, it would be necessary to force the vapour through a liquid; however, I procured some earthen syphons, hoping, meanwhile, to find a solution to this problem by experimenting upon chloride of barium mixed with an infusible matter, serving as a support, and capable of playing the part of an energetic acid, and displacing hydrochloric acid.

This I had no difficulty in finding; I used from the first calcined alumina, which, under this treatment, served as aluminic acid, and by this means I sought to produce an insoluble aluminate of baryta susceptible ultimately of decomposition, by prolonged boiling, into hydrates of alumina and baryta.

Aqueous vapour, passing through a granular mixture of alumina and chloride of barium heated to redness, produces, in fact, an abundant disengagement of hydrochloric acid; and the fritt treated by boiling water, and filtered, gives a colourless, limpid, and highly alkaline liquid, precipitating abundantly with sulphuric acid and sulphates. This I believed to be the baryta I was searching for; but was surprised to find that the liquid yielded an abundant precipitate with weak nitric and hydrochloric acids.

The liquid held aluminate of baryta in solution, and this aluminate, though less soluble, should henceforth be classed with aluminates of soda and potash, just as baryta itself is less soluble than soda and potash. In fact, the aluminates of soda and potash diluted with water produce no precipitate with soluble salts of baryta. Aluminate of lime, on the contrary, is so insoluble that lime water poured into a solution of aluminate of baryta determines in it, after a few seconds, a brilliant precipitate of aluminate of lime; so that, by adding to the fritt, before boiling it, milk of lime in excess, the filtered liquid issues a solution of hydrate of baryta perfectly free from alumen.

Chlorine of barium being too expensive, I substituted for it, in a series of successive experiments, sulphate of baryta, a mixture of sulphate of baryta, Provençal ferruginous alumina, and charcoal, previously submitted to the action of aqueous vapour in excess. The fritt, treated by boiling water, also produced a limpid, colourless solution, yielding no indications of iron or of sulphocyanide of potassium, or sulphide of barium tested by acetate of lead, resembling, in this respect, soluble aluminate of baryta.

By operating in this way, the sulphuric acid of the attacked sulphate of baryta is drawn away by the vapour in the form of sulphide of carbon, sulphur, sulphurous acid, and sulphuretted hydrogen gas. Crystallised sulphur is often deposited in the receiver, together with a large quantity of milky water, a real milk of sulphur which filters through paper perfectly, without any change in appearance.

This milk of sulphur, which is perfectly free from alkali, may one day, perhaps, be employed in place of flour of sulphur, if not for agricultural purposes, on account of the difficulty of transport, at least in medicine, because, being sulphur in a nascent state, it doubtless possesses great energy of action.

It is almost impossible to obtain pure salts of alumina with mineral acids for base, because hydrate of alumina being precipitated from alum, free from iron, this alumina necessarily includes a portion of the saline liquid in which the precipitate takes place; little balls are formed, from the interior of which no washings will eliminate the saline liquid. It is otherwise with soluble aluminate of baryta; for, by adding to it the exact amount of sulphuric acid required to precipitate the baryta in the state of sulphate, all the alumina is precipitated at the same time; then, by the addition of an excess either of sulphuric, nitric, hydrochloric, or acetic acid, the sulphate of baryta remains on the filter, while the pure salts of alumina pass through it in the form of a limpid solution, which, when suitably evaporated, yields aluminous salts free from all foreign bodies.—*Comptes-Rendus.*

PHARMACY, TOXICOLOGY, &c.

On the Apparent Difficulty of Detecting Strychnia in the Presence of Morphia—Discovery of a more Powerful Reagent for Strychnia, by JOHN HORSLEY, *F.C.S., Analyst for the County of Gloucester.*

THE first part of the title of this paper has reference to that of Dr. Reese, contained in the CHEMICAL NEWS for June 7. It is very true, as stated by Dr. Reese, that morphia in excess has the power of preventing the effect of chemicals upon strychnia, as ordinarily practised; but the difficulty of detecting it is more apparent than real, all that is necessary being (if the two alkaloids have really been extracted from a dead body) to adopt the method of precipitation of strychnia by the addition to the concentrated aqueous or neutral solution of a few grains of neutral chromate of potash, as suggested by me at the British Association for 1856, aiding the formation of the golden-coloured crystalline precipitate by brisk agitation for a minute or so with a glass rod, then carefully decanting the supernatant liquor containing the morphia (the chromate of which is longer forming); or it may be passed through a very small filter, and the chromic salt of strychnia collected for experiment with strong sulphuric, the merest particle of the salt being sufficient. Under the microscope, the crystals of chromate of strychnia are in the form of little golden-coloured stars; but the corresponding salt of morphia is that of little round granules, with a dark ring on the outside. As a matter of course, these latter, when touched with sulphuric acid, turn green, and so, when in excess, have the effect of masking the reaction of strychnia, which remains passive.

As] regards the new reagent,—that is, the nitroprusside of sodium,—which I used quite by chance the other day, and was astonished at the result, a comparison between it and the bichromate of potash is immensely in favour (where no other alkaloid but strychnia is present) of the nitroprusside, the extreme limit of detection being $\frac{1}{100000}$th; indeed, 75,000 is quite marked, but the bichromate scarcely reaches 3000. There is also a greater degree of intensity as well as persistency of colour, nor is there any disposition to that greenness as observed in the reduction of chromic acid; and, as far as I can see, it is entirely free from objection, and commends itself as *the* test *par excellence*. It is useful as a precipitant of strychnia, the crystals being in the form of very long spiculæ, and sometimes needles, which strike a splendid colour with sulphuric acid.

I arrived at the test figures thus:—1 grain of strychnia was first dissolved in 100 of water. Of this solution, 1 drop was added to 999 of water; a fragment of nitroprusside was then thrown in, and agitated till dissolved. Of this mixture, 1 drop was let fall into a small white-ware dish, and dried at a steam heat, and on cooling the colour was developed by drawing across the spot a glass rod dipped in sulphuric acid.

On the Colour-tests of Strychnia, as Modified by the Presence of Morphia, by ROBERT P. THOMAS, *M.D., Professor of Materia Medica in the Philadelphia College of Pharmacy.*

DURING the last few years the attention of the profession has been attracted to the consideration of the various means which have been recommended for the detection of strychnia in cases of poisoning by that powerful agent; and some of the ablest minds in Europe, and of this country, have contributed the results of their labours to the common stock of knowledge on this important point. Having received a thorough investigation from such hands, and its relations traced in almost every possible combination or association, and the test of its presence—both physiological and chemical—having been proved to be alike clear and distinctive, it would appear probable that little more could be added towards the perfection of its history.

Nevertheless, a question of great moment has recently arisen, as to the possibility of detecting it at all, if the poison should be associated with an equal, or a greater quantity of morphia, or a salt of morphia in the presence of an organic fluid.

The property, referred to morphia when combined with organic matter, of preventing the detection of strychnia by colour-tests, will, if confirmed, afford a satisfactory explanation of the difficulty experienced by Drs. A. S. Taylor, and G. Owen Rees, in their examination of the viscera of J. P. Cook, as elicited on the celebrated trial of William Palmer for his murder, in May, 1856. They could not detect a trace of strychnia, notwithstanding the symptoms antecedent to Cook's decease pointed unequivocally to this alkaloid as the fatal agent. If not decomposed, it must have been masked by the presence of morphia, as Mr. Bamford, the attending physician, administered half a grain of this narcotic each night for three successive nights previous to his decease. The circumstances of this trial, and the experiments, described in the papers referred to, furnish an imperative reason for a further investigation of the subject; as the suicide or murderer can destroy all traces of his work, by simply combining an excess of morphia with a poisonous dose of strychnia. The former will not delay the fatal action of the latter, but, on the contrary, will rather aid it.

With the view of determining the important question, whether strychnia is actually decomposed when treated with test-agents in the presence of morphia, or whether it is merely masked by such presence, I have performed more than a hundred experiments, of which an account of a few of the most valuable and satisfactory is now submitted. Premising that, in the examination of minute

portions of strychnia, success or failure depends entirely upon the care given to the details. In all of my experiments I employed crystals of the pure alkaloids and of their salts.

The "colour-tests" referred to in this paper, are those furnished by bichromate of potassa, or by the ferricyanuret of potassium (red prussiate of potassa), when added to a portion of strychnia previously dissolved in a drop of strong sulphuric acid. The discrepancies in the results of the published experiments of different observers depend, I think, in a great measure, on their diverse modes of procedure, and therefore I feel justified in giving a precise account of—

The Mode of Testing.—In every instance the material to be tested was employed in the solid form, such as the pure alkaloids or their salts. If it existed in solution, it was reduced to a solid consistence by evaporation in a test capsule, spontaneously, or by a very gentle heat. Having thus procured a solid substance, a small portion of it was placed upon a white plate, a drop of strong pure sulphuric acid was added, and trituration was carefully made with a glass rod until the substance was dissolved. Then a small quantity of powdered bichromate of potassa, or of powdered ferricyanuret of potassium, was deposited on the plate, near the acid mixture but not touching it, and to the powder a minute drop of water was added—just enough to partly dissolve it—and then, with a pointed glass rod, a little stream was drawn from each of the solutions in such a direction as to cross each other. Immediately at the point of intersection the play of test-colours was beautifully manifested. When the bichromate was employed, the sequence of colours was blue or violet, instantly changing to purple, then gradually becoming red, and finally greenish-yellow. When the ferricyanuret was used, the colour was a rich bluish-purple, changing rapidly to a light rose-red. It is important to proportion the sulphuric acid to the amount of alkaloid, avoiding an excess beyond what is necessary to its perfect solution; and, therefore, it is better to take the acid out of the bottle by a pointed glass rod, rather than to drop it from the lip.

The first series of experiments was instituted for the purpose of determining how far pure morphia or one of its salts, when combined with strychnia, would prevent the manifestation of the colour-tests.

Experiment 1.—Accordingly, equal weights of the pure alkaloids were rubbed together in a mortar and tested; next, one part of strychnia to three parts of the sulphate of morphia; then one of strychnia to four of the acetate of morphia; then one of strychnia to eight, and, finally, one to twenty parts of the sulphate of morphia. In each case the result was entirely satisfactory; the colour-tests flashing out with more or less distinctness, in proportion to the relative quantity of morphia, as soon as the margins of the two solutions on the plate came in contact. As intimated above, one solution was made by rubbing the powdered bichromate with a drop of water, the other by triturating a portion of the morphia and strychnia with a drop of sulphuric acid, being careful to use just sufficient acid to insure a perfect solution. I did not consider it necessary to carry this experiment any further, because, in a case of poisoning in which the morphia should be twenty times greater than the strychnia, the fatal result and the attendant symptoms would probably be more characteristic of the action of the former than of the latter, and our experiments would be devoted to its detection.

(To be continued.)

INTERNATIONAL EXHIBITION.

Classes III. and IV.

(*Continued from page* 327.)

Substances Used as Food, and Animal and Vegetable Substances used in Manufactures.

If any sceptical oilman entertains a doubt of the possibility of pickling green vegetables and preserving green fruits, still retaining their natural colour, without the use of copper, he will have it immediately dispelled on examining the cases of Messrs. Lazenby (unnumbered), Batty and Co. (754), Messrs. Crosse and Blackwell (769), and several others. All these are guaranteed to contain no trace of copper, and it would seem difficult to surpass them for the naturalness of the appearance they present. This, however, would appear to have been done by some French exhibitors, whose gherkins are not only beautifully green, but retain the peculiar bloom of the fresh vegetable. The English display, nevertheless, is highly creditable, and, besides the cases we have mentioned above, we would call attention to Nos. 753 and 808 as well worthy of commendation.

The articles in Classes II., III., and IV. in the French exhibition are placed in a particularly bad light,—so bad that in some instances it is difficult to see the things in the cases. Those, however, in Class III. which can be seen, look remarkably good, and it is hard to pick out any for especial praise. The truffles, for which our neighbours are famous, appear in great numbers, but do not look inviting. In 327 there are lobsters preserved in their shells, which look very fresh and good.

The tobaccos exhibited by English houses do not call for any especial notice from us, although the article is well represented. The display of teas is smaller than we expected to see, considering how largely it is consumed in this country. Messrs. Dakin and Co. have a case (770) of fine-looking leaves, and some curiosities in the shape of *Physic teas* of several kinds, the properties of which we should like to know something about. Very few specimens of coffee are exhibited; but Case 827 is worthy of notice as containing the berry in various stages of growth. One foreign product, isinglass, is well represented in this department. In Cases 787 and 825 the visitor will see a variety of different kinds of leaves of very fine quality, and also the manufactured article, of a good staple and very clean. Case 830 contains some ornaments of isinglass more curious than pretty, and specimens of various products manufactured for wine merchants and brewers. The exhibition of this article is almost confined to English manufacturers, for we have observed very few specimens in the foreign departments.

There are not many exhibitors of gelatine, and, as the best looking, we should select that shown by Mr. Dufaville, in an unnumbered case. It is called Fish gelatine, and looks remarkably clear and bright. In 814 Nelson and Co. exhibit samples of their manufacture, and in a case mentioned above, 825, there is a respectable-looking article. French gelatine, of very good appearance, is exhibited in No. 771 of that department.

Biscuits and confectionery are largely exhibited, but of these we have had no opportunity of judging, and can only speak of the appearances. Seeing the beautiful look of the sweetmeats, all said to be coloured with harmless materials, we hope we shall never again have to report cases of poisoning by such articles.

In Class IV. there is one article in which English manufacturers hold an indisputable pre-eminence, and that is soap. If proof of this were needed, it is to be found in this exhibition. The case which will perhaps attract most attention is that of Messrs. Williams and Son (953). This firm exhibits all kinds of soap,—the fine curd used by silk dyers for ungumming silk, the coarser sorts used by cloth millers and wool spinners, and the various kinds used for household purposes. The interest of the case, too, is increased by samples illustrative of the manufacture, showing, for example, the quantity of kitchen-stuff, water, and alkali which go to make up a pound of mottled soap, and the tallow, rosin, and alkali for a pound of common yellow. Knight and Sons (924) and Cowan and Sons (917) exhibit fine blocks of various kinds. The last-mentioned firm also exhibit (501) a series illustrating the manufacture of animal charcoal from bones, showing first the extraction of the grease for making soap, and the subsequent steps for the preparation of the charcoal for sugar refining,—a process they carry on in conjunction with soap making. In 916 and 943 are also shown fine specimens of soaps, nor should the display of the West of England Soap Company (950) be passed without notice. Fancy soaps are also well represented in the cases of Mr. Cleaver (1168) and Messrs. Yardley and Statham (1195). If the English visitor wishes to gratify his national pride, he may take a glance at the specimens in this class exhibited in the French department. He will there find several samples of the article whose acquaintance he may possibly have made on the occasion of some visit to Paris,—a hard, chalky, uncomprising substance, which no efforts would induce to make a lather, and which left a smell on his hands and face that the amount of water provided for his use could never rinse away, a smell that is but too plainly perceptible in this Exhibition at some distance from the cases in which the soap is exhibited. Spain shows what we presume is genuine Castile soap, but which is no further remarkable.

Next to soap come candles, and on this article we may also congratulate our countrymen. The case of Price's Patent Candle Company (936) is one of the most elegant in this department, and the articles displayed fully sustain the reputation of the Company. The visitor will notice the specimens of palm oil in different stages of treatment, and the beautiful results obtained by the distillation process. He may perhaps be a little puzzled at Case 911, in which Mr. Bauwens exhibits palm oil distilled by *sub*-heated steam, the exact meaning of which term we should be glad to have explained. Case 926, shown by Messrs. Langton and Co., will be sure to attract notice for the beautiful specimen of spermaceti it contains, and which is alone worth going to the Exhibition to see. The case is further noticeable for samples of spermaceti in different stages of preparation, which will give an unlearned visitor a clear notion of the method used for separating the pure substance and the oil from the head and body matter. The case of Messrs. Ogleby and Co. is hardly less remarkable for the beauty of the spermaceti, here crystallised in a form resembling the young, soft antlers of a stag, or the extremities of the branches of a sumach tree. The specimens of stearic acid in this case are also very beautiful.

Paraffin, too, is well represented. In 918 Messrs. Field and Co. show specimens looking very bright and hard. Several other cases contain samples of great excellence, and we should hope this article would soon be in common use. Wax candles are exhibited by a few houses, in some instances beautifully coloured, and of elegant and fantastic forms; but the use of these candles, we presume, is passing away before the cheaper and equally well-looking articles, with higher illuminating power, which modern science has called forth.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Six Lectures on Some of the Chemical Arts, with Reference to their Progress between the Two Great Exhibitions of 1851 and 1862, by Dr. Lyon Playfair, C.B., F.R.S., Professor of Chemistry in the University of Edinburgh.

Lecture III. (*Thursday, May 22, 1862.*)

Calico Printing—Showing the Important Chemical Manufactures dependent on this Art, and some of its Preliminary Processes.

In my last lecture I brought before you various colours produced by coal-tar which had recently been employed in the art of calico printing. I also at the same time mentioned to you that the discovery of these colours from coal-tar was likely very materially to alter the whole art of calico printing, which, in its present aspect, involves some beautiful applications of science. You will readily understand that, after we have gone over the preliminary processes connected with this art.

"Calico printing" is a generic term. Although the term "calico" is used in it, it embraces all the arts of making dresses with coloured patterns, whether they are manufactured from silk, from woollen, from linen, from cotton, or from any mixture of those materials. It is, therefore, merely a generic term, describing the ordinary dresses which receive designs by means of impression and not by mere dyeing. The name "calico printing" was originally derived from Calicut, in Hindostan, where the art of impressing cotton with colours was carried out to a great extent, although the same art had been employed long before in Egypt as regards linen. The skill of the printer consists in placing as many colours as possible upon the tissue, and deriving them from one dyeing material; for all his supplementary processes are merely confessions of his inability to produce all the colours which he desires from one material. Pliny gives us a description of the manner in which the ancient Egyptians practised the art, and his account is so excellent that I could not possibly give you a better explanation of the art of the present time. I therefore quote it in full. "Robes and white veils," says Pliny, "are painted in Egypt in a wonderful way. They are first imbued, not with dyes, but with dye-absorbing drugs, by which they seem to be unaltered, yet when they are immersed for a little while in a boiling caldron they are found to become painted. Yet, as there is only one colour in the caldron, it is marvellous to see many colours imparted to the robes in consequence of the influence of the excipient drug. Nor can the dye be washed out. A caldron which would merely confuse the colour of cloths previously dyed is thus made to impart several pigments from a single dye stuff, painting as it boils." You will find as I proceed in the description of the art that this latter term of Pliny's, "painting as it boils," is a true description of the art as now practised. Since the time of the ancient Egyptians the practice of calico printing has made enormous strides, and yet it would be impossible in the present day for any one to write a more concise or accurate description of the art even as now practised, than Pliny has done of the condition of the art of the old Egyptians. Although the principles upon which the art was carried out were known in former times, yet its resources have vastly improved.

I may mention as an instance that it is recorded that Alexander the Great wore a pectoral of printed linen,—probably not better, and certainly not nearly so gaudy, as those Turkey red handkerchiefs which we are accustomed to send to Brazil and Mexico to adorn the lowest classes of the population.

The British calico-printing trade is now of great magnitude. It consumes one-seventh of all the cotton which is imported into this country. Perhaps this may not give you an idea of its largeness, and therefore I will put it in another way. There are printed annually, in this country, thirty-one millions of pieces of calico, which is equal to 450,000 miles. This is also a method of indicating the largeness of the subject, which it is difficult to understand; supposing you were to measure the calico printed annually in Great Britain by a gigantic standard measure—take the diameter of the earth as your "yard measure," with which you are to measure the calicos printed in Great Britain in one year,—you would require to stretch it along the diameter of the earth fifty-six times; or, if you were to wrap the whole world round with a bandage of the calico printed annually in this country, you would envelope it in nineteen folds. You will hence see that it is an art of great magnitude, and that it is one which, therefore, justly draws our attention, not only on account of its importance as regards itself, but also (and this, as you will observe by the programme, is mainly the subject of my lecture to-day) on account of the arts which it brings into its train, and the important manufactures which it creates and helps to sustain. To-day's lecture, therefore, is directed to these arts, and to the preliminary processes in the printing of calico.

The preliminary processes are numerous. I will merely enumerate them in their names just now. There are six of them.

There is, first, the bleaching of the calico; second, the application of the mordants, or what Pliny has called the "excipient drugs," which drag the colour out of the bath and fix it upon particular parts of the fabric; third, the resists or discharges, which keep the calico white at particular parts, or produce a white pattern; fourth, the ageing, which will explain itself when we come to it; fifth, the dunging or the fixing of the mordants; and sixth, the dyeing.

I commence with the bleaching. The bleaching of the calico is an exceedingly important part of the process. On the right hand side of that small table you will see the calico as it is delivered to the printer from the weaver. It is of a grey colour, and not at all adapted for the purpose of the printer. It contains the flour and the grease which the weaver uses in his operation. It also contains a natural resin, which prevents the calico from taking on the colour, and it is necessary that all this should be removed, and that the calico should be converted from that dirty grey into a white before the calico printer can use it. Well, up to the last two years of the last century—to 1798,—the bleaching of calico was an extremely formidable operation. It required for its completion from the month of March to the month of September. After the grey calico was boiled in alkaline lyes to take out the grease of the weaver and the resin of the natural calico, it was spread out in vast fields, and was exposed to the action of the sun and moisture, being occasionally watered, first with water, and then with buttermilk; and in this way a true combustion took place. Under the influence of the sun and the moisture the oxygen united with the colouring matter and burnt it; and the colouring matter was thus burnt out of the cloth, and the cloth became white. In this combustion, however, part of the cloth itself was burnt, and the texture was often injured; but the greatest evil was this,—that every calico-printing works, in order to bleach its calico for use, required vast estates in connection with the works upon which to spread out the calico to the action of the sun and air; or in absence of these, the printers sent the calico to Holland to be bleached. The bleaching, therefore, became an extremely expensive part of the operation. Towards the end of the last century it was discovered that bleaching might take place in a much more easy way; but the circumstances that led to this discovery are so interesting, that I would not hesitate, if we had not so much before us, to give you a description of the manner in which this discovery resulted. I will do so in a few words. France, during the revolution, was shut out from receiving a supply of soda, which was used to dissolve out the resin of the calico, from Spain, where it was manufactured under the name of barilla, being derived from the ashes of sea-weed. Our cruisers were vigilant, and we shut out this supply of soda from France. The consequence was that the soap-boilers of Marseilles were nearly ruined, and the manufacturers of calico also suffered extremely. Accordingly, the government offered a high reward to any person who would discover how to make soda from sea-salt. Sea-salt, as you are aware, contains the metal sodium, but united with chlorine and not carbonic acid. It was sagaciously thought that soda might be made out of it; and it was ascertained that, some time before, Leblanc, a private manufacturer, had actually practised a method for converting this common salt into soda. The process is a complex one. I cannot describe it to you further than by stating that the first step is to treat the common salt with sulphuric acid, which drives off the hydrochloric or muriatic acid, and leaves sulphate of soda behind. This is then treated with carbonate of lime, and is converted into carbonate of soda. For a long time the muriatic acid from the common salt went out through tall chimneys into the atmosphere, and devastated the country by destroying vegetation. It is this of which Lord Derby complained in the House of Lords the other night, and which led him to move for a Committee to inquire in what manner manufacturers can be restrained from sending these injurious gases into the atmosphere. Lancashire alone converts 135,000 tons of common salt into carbonate of soda every year. This hydrochloric acid was for a long time lost, but it was soon found to consist of chlorine and hydrogen; and this chlorine possesses strongly bleaching powers. I have here some of this chlorine dissolved in water. If I add to it a colouring matter such as this indigo, you will see that the colour quickly disappears. The colour of the indigo is removed from it.

When it was found out that chlorine had such a power of destroying colour, persons began to think that it might be used as a bleaching agent, instead of exposing the calico for such a number of months to the action of the sun and air. Now, the mode in which this bleaches the colour is by the decomposition of water. Water, as you are aware, consists of hydrogen and oxygen, and the chlorine has a great love for the hydrogen and desires to unite with it; and so it takes away the hydrogen from the water, and forms hydrochloric acid with it; and the oxygen liberated from the water goes and burns away the colouring matter. You know that ordinary combustion in a fire-place is the oxygen uniting with the material of the fire. So here, the oxygen of the water, being set at liberty by the hydrogen uniting with the chlorine, burns the colouring matter and destroys it, just as the oxygen burnt the colouring matter when the calicoes were exposed to the air in the old process of bleaching.

After a time it was found that if this chlorine, which is a gas, was passed over lime it was absorbed by the lime, and formed what we are now so familiar with as bleaching-powder, and that this bleaching-powder bleached just as well as chlorine did, and without having the irritating smell and the irritating action on the lungs which are common to chlorine. As soon as the property of bleaching-powder was discovered, an extraordinary impulse was given to the process of bleaching. After the resinous and fatty matters

have been removed from the grey cloth, it is treated with a weak and clear solution of chloride of lime. In this action the chlorine, desiring to unite with the calcium of the lime, accomplishes this by liberating the oxygen which destroys the colouring matter. From beginning to end of the operation of bleaching only five days are necessary instead of the months which were formerly required. Five days are occupied in converting this grey cloth into white cloth, to make it ready for the calico printer, who is generally his own bleacher. The cloth is first singed by being passed over a red hot cylinder, as you see represented here in the second diagram. This is done tolerably slowly, in order to take away the fibres and clean them from the surface. The texture, however, does not get injured. After being passed over the red hot cylinder it appears in this state,—a little browner. It is now boiled in lime. The lime loosens the resins, and forms a compound of resin and lime on the cloth. This is boiled with dilute sulphuric acid in order to dissolve out the lime and liberate the resin. It is now boiled with a solution of carbonate of soda—the substance is made from common salt, and it renders the grey calico much whiter than it was before. After that it is passed through a solution of chloride of lime. There are one or two boilings which I need not enter into, but the mode in which the boiling is effected is interesting. There is a diagram representing the "bowk," as it is called, or the vessel in which the boiling of the soda and the lime takes place. You see the calico is coming in by machinery, and a boy is represented spreading it along on the floor of the caldron. In this there is placed, after the boy of course has departed, a quantity of water and a quantity of carbonate of soda; and there is an elegant fountain arrangement which cannot be represented there on account of the boy being present, by which the hot fluid is thrown up to the top through these tubes. It is prevented by the arch at the top from getting out, and is thrown in a fountain-shape over the cloth below, so that it is continually percolating through it, and coming down over it; and in that way the resinous matter is completely removed from the cloth. The latter is now passed through a solution of bleaching powder—chloride of lime, consisting of lime and chlorine. The chlorine has a great desire to unite with the calcium, the metal of the lime, and form chloride of calcium, and the oxygen being set at liberty burns away the colouring matter of the calico and bleaches it. There would appear to be no decomposition of the water, as in the case of bleaching with chlorine in the form of a gas. All these operations—boiling in lime, boiling in soda, steeping in the bleaching liquor, and all the operations of washing and drying, occupy only five days from the beginning, and it is carried on in works of small extent. No estates are necessary in connection with the works. The verdure of miles of country is no longer defaced with outstretched calico bleaching in the sun.

Before parting from this subject, let us pause here to consider how this method of bleaching has raised up several important manufactures.

The soda-ash trade is of enormous extent, and is now carried on by extensive manufacturers to supply materials for bleaching and washing soda. Sulphuric acid is necessary in its production, as I told you; so that the price of sulphur regulates the price of calico. The price of sulphur also regulates the price of other materials, such as soda, and of glass, in making which soda is much used. About twenty years ago, as many of us recollect, the King of Naples was persuaded by a Marseilles firm that it would augment the small revenues of his country very much if he gave that firm a monopoly in all the Sicilian sulphur. They immediately raised the price of sulphur from 5*l.* to 14*l.* a ton. Our calico trade, soap trade, glass-works, and various other interests were soon affected by this proceeding, and the consequence was that our government were obliged to take some of the Sicilian sulphur and convert it into gunpowder, with which our fleet went to Naples, threatening to bombard that place unless the monopoly was abolished. The Neapolitan Government did not like the return of their sulphur in this form, and so they permitted the export on the old terms, and our manufactures again flourished. But what was the consequence of this short monopoly. Naples has lost enormously by it. Like all foolish experiments of this kind on trade, it had a most disastrous effect upon those who made it, and it has had a most beneficial effect upon our sister country, Ireland. The skill of our chemists was brought into play to see whether they could not discover sources of sulphur in our own country. We found an abundant source of sulphur in iron pyrites, which exists in such large quantities in Wales and Ireland, and also in Spain and Portugal. The consumption of Sicilian sulphur is thus much reduced.

There are one or two other things in connection with these arts which I must allude to very shortly, as my time is going on.

In the manufacture of this bleaching powder, where chlorine is made, hydrochloric or muriatic acid is boiled with peroxide of manganese, and this produces chlorine. All the manganese was, until recently, wasted after the operation. The chloride of manganese was a waste product, and there were no means discovered for getting back this expensive salt—for producing again the peroxide of manganese after it had been boiled with hydrochloric acid. A beautiful plan has been recently invented, but which will be appreciated chiefly by the chemists present. The protoxide of manganese produced by precipitation from the chloride will not absorb oxygen from the air to form peroxide of manganese, but carbonate of manganese, $MnOCO_2$, when roasted in air, allows its carbonic acid to be displaced by the oxygen of the air, and peroxide of manganese, MnO_2, is thus produced. I here take a little of this carbonate of manganese which is a white or nearly white substance; if I gently roast it, stirring it at the same time, you see that it becomes the ordinary brown oxide of manganese, the carbonic acid having been displaced by the oxygen. In this way all this manganese, or, at least, the greater part of it, is recovered.

There is another interesting process for producing bleaching powder, which has been made within the last few years, and which my chemical friends here will fully appreciate if they are not acquainted with it already, but which I fear I shall scarcely make intelligible to a general audience. It was devised by Mr. Shanks, of St. Helens. First common chrome ore is roasted with lime, and is thus converted in the usual way into chromate of lime,—this yellow salt which I have here. The chromic acid in this compound contains oxygen. The chromate of lime is boiled with hydrochloric acid, the hydrogen of which is taken away by the oxygen of the chromic acid. Chloride of chromium is formed, and part of the chlorine is liberated which combines with the lime to form bleaching powder. The chloride of chromium remains in solution. This chloride of chromium is again treated with lime, and precipitates oxide of chromium with which we started, and chloride of calcium remains in solution. Now, this oxide of chromium is merely mixed with more lime and roasted again in the air. It abstracts oxygen from the air and forms again chromate of lime, another of the substances with which we started; so that by this beautiful process the oxygen is continually taken from the air, forced to unite with the hydrogen of the hydrochloric acid, and the chlorine, entering the lime, forms bleaching powder. The process is, therefore, elegant, but is not yet extensively employed.

I now pass to the subject of mordants. The application and fixation of mordants form the most important parts of the preliminary operations of the calico printer's art. The mordant is the material which is fixed upon the cloth, and which has the power of taking the colouring matter out of the drug. You will find that among these specimens here I have a pattern printed upon this cloth. The pattern is printed with what are called the mordants. These mor-

dants are generally oxides of iron, oxides of tin, or oxides of aluminium or alumina, most of them being oxides which play the part in the operation of weak acids. Now you will understand what the operation of a mordant is, if I give you some elementary experiments upon this cloth. I dip the cloth into a mordant which, in this instance, is a solution of oxide of iron. Now, the object of the mordant is to enter into the material, and by combining with the colour to render it insoluble within the pores of the cloth. I have got the mordant within the pores of the cloth, and if it be immersed in a colour-giving liquid, the mordant will combine with it and render the colour insoluble within the pores of the cloth. I now dip it in this solution, and you see that it immediately becomes of a deep blue. In this case I have used prussiate of potash. That blue colour is rendered insoluble in the cloth, and will bear washing. By means of these mordants it is possible to render insoluble the colour, which would otherwise be soluble and wash out. It penetrates the pores of the cloth, and the action of the mordant being to render it insoluble, the colour remains.

The art of the calico printer, however, consists in trying to produce several colours at one operation. I will represent that also to you in an elementary way. I have here the means by which I may produce two colours by one operation. In this case I have copper in one part, and iron in another. If I add a little acid to this and stir it, and then put the cloth into the solution, I have no doubt that after a little time you will see that one part will come out of a mahogany colour, and the other, as it gets wet, will become blue. You see in this case we are producing two different colours by one operation. We can always manage, by a little chemical artifice, to get different colours out of the same material.

The calico printer's art, however, would not be contented with such a simple and elementary experiment as that. He sometimes desires to produce twenty colours instead of two colours. You must follow me carefully in order to understand how he gets such a great success in his art. I must now stop my explanation of the action of mordants by interpolating it with an important operation which he performs, and which, though simple, is important. I will here try to print a cross on this cloth with cochineal. You see that I produce nothing but an ugly smear. The colour runs aside all over the cloth as it would do upon blotting paper, and I cannot produce the cross that I desire because the colour runs about and produces no definite character. The calico printer has to overcome this difficulty. He must print on his mordant in such a way that it may not run, and so that the pattern may be precise, as you see on these print patterns, and that there may be no running at the edges. Now, he effects this by a very simple operation. He puts a certain quantity of gum into the substance, and the adhesiveness of this gum prevents it from running, so that I can now produce exactly any figure which I desire. I produce here a cross, and this cross will be as precise as I happen to paint it on, because the gum in this case prevents the liquid from running at the edges of the cloth. It is by mixing it with gum, or "thickeners," as it is termed, that a precise pattern is produced. Now this thickening has in itself created quite a set of new works. The making of thickeners for the calico printer has caused a large trade to arise in the manufacture of "British gum," as it is termed. Acetate of iron and acetate of alumina are the two common mordants which are used. These mordants are thickened and mixed in the necessary proportions which I shall presently explain, and are printed in the desired pattern on the cloth. The most elementary way of doing this is by a process which was followed in Hindostan long ago. The pattern was cut out on a block, and then the thickened mordant was taken upon an elastic drum and spread on the block, and the pattern of the block was then printed on the cloth. But as mechanical contrivances became improved, the process of printing by machinery was adopted. We will show you the machine in the next lecture as we are coming more upon that part of the subject then; but here is the means by which it is done. The impression is made upon a roller like these. The rollers are generally much larger than this. They are so made to work, by mechanical contrivances, one into another, that they will produce from five to twenty impressions by acting in harmony with one another so as to form a unity in the design. It is a beautiful contrivance of the mechanist, but it is not a chemical subject upon which we can at all dwell.

Now, before completing the subject of mordants I have still another thing to bring before your attention, and that is the resists and discharges. There would be no beauty in our designs were we not able to preserve the white portions upon them, either to introduce new colours afterwards, or to produce certain desired patterns. For instance, in this yellow pattern you observe these whites which are left quite white, although they have been in the bath. Now, the resists or discharges are merely means of preventing the mordant fixing itself to particular parts of the cloth, where it is not desired to have the colour produced. Certain acids, oxalic acid, lime juice, or citric acid, are placed upon the parts where these oxides are not desired to fix themselves, so that while the oxides run over the whole of the rest of the cloth they meet with these resists where they have been applied, and those parts come out uncoloured. This is a matter which may be readily understood.

One of the most important of these resists or discharges is oxalic acid. This was an expensive substance—so expensive that the consumption throughout the whole world was supposed to be recently only fifteen tons weekly. It is the acid which exists in the common sorrel, and is often called salt of sorrel. It has been made, until within the last year or two, by acting upon starch or sugar with nitric acid. Now, starch and sugar are what some chemists call hydrates of carbon; that is to say, they are compounds of carbon in which hydrogen and oxygen are present in the same proportion as in water. The starch and sugar are themselves expensive materials, and the nitric acid is still more expensive. There is, however, a hydrate of carbon which is very cheap. It is a waste material which we have not known what to do with until about a year ago. This is common sawdust. It is of the same chemical composition as starch or sugar. When sawdust is thrown upon a fire it unites with oxygen, burns, and produces carbonic acid. If this oxidation be stopped half way oxalic acid is produced. Oxalic acid is half way between the oxidation of any of the hydrates of carbon and complete oxidation for the formation of carbonic acid. Now, this sawdust within the last year or two has been made to produce oxalic acid by a beautiful manufacture. A process often employed to produce imperfect oxidation is to heat the organic body with hydrate of potash. In this case the water of the hydrated alkali is decomposed, the oxygen entering into the organic body and decomposing it, while the hydrogen escapes either free or sometimes carrying off some carbon in its flight. Messrs. Roberts and Dale, of Manchester, have followed this plan very successfully. Soda being a substance which is much cheaper than potash, it naturally suggested itself as equally fit for the purpose of producing this partial combustion, but practically it is found that it does not produce oxalic acid by its action on sawdust. Potash which will produce it is too dear to be employed exclusively. It has been found, however, that two equivalents of hydrate of soda, and one equivalent of hydrate of potash, answers admirably. It is curious that soda will not do alone. The sawdust is mixed with these quantities, and allowed to remain in contact for a little time, and it assumes this brown appearance. It is now heated for three or four hours at a temperature of 400° in shallow pans having

cast-iron bottoms; and now it gradually gets into an intermediate product, but only contains about two or three per cent. of oxalic acid. It is now heated still further, and is converted into oxalic acid in combination, of course, with soda and potash, forming the oxalates of those alkalies. Here comes a beautiful process, one which is still mysterious to the chemist. This mixture of oxalate of potash and oxalate of soda is thrown upon a filter, and a solution of carbonate of soda is passed through it, and probably from the oxalate of soda being more insoluble than oxalate of potash, the oxalate of soda remains behind, while carbonate of potash filters through. This oxalate of soda is now mixed with lime and forms oxalate of lime, liberating the soda. The soda is added to the potash which came through in the filtering. Sulphuric acid is now added to the oxalate of lime. Sulphate of lime is thus formed, and oxalic acid remains dissolved in the water, from which it is afterwards crystallised, and you get this beautiful material, oxalic acid. Two pounds of sawdust will yield one pound of oxalic acid. Roberts and Dale, who have perfected this process in Manchester, now make nine tons per week; and the price of it has fallen from fourteen or fifteen pence a pound, to eightpence or ninepence a pound.

Now, I want to show you the application of these acids —oxalic, tartaric, and the various others—to the purpose of discharges. You can understand these readily as resists. They protect the white places and keep them uncoloured; but you may not so readily understand how they act as discharges. A "discharge" is a term applied to an agent which is used for the removal of the colour from any part of the cloth when it has once been dyed. Suppose I want to take away some of this colour from this red cloth, how am I to accomplish it? I remove it by a very pretty artifice. Chloride of lime bleaches on account of the chlorine that it contains. Now, supposing I desire to get a white place upon this cloth, I have merely to print an acid, such as tartaric acid, upon my red material, and then dip it in chloride of lime. The acid used will unite with the lime and liberate the chlorine, and then a spot will be discharged so that the fabric will be bleached at that spot. I think I can show you this in a simple manner. I have here a solution of chloride of lime, and there are little spots printed on the cloth with tartaric acid. I have here also some Prussian blue printed along with the tartaric acid where the colour will be discharged. Now, if I dip this in chloride of lime and leave it, see how the colour is gradually discharged from the place where the acid was put, producing my white pattern, and at the same time bringing out these beautiful blue spots. This is a topical production of chlorine at the particular place and under the precise circumstances in which we desire to have it. Now, many applications of this principle are made. For instance, I have in this case nitrate of lead and bichromate of potash, which will produce a yellow, and in the same way I can bring out the colour, and produce yellow spots as well as white,—white around the yellow. We can by this means produce a large number of different kinds of patterns, by using my discharger topically, and producing the colours exactly as they are required. Thus, these dischargers, or acids, which are a manufacture of themselves, become an extremely important means of producing various patterns in the calico-printing art.

Now, let us return to the mordants, for they are very important. The mordants are printed on the cloth. Now, these mordants are printed by the machine—acetate of alumina for the red, a dilute solution of acetate of iron for the purple, a strong solution of acetate of iron for the black, and a mixture of acetate of iron and acetate of alumina for chocolate. But all these are soluble, and wash off in water. The object now is to render them insoluble, so that the colour may be lodged within the pores of the cloth itself. That is done by what is termed "the process of ageing." This formerly consisted of hanging up the cloth in certain folds in rooms heated to summer temperature, for five or six days, and often for longer, according to the amount of iron which is present in them. During this time, while they are heated up to a summer temperature, the acetic acid escapes from the mordants, and the oxide of iron and oxide of alumina are left upon the cloth. But a large establishment was until recently required for the process of ageing. You required extensive rooms in which to hang up the cloths, and great delay was caused in the dyeing. Within the last year or two an improvement has been introduced, by which the necessity for these extensive rooms is removed. We have only a decade of discoveries to treat of here, between the Exhibitions of 1851 and 1862. In 1856, Mr. Walter Crum, of Glasgow, a man of science, to whom the art of calico printing already owed much of its perfection, discovered a beautiful mode of ageing which is now getting generally adopted. The printed cloths are passed up into a room heated to about 100 degrees, and moistened by steam which comes into the apartment through a trumpet-mouthed opening. By this process the printed material absorbs in fifteen minutes a large quantity of moisture, and it quickly loses acetic acid. It has not, however, become sufficiently aged, and it is therefore now taken from this room and folded up loosely, but simply in single folds. It is put in rooms heated to 80 degrees, and is still kept moist. Here in the course of two or three days it absorbs oxygen and becomes sufficiently aged.

Now comes a curious feature of the operation, which startles one at first as a means of cleansing the cloth. It is necessary, if you will allow me to use the expression, to scour the surface of the cloth in order to make it clean enough for the purpose of the calico printer. You recollect that these mordants have been put on by means of thickeners of gum and various other materials, and it is necessary to remove all these thickeners and other substances by scouring the surface of the cloth, so that the printer can use it. Now, if nothing more than this was required, it would be sufficient to pass this printed material through ordinary hot water, and that would take off the thickeners and leave the cloth in the proper state; but that is not sufficient, and for this reason: all the mordants are not decomposed in the process of ageing. Some of them still remain in a soluble state, and if you simply used a bath of hot water the soluble mordants would attach themselves to the portions of the cloth which are to be left white, and would foul them, so that in dyeing, the cloth would be smeared over by the portions of the mordants thus dissolved. To prevent this the calico printer made use of a curious process, which was to pass the cloths through baths in which the dung of cow-houses was placed. In this way all the soluble mordants were converted by the cow refuse into insoluble substances, which could no longer attach themselves to the cloth and cause a confusion of the colours. In consequence of this operation large dairy establishments were connected with print works, so that the printer might have a sufficient quantity of cow refuse for his purpose. After a time, when the action of this substance began to be properly understood, chemists asked themselves whether some substitute could not be used, so that this objectionable process might be dispensed with. They soon discovered that the peculiar action of this refuse upon the mordants was due to the phosphates which it contained to a considerable extent; and it was then easy to make artificial phosphates,—phosphate of soda and phosphate of lime,—and these were mixed together with glue, and sold for a long time under the name of "dung substitute." Within the last few years chemists have found that even these phosphates are not required, and that it is better to use arseniate of soda—arsenic acid united with soda. You may have some idea of the enormous quantity of this highly poisonous salt that is used, when in Lancashire alone 500 tons of this arseniate

is annually made for the purposes of the calico printer. The use of this substitute has a great advantage. The material is added to the bath, and you may pass several thousand pieces through the same bath, by adding a little additional arseniate of soda. The same bath may thus be used for several thousand pieces without being changed; but in the old plan, where cow dung was used, it was necessary to change the bath after a few pieces had been passed through it, so that the application of these phosphates and arseniates to the purposes of the calico printer has enormously aided him in diminishing the necessity for labour. I am, however, sorry to say that calico printers do not know what to do with the waste of these arseniate baths after they have used them, and they require changing; and, in order to get rid of this arseniate, they have turned it into the nearest stream in the neighbourhood of the works; and then this stream passes in course of time into the reservoirs in which the water is stored for the purpose of being supplied to the inhabitants of the district; so that they are obliged to drink the arseniate which the calico printers have used. Only a few months ago I was sent down to Stockport, in Cheshire, where they suspected that they were drinking arsenic in the water supplied to them. The town is supplied with water from streams which have passed some print works. The mud of the reservoirs was highly charged with arsenic and lead, and the water which the inhabitants drank had arsenic in it, but in such a small quantity that it was not hurtful. This practice of throwing the arseniates into the streams cannot be too highly reprobated. As calico printing is increasing, these poisonous materials are also increasing in their consumption; and however we may wish to allow manufactures to proceed without any legislative interference, it is clearly quite wrong that they should think so slightly of the health and life of the rest of the population, as they appear to do when they pour these poisonous materials into our streams.

I have now, as my hour is drawing nearly to a close, to allude to the last part of our preliminary subject, that is, the process of dyeing; but as we shall deal with that more fully in the next lecture, I will now only allude to it very slightly.

The oxide of iron, the oxide of alumina, and the other mordants which are employed, have a great disposition to unite with colouring matters and to produce insoluble precipitates. There are some of these insoluble precipitates produced here. For instance, this is with the colouring matter of cochineal, and the mordant employed is acetate of alumina. The precipitate consists of cochineal and aluminum. The process has carried down the colouring matter, and produced an insoluble substance which is called a lake. Here is a lake which has been produced by iron.

Now, the most common colouring matter which is employed is the material called madder, of which there are several specimens there. This colouring material, madder, forms, with these mordants, various degrees of colour. I have here an interesting series of the true colouring matters produced from madder, or extricable from madder, by various chemical reagents; for these I am indebted to Dr. Schunk. You will see here a large number of them; but the most important of these substances is this beautiful crystalline material alizarine, the chief principle in the madder. What are the lakes or insoluble substances which the colour produces with these mordants? Alumina produces with the colouring-matter of madder, a red; iron, when weak, produces purple; iron, when strong, produces, with the colouring-matter of madder, black. Now, you may easily conceive that you may have any mixtures of red, purple, and black, according as you take a larger proportion of one out of the other. If you wish to produce a chocolate upon your calico you mix the colouring-matter with alumina, which produces purple, and with iron which produces red, and the red and the purple together produce chocolate. You may also produce any shade of these colours that you wish, according as you make the solutions of the mordants strong or weak. In this way, by the use of these various substances and by printing these mordants in various strengths and admixture upon the cloth, you can produce a large number of colours.

Now, let us see how this takes place in the arts. After you have printed these mordants upon the cloth, aged it, and dunged it, you have got it into the state which is represented there. You now pass it through madder and blow in steam, gradually raising the temperature in about two hours to the boiling point. You must not do it too quickly. When you have put the alumina alone it becomes purple; where you have put a weak solution of iron it becomes red; where there is a strong solution of iron it becomes black; and where you have applied a mixture of alumina and iron it becomes chocolate. Now, this specimen is of an unpromising colour. It is ugly and smeary, and has been dyed too strong intentionally. It is now cleaned by soap, which, you see, cleans it very much, and produces a much better effect. It cleanses the whites, and also cleanses the purples. The effect is thereby much improved. It is then finally passed through a weak solution of chloride of lime, and this bleaches the various portions of colour which may have got attached to the white, and brings it into a state in which it is capable of being sold. You see now that Pliny's explanation was the best which could be given. The mordants are painted in a pattern upon the cloth, and when it is put into a bath, the bath really "paints as it boils." It paints the different colours according to the nature of the mordant which was placed upon it, producing various shades and various colours, according to the chemical nature and the strength of the mordants which are employed.

In the next lecture I propose to bring before you the discoveries which have taken place in this interesting art during the last ten years.

NOTICES OF PATENTS.

1548. *Paper.* T. Routledge, Eynsham Mills, near Oxford Dated June 17, 1861.

This invention consists in the preparation of "half stuff," and paper for Esparto or Spanish grass (spartum ligeum and stipa terracissima), the same being applicable to straw and other fibrous vegetable substances, as stated in the specification of a former patent (No. 274, dated February 2, 1860). The present improvement consists in the employment of milder alkaline leys for the caustic liquors which were then used in the separation of the fibres.

The samples of Esparto fibre and paper made from the same, shown by Mr. Routledge in the International Exhibition, are very favourable indications of the practical value of this process.

1553. *Refrigerating the Fresh Water Produced by Condensing Steam.* A. R. L. De Normandy, Clapham Park, Surrey. Dated June 18, 1861.

This specification refers to certain modifications in the construction of the refrigerator which forms part of Dr. Normandy's "marine aërated fresh water apparatus."

1559. *Heating by Means of Lamps.* W. B. Taylor, Balmes Road, London. Dated June 18, 1861. (Not proceeded with.)

In the construction of lamps burning oil or other highly carburetted fuel, and which are intended to be applied to heating purposes, it is proposed to dispense with the ordinary glass chimney and to substitute a tubular-shaped vessel in which the liquid to be heated is contained, this

serving also as a flue or means of securing the perfect combustion of the oil or smoke-producing fuel.

The effect of surrounding the flame of an ordinary oil-lamp with a receptacle containing cold liquid, would be likely at first to interfere so much with the proper motion of the ascending current of air as to give rise to the production of a smoky flame, and to the deposition of considerable quantities of carbon upon the cold metallic surface next the flame.

Grants of Provisional Protection for Six Months.

1317. Michael Henry, Fleet Street, London, "Improvements in the process of, and apparatus for, preparing materials for the manufacture of paper, and in obtaining products from agents used in the said process, part of the invention being also applicable to apparatus for washing."—A communication from Jean Baptiste Vasseur and Xavier Lamotte, Boulevart St. Martin, Paris.

1371. William Gossage, Widnes, Lancashire, "Certain improved apparatus to be used in the manufacture of soap."

1385. Leo De la Peyrouse, Panton Square, London, "Improvements in treating neutral and acid fatty or oily substances, resins and resinous substances, and compounds or products containing paraffin."—Petitions recorded May 8, 1862.

1400. George Carter Haseler, Vittoria Street, Birmingham, "Improvements in the manufacture of lockets, and of a new application of parkesine as a substitute for glass in the construction of lockets and other articles of jewellery."

1453. Richard Archibald Brooman, Fleet Street, London, "An improved method of, and apparatus for, the production of photographic and stereoscopic portraits and pictures."—A communication from Leon Farrenc, Rue St. Quentin, Paris.

1455. Henry Deacon, Appleton House, Appleton, Lancashire, "Improvements in the manufacture and production of certain colours, and in the apparatus employed therein."

1484. Amand Athanase Lamiable, South Street, Finsbury, London, "Improvements in cementing cast and wrought iron to obtain cast steel."

1496. Christopher Binks, Parliament Street, Westminster, Middlesex, "Improved methods of obtaining oxygen and chlorine gases."

1528. William Petrie, Charlton, Kent, "Improvements in vessels for boiling chemical products, as sulphuric acid, and in apparatus for indicating the degree of concentration and temperature of such products in the boiler, which apparatus is applicable to other pyrometric purposes."—Petition recorded May 20, 1862.

1550. Henry Cook, Manchester, "Improvements in electric batteries."—A communication from Jean Minollto, Turin, Italy.

Notices to Proceed.

3229. Jabez Jones, Liverpool, "Improvements in the manufacture of lead, tin, and other metals, or amalgamation of metals of a like nature fusible at a low temperature into sheets of any thickness or length, and in the apparatus connected therewith."

735. Brereton Todd, Bissoe and Perran Smelting Works, near Falmouth, Cornwall, "Improvements in the manufacture of antimony and oxide of antimony."

1030. Henry Deacon, Appleton House, Appleton, Lancashire, "Improvements in the manufacture of caustic soda."—Petition recorded April 10, 1862.

1244. William Taylor Glidden, Massachusetts, U.S., "A new and useful mode of restoring phosphatic guano."—A communication from Louis Harper Brooklyn, New York, U.S.—Petition recorded April 29, 1862.

228. Rudolph Bodmer and William Wilson, Newport, Monmouthshire, "Improvements in the process of manufacturing artificial stones, parts of which improvements are also applicable to the manufacture of artificial fuel."—Petitions recorded January 28, 1862.

CORRESPONDENCE.

Blue Ammonia Vapours.

To the Editor of the Chemical News.

Sir,—I hope that the controversy going on about Mr. Williams's blue vapours will now be satisfactorily settled to all who, like you, derive their chemical experience from the laboratory, and not from dunghill experiments. I must say, however, that when a boy I have often seen bluish vapours rise from dunghills, immaterial whether close by or far from any chemical works or factories, and how such a vapour would appear to Mr. Williams, with his back to the light when he applies his strong liquor ammonia, must be apparent to all, even if they had never heard of blue ammoniacal vapours or

Blue Moonshine.

London, June 14.

To the Editor of the Chemical News.

Sir,—I am very glad to see that Mr. Williams has accepted your invitation to explain the appearance of his "bluish vapours." It was due to the profession, as well as his own reputation that he should do so; and now perhaps it is made quite clear that what provoked your remarks is simply an error in description. I tried several experiments after reading your report of the trial, and I could in no way get what, by any stretch of the imagination, I could call bluish vapours. Since then I have tried those suggested by Mr. Williams in his letter, with no better success. It is true that when I set a dish of ammonia and of hydrochloric acid side by side in the open air, and watched the vapour form against a bright blue sky, I might, if I had never seen the same thing in the laboratory, have called the vapour bluish.

Mr. Williams's error lies in asserting that bluish vapours from ammonia are proofs of the presence of hydrochloric acid, just as a sky-blue mixture is a proof of the presence of water with milk.

I can imagine a farmer looking blue as he sees a "waif of chemical" sweep like a destroying angel over his crops. He may, indeed, get blue vapours; but under no circumstances can I imagine the vapour from the contact of the two gases in question to look other than white. The vapour may present a different appearance according as it is dense or attenuated; but white it is, and must be.

I am, &c. F. C. S.

June 17.

MISCELLANEOUS.

Note on Thallium, by William Crookes.

Since my preliminary announcements of the discovery of this element, I have purposely avoided publishing the various results of experiments week by week as they were obtained, my object being to prepare, at some future time, a paper on the subject, which might be deemed worthy of a place in the "Transactions" of the Royal Society. Within the last week, however, it has come to my knowledge that a continental chemist has recently had the good fortune to discover a plentiful source of this element, and has prepared the metal and several of its compounds. This has induced me to collect together into one paper all the facts which I have as yet discovered. It will probably be published in an early Number of the Chemical News; and in the mean time, both for the purpose of placing the dates on record, and of showing that though silent I have not been idle during the past year, I publish the following letter from a good friend whose assistance in this matter has been of

more material service to me than a perusal of his letter would lead the reader to imagine :—

"5, New Cavendish Street, London, W.
"June 16, 1862.

"My Dear Sir,—I well remember paying you a visit in January last to see the 'thallium' you had then got into a more definite shape. As I had seen its reaction in the spectroscope many months before, and was much interested in the remarkable discovery, I felt great pleasure in watching the progress you had then made in the investigation.

"You had several compounds of the body, including, if I remember rightly, the sulphide (of which I possess a specimen), and the oxide which you had obtained in crystals. The quantity of material you had, however, was but small; and it was only by exercising the greatest amount of ingenuity that you were able to demonstrate the nature of its compounds.

"The most interesting point, I well remember, was the metal itself, deposited by means of a weak galvanic current,—first on a bar of copper, where it presented a coherent metallic appearance, of dark colour, but which, when freshly scraped with a knife, gave a coloured metal similar to lead. You also had a larger quantity precipitating on platinum in the spongy form, which compressed, gave metallic lustre; and when tried in the spectroscope, gave the green line magnificently.

"Very shortly after I met our friend, Mr. E. O. Brown, of the Chemical Department, Woolwich, who had been testing some of your results, and I was much interested to hear so many particulars as to its chemical character and reactions. Its precipitation as a subsalt or subchloride by water, and its iodide and oxide, pointed out its true metallic character; if, indeed, any doubt could have existed in my mind previously upon the subject.

"I am sorry I was prevented from fulfilling the promise I made you in January, to work the sulphur containing the thallium for you on a large scale, and so get out sufficient for the purposes of a strict chemical investigation. This I have been prevented from doing from unforeseen circumstances, as you are aware; and I must say I am very much astonished that with your means you have been able to make out the large number of facts you already know respecting the element, particularly when we consider that the mineral you had to work with did not probably contain more than two or three grains in the pound. "I remain, yours, very truly,

"W. Crookes, Esq., F.C.S." "John Williams.

The following paragraph on this subject appeared in the *London Review* for the 14th inst. :—

"*Thallium.*—Mr. Crookes, whose discovery eighteen months ago of this new element by the spectroscope we have already announced, has since prepared numerous compounds of it, some samples of which are to be seen in the Chemical department of the International Exhibition. We were shown some time since a specimen in its pure metallic state, obtained by Mr. Crookes, but as no detailed statement of its characters, nor of the nature and actions of its salts, have been as yet published, although a short abstract has been displayed with the specimens since the opening of the Exhibition, it may be interesting to our readers to know what this new element—the only one discovered by an English chemist since Sir Humphrey Davy's detection of the metallic bases of the alkalies—is like. It is a dense, heavy, rather lustreless metal, very like lead, to which metal it is also very similar in its physical properties, but is a trifle heavier, and tarnishes perhaps a little quicker. Its colour, however, is not identical. In chemical properties it is similar to mercury, lead, and bismuth. Mr. Crookes is continuing his researches, and we are glad to state that the Royal Society has voted him a grant of 50*l.* towards the expenses of these costly investigations."

Ozone.—In a letter to Professor Faraday, Schönbein writes :—"After many fruitless attempts at isolating ozone from an ozonide, I have at last succeeded in performing that exploit; and have also found out simple tests for distinguishing with the greatest ease ozone from its antipode, 'antozone.' As to the production of ozone by purely chemical means, the whole secret consists in dissolving pure manganate of potash in pure oil of vitriol, and introducing into the green solution pure peroxide of barium, when ozone, mixed with common oxygen, will make its appearance, as you may easily perceive by your nose and other tests. By means of the ozone so prepared, I have rapidly oxidized silver at the temperature of 20° C., and by inhaling it produced a capital 'catarrh.'"—*Philosophical Magazine.*

Chemical International Banquet.—The English chemists entertained the chemical jurors and other distinguished chemists at present visiting London, at a banquet given at the Trafalgar, Greenwich, on Tuesday evening last. The chair was taken by Dr. Hofmann, President of the Chemical Society, and the company consisted, among others, of Professor Faraday, the Master of the Mint, M. Balard, M. Pelouze, Professor Schrötter, Dr. Percy, Professor Fehling, Mr. Walter Crum, Professor Daubeny, Professor Wurtz, Dr. De Mussy, Professor Regnault, M. Peligot, Professor Williamson, Professor Baumbaner, Professor Frankland, Le Viscount de Villa Maier, Dr. Odling, Dr. Stenhouse, Mr. Grove, Mr. Warren De la Rue, M. Woskrescenski, M. Stas, Professor W. A. Miller, Baron Thénard, Dr. Atkinson, Mr. G. Wilson, Mr. Maskelyne, M. Persoz, Professor Forchammer, Professor Brodie, Mr. D. Campbell, M. Daubrée, Mr. Smee, M. Barreswill, Mr. Attfield, Professor Anderson, Professor Roscoe, Mr. Young, Professor Piria, Dr. Müller, Professor Sobrero, Professor Mennier, Professor F. C. Calvert, Mr. Abel, Dr. Normandy, Professor Lourenço, Professor Chandelon, Professor Redwood, Mr. Morson, Mr. Perkin, Dr. Noad, M. Kuhnheim, Dr. Buckton, Dr. Gilbert, Dr. Lieben, Dr. Price, M. Barral, Mr. Hiesch, Professor Herapath, Dr. Marcet, Dr. Longstaff, Mr. Hanbury, Mr. Field, Dr. Bence Jones, Dr. Bernays, Mr. J. P. Gassiot, Dr. Gladstone, Dr. Letheby, Dr. Lyon Playfair, Mr. Porret, Mr. T. Taylor, Mr. Warrington, Dr. Dexter, Professor Fremy, M. Hulot, &c.

ANSWERS TO CORRESPONDENTS.

R. J. R.—In our next.

A. R.—Copper wire conducts electricity several hundred times better than any saline solution of the same sectional area.

J. W.—Messrs. Johnson and Matthey, of Hatton-garden, will supply you with osmium or any other of the metals of the platinum group.

S.—Dilute hydrochloric acid will effectually remove ink stains from paper. The paper will not be injured if the acid be thoroughly washed out of it immediately after it has done its work.

A. B.—O'Neill's treatise on Dyeing is the best with which we are acquainted.

A. Wilkinson—A porous surface moistened with water containing a very small quantity of mineral acid will probably answer your requirements.

Bronze for Rifle Barrels—Rifleman.—The following is the composition of the acid solution generally used by armoury-serjeants at British stations for the purpose of bronzing the iron barrels of rifled small arms :—

Nitric acid, sp. gr. 1·42. . . .	2 ounces.
Spirits of wine	3 ,,
Sweet spirits of nitre	3 ,,
Tincture of steel	4 ,,
Sulphate of copper	1 ,,
Distilled or rain water	2 quarts.

The surface of the metal being thoroughly cleaned, especially from grease, it is painted over repeatedly with the above mixture, the superfluous oxide being occasionally removed by a scratch brush. This treatment is continued until a permanent coating of the requisite depth of colour is obtained, when the barrels are usually washed in boiling water, rapidly dried, and well rubbed with mutton suet or thick Rangoon oil to prevent subsequent rusting

THE CHEMICAL NEWS.

VOL. V. No. 134.—June 28, 1862.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On some Double Salts containing Chloride of Mercury and Chloride of Ammonium, by JOHN D. HOLMES.

ON treating some chloride of mercury which contained some cyanide (a residue from the preparation of chloride of cyanogen) with hydrochloric acid, dissolving in water and crystallising, after a considerable quantity of chloride of mercury had crystallised out, the solution deposited some tubular crystals, which contained chloride of mercury and chloride of ammonium in somewhat remarkable proportions as the following :—

	I.	II.	III.	Theory.
Chloride of mercury	95·96	95·90	95·78	95·86
Chloride of ammonium	3·99	4·06	4·10	4·14
	99·95	99·96	99·88	100·00

The crystals are anhydrous and yield results closely corresponding with the formula $9HgCl,NH_4Cl$. They may be formed by dissolving 25 parts of chloride of mercury and one part of chloride of ammonium in hot water, with the aid of a little hydrochloric acid, and crystallising. No. III. is an analysis of the salt so prepared. It crystallises in forms of the oblique system, has a specific gravity of 3·06, dissolves readily in water, but is decomposed, the solution depositing crystals of chloride of mercury, unless a little hydrochloric acid be added. A solution of this salt gives a yellow precipitate with potash, but does not evolve ammonia, even when boiled; with bichloride of platinum, when added in considerable excess, it gives a yellow precipitate.

If an excess of hydrochloric acid be added to a solution of the above salt, it deposits on evaporation long acicular crystals, containing,—

		Theory.
Chloride of mercury .	82·08	81·95
Chloride of ammonium .	10·88	10·79
Water	7·01	7·26
	99·97	100·00

which corresponds with the formula $3HgCl,NH_4Cl4HO$. They are permanent in the air, and very soluble in water. If three equivalents of chloride of mercury and one equivalent of chloride of ammonium be dissolved in water, and the solution evaporated, on cooling it becomes almost solid, from the formation of a mass of very small acicular crystals, which dissolve again on the slightest elevation of temperature. Some of the crystals dried at 100° C. gave :—

			Theory, $3HgCl,NH_4Cl$.
Chloride of mercury .	88·28	88·59	88·36
Chloride of ammonium .	11.7	—	11·64
	99·98		100·00

But the water they contain appears to be variable, the loss at 100° C. being on different occasions 2·53, 4·67, 4·88, and 5·02 per cent.

TECHNICAL CHEMISTRY.

On the Preparation of Pure Caustic Soda on the Large Scale, by Dr. PH. PAULI, *of the Union Alkali Works, St. Helens, Lancashire.*

THREE tons of commercial caustic soda, containing excess of water, alumina, and all the impurities which commonly occur in this substance, are fused in a cast-iron pot. During the evaporation nearly all the carbonate, and by far the larger quantity of the other salts, separate out on the surface as a scum, and can be easily removed. The liquid mass is then heated to dull redness, and kept at that temperature during the night. In the morning the mass appears perfectly transparent, the sides and bottom of the vessel being coated with cauliflower-shaped masses of crystals consisting of silicate of alumina, with chloride and sulphate of sodium and a little lime. The clear fused liquid is ladled off from these crystals, and when cooled is ready for use.

The soda thus obtained is perfectly free from alumina; a small quantity of the soda was fused in a platinum crucible, and some pure alumina added. This remained undissolved in the fused mass, swimming about like a precipitate in the red-hot liquid. On cooling, water was added to this fused mass, and the alumina was found to dissolve completely. If the commercial soda contains oxide of iron, this also separates out completely during the process of fusion. Lime, on the other hand, is dissolved in large quantities by the caustic soda, but it is completely separated by solution in water. The caustic soda prepared in this way is hard and brittle, and can be easily obtained as a fine powder by attrition in an iron mortar. It contains only a trace of carbonate of sodium. This product, which will doubtless prove a valuable reagent in chemical laboratories, is now prepared on a large scale by Messrs. Evans and McBryde, Union Alkali Works, St. Helens, Lancashire.

Aluminium.—Owing to the length of Mr. M'Gauley's important article on Aluminium, commenced last week, we were compelled to defer the conclusion to a subsequent Number; and so much space being this week occupied by our Index, we are again obliged to omit several articles which we have in type. We therefore feel that it is only right to state at once, what we intended mentioning at the conclusion—that the above article was extracted from the *Intellectual Observer*, one of the best as well as most popular of the monthly magazines. We shall take an early opportunity of giving a more extended notice of this serial; and in the mean time can refer our readers to the extract already given as a sample of the sterling character of the intellectual food provided for its readers by the publishers, Messrs. Groombridge and Co.

PHARMACY, TOXICOLOGY, &c.

On the Colour-tests of Strychnia, as Modified by the Presence of Morphia, by Robert P. Thomas, *M.D., Professor of Materia Medica in the Philadelphia College of Pharmacy.*

(*Continued from page* 342.)

Experiment 2 was an exact duplicate of the first in all of the varied strengths of the two articles, with the simple difference of having them in solution in water acidulated with acetic acid, instead of being in the solid form.

To obviate the necessity of a subsequent concentration of the solutions by heat, the salts or alkaloids were macerated in small measures only, of acidulated water, and to each solution an excess of a solution of potassa was added, and then an equal measure of chloroform, which dissolved out the strychnia, and subsequently yielded it as a filmy deposit in a capsule, by spontaneous evaporation.

With sulphuric acid and bichromate of potassa, the proper test-colour was manifested by the deposit in each case.

In my opinion, these experiments determine conclusively that strychnia can be detected by colour-tests, even when masked by morphia—either as the alkaloid or one of the salts—to the extent of twenty times its own weight.

But the question raised is not so much whether strychnia will be masked by pure morphia or its salts, uncontaminated with anything else, as whether morphia in the presence of organic mixtures has the power of preventing the recognition of strychnia by the usual colour-tests. To determine this latter point, my attention was directed, in the next instance, to the devisal of a simple and practical method, by which its recognition could be secured. Upon reflection, I concluded that the most feasible plan of overcoming the difficulty would be to separate the poison entirely from the organic mass, by chemical agents and solvents.

After careful investigation and repeated trials, I selected as the agents the three fluids used in experiment 2. These fluids can always be obtained at trifling cost, of easily ascertained strength, and of known purity. They are,—1. Acetic acid, of the specific gravity 1·041. 2. A solution of one drachm of caustic potassa in a fluid ounce of water. And, 3. Chloroform.

Acetic acid was chosen, because, when in excess, it has the property of dissolving all of the ordinary salts, both of morphia and strychnia, as well as their tannates, which are generally described as being insoluble; and, therefore, by treating an organic mass containing these alkaloids with this acid, we would obtain a solution of the acetates of morphia and strychnia.

The solution of caustic potassa was selected for several reasons. For instance, in neutralising the acetic acid, it forms a soluble salt of potassa, thereby getting rid of the acid when we are done with it. It saponifies the fats of the organic materials; it decomposes their sugars; and it dissolves morphia, but does not dissolve strychnia, thus enabling us to separate the one alkaloid from the other by its agency.

Chloroform was resorted to for its solvent and volatile properties. Thus 100 parts of it, according to M. Pettenkoffer, at ordinary temperatures dissolve 20·16 parts of strychnia and only 0·57 parts of morphia. A fluid-drachm of it, holding the strychnia in solution, will evaporate spontaneously in a few minutes if placed upon a saucer or plate.

As the solution of potassa dissolves morphia and rejects strychnia, while chloroform has the reverse property of taking up the strychnia and rejecting the morphia, it must be evident that the conjoint use of these fluids would effect an entire separation of the two alkaloids—the morphia being held by the potassa and the strychnia by the chloroform.

Another important practical advantage in the use of these fluids is found in their different specific gravities. The chloroform, being the heavier, sinks to the bottom of the vessel containing them, and thus a separation is easily accomplished.

The eliminating properties of the three fluids were determined in the following way:—

Experiment 3.—One grain of strychnia and three grains of opium were macerated for three days in a mixture of equal measures of acetic acid and water; then filtered, and to the clear liquid equal bulks of the caustic solution and chloroform were added, and the whole well shaken together. Upon subsidence the chloroform was separated, and a part of it evaporated on a plate. The deposit thus obtained was treated with sulphuric acid and the bichromate of potassa, in the manner before described, when a fine play of test colours resulted.

In this and the subsequent experiments care was always observed to have the caustic solution in sufficient excess over the acetic acid to dissolve the morphia, and leave the solution alkaline to test paper.

Having thus determined the practicability of regaining the strychnia in a separate state from a solution in which it had been associated with the various alkaloids of opium, I proceeded, in the next instance, to the examination of its relations to morphia in the presence of organic matter. For this purpose a nutritive mass was prepared as the representative of the contents of a man's stomach, if death should occur soon after a meal. The mass consisted of two ounces of minced meat,—such as is used for pies, and containing meat, suet, various dried fruits, cider, spices, and a little brandy,—two ounces of bread, a portion of salt, pepper, and vinegar, and two fluid-ounces of a strong infusion of coffee, well sweetened. In other words, an association of the nitrogenous and non-nitrogenous elements of food, with spices, alcohol, tannic acid, and caffein. To this mass one grain of strychnia and five grains of morphia, which had been previously well triturated together and dissolved in a mixture consisting of one fluid-drachm of vinegar and fifteen fluid-drachms of water, were now added, and the whole carefully mixed. The mixture was set aside for twenty-four hours to permit the alkaloids to permeate the entire mass, and it was then divided into four equal parts, each part being made the subject of an experiment, as follows:—

Experiment 4.—The first portion, containing one quarter of a grain of strychnia and one and a-quarter grains of morphia, was treated with f℥ij. of acetic acid (1·041), and f℥iv. of water. The mixture having been allowed to stand for sixty hours, was raised to the boiling heat, and ebullition was maintained fifteen minutes. Having been strained and filtered, equal quantities of chloroform and the caustic solution were added to the filtered liquid. The whole were well agitated together, and after subsidence the chloroform was separated and evaporated. Upon treating the deposit with sulphuric acid and ferricyanuret of potassium, the distinctive play of colours appeared.

This experiment proves that strychnia is not decom-

posed by a heat of 212°, maintained for a short period, even though morphia and organic matter be present. The elevated temperature is objectionable, however, from the large amount of starchy matter dissolved by the boiling water, which clogs the subsequent steps of the process.

Experiment 5.—The second portion, containing the same amount of alkaloids as the preceding, was macerated twelve hours in f℥ij. of acetic acid and f℥ij. of water, then strained with pressure, and filtered. The resulting liquid was treated with equal measures of chloroform and the caustic solution. The deposit from the evaporated chloroform yielded the proper colours with the test agents.

This, like many subsequent experiments, proves that it is unnecessary to employ heat; acetic acid, mixed with an equal measure of cold water, being amply sufficient for the extraction of alkaloids from an organic mass.

Experiment 6.—The third portion was macerated, like the preceding, for twelve hours, in acetic acid diluted, and then pressed and filtered. The resulting liquid was placed in a capsule before the register of an ordinary house furnace, and was evaporated to a syrupy consistence by the warm air passing over its surface. To this the chloroform and the solution of caustic potassa, in equal measures, were applied, and all well mixed. The separated chloroform yielded a deposit which was proved to be strychnia by the test agents.

By this experiment we learn that a solution containing both alkaloids in contact with organic matters may be evaporated by a moderate heat almost to a solid consistence without their decomposition.

Experiment 7.—The fourth portion was set aside for twelve days in a room having a temperature of 68° to 70°. At the expiration of this period the mass had become sour and offensive, and was spotted with crusts of mould. It was then macerated for twelve hours in a mixture of f℥ss. of acetic acid and f℥jss. of water; after which it was strained, filtered, and evaporated like the preceding to the consistence of syrup, by the passage of a current of warm air over the surface. As the resultant liquid was intensely sour as well as bitter, I added two measures of the caustic solution and one of chloroform, and agitated the mixture as in former cases. When the chloroform was separated, and a part of it evaporated in a capsule, the most beautiful and distinctive colours appeared after the employment of the proper tests.

This experiment was instituted for the purpose of ascertaining whether strychnia is liable to undergo any change in the presence of organic matter by the lapse of time, even where that matter has fermented and exhibited evidences of incipient decomposition. The result affords a strong inference that it could be detected in the contents of the stomach after a body had been deposited several days in the grave.

I carried this experiment a step further with a view of determining whether morphia could also be regained from an organic mixture in a state of commencing decomposition. Consequently, after separating the chloroform and caustic solutions from each other, I added acetic acid to the latter, drop by drop, until it was neutralised. Then a solution of tannic acid was added, and the precipitated tannate was collected and dissolved in acetic acid. The solution afforded a red colour with nitric acid, and a rich blue colour with the persulphate of iron, and was, moreover, very bitter to the taste, thus establishing conclusively the presence of morphia.

Having entirely satisfied my own mind by the foregoing experiments that no inherent difficulties exist in the detection of strychnia by the colour-tests when it is associated with morphia and involved in a great mass of animal and vegetable matters, I might with propriety dismiss the subject; but as so large a quantity as a grain of the poison might not be found in the stomach after death, in consequence of the use of emetic remedies, or the partial absorption of what had been swallowed, I concluded to try the process on a much smaller scale, and accordingly performed as follows:—

Experiment 8.—Two ounces of minced meat (similar to that used in experiments 4, 5, 6, and 7), and two ounces of bread, were macerated eight hours in four fluid-ounces of water, holding in solution $\frac{1}{100}$th of a grain of strychnia and $\frac{1}{33}$rd of a grain of the sulphate of morphia.*

Half a fluid-ounce of acetic acid, with an equal measure of water, was added, and the mixture set aside for six hours. It was then strained with pressure and filtered. The remaining clear liquid was divided into two portions, *a* and *b*.

One portion (*a*) was placed in a capsule in a current of air at 70°, and thus evaporated to a syrupy consistence. It was treated with the caustic solution and chloroform, as in the former cases; and, upon careful application of the tests to the film obtained by evaporating the chloroform, clear and undoubted evidences of the presence of the poison were afforded by a transient yet distinct play of colours.

The other portion (*b*) was treated directly with a measure of chloroform and two measures of the caustic solution. After mixing these carefully together, the chloroform was drawn off, placed in a capsule, and evaporated spontaneously. A very delicate filmy deposit, of a white colour and bitter taste, was observed in the capsule. A part of this gave, with sulphuric acid and ferricyanuret of potassium, a distinct but evanescent change of colours, indicating positively the presence of strychnia.

To determine more satisfactorily the precise nature of the deposit obtained from this experiment, I evaporated upon a slip of glass drop after drop of the chloroformic solution, until an appreciable deposit was procured, and to this a drop of dilute nitric acid was added. When dry it was placed under a microscope at a magnifying power of 100 diameters, and a crop of well-defined crystals of the nitrates of strychnia and brucia was apparent.

This experiment proves conclusively that strychnia in minute quantities can be regained from organic mixtures, notwithstanding the presence of three times its weight of sulphate of morphia.

In conclusion, I may be permitted to offer to those who shall be called upon to determine the presence or absence of strychnia in cases of suspected poisoning, a few practical suggestions, viz:—

1. In testing for minute portions of the poison, success or failure depends entirely upon the care given to the details of the process.

2. In examining the contents of a stomach, the employment of heat is not required for the detection of strychnia, nor should any unnecessary fluid be added which might require subsequent evaporation. Equal measures of acetic acid and cold water, in sufficient amount to thoroughly acidulate the mass, are all that is

* These minute quantities were obtained with great accuracy by dissolving one grain of the former, and three grains of the latter, in fifty fluid-drachms of water, acidulated by a few drops of acetic acid. Half a fluid-drachm of this solution contained gr. 1-100th of strychnia and gr. 1-33rd of the sulphate of morphia.

requisite to extract any alkaloids or their salts that may be present, and to ensure success these must be reduced to the solid state.

4. In the application of reagents for the production of the colour-tests, care should be observed not to add any more sulphuric acid to the strychnia than what is necessary to dissolve it; and, in like manner, the powdered bichromate of potassa, or ferricyanuret of potassium, should only be moistened, as by touching a glass rod with the point of the tongue and then rubbing it over the powder. In this manner, two saturated solutions or mixtures are obtained, which show the play of colours as soon as their margins are brought into contact, even though the quantity of alkaloid present be very minute. —*Amer. Journ. Med. Science.*

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, June 19, 1862.

Mr. W. Crum *in the Chair.*

Dr. Marcet gave a lecture "*On the Chemistry of Digestion.*" Until very recently but little attention had been bestowed by chemists on those changes which go on under the influence of organic life, and, in consequence, many vague speculations had been entertained and published concerning this most interesting department of science; of late years, however, many able investigators had taken the subject in hand, and much progress had already been made. Many obstacles attended these inquiries on account of the difficulty of observing the conditions of the immediate principles during life; the term "immediate principles" being applied to those substances produced by organic life from which no less complex body could be obtained without a complete destruction of the substance in question. As an example of the power possessed by organic substances of preventing ordinary chemical reactions, the influence of albumen or the serum of blood on lactate of iron was shown. A mixture of this salt with white of egg gave no colour with ferrocyanide of potassium, although the lactate itself furnished the ordinary blue precipitate. With respect more especially to the chemistry of digestion, it appeared that after a long fast the contents of the stomach were alkaline, and very small in quantity; as soon, however, as food was introduced, the gastric juice was secreted in quantity, and an acid reaction was perceptible. The object of the action of the gastric juice was, no doubt, to render the food capable of absorption; and accordingly it was found that albuminous, gelatinous, and other similar matters introduced into the stomach, became converted into a substance called "peptone," which, according to Lehmann, might be viewed as the same body, whatever nitrogenous food was employed; it had been shown, however, that the peptones resulting from the digestion of cartilage and the mucous membranes rotated the plane of polarisation of light, whereas peptones from albumen had not this power. The gastric juice, which was at first abundant, gradually diminished in quantity and became more acid, probably in order that it might act on the less masticated or less easily digestible portions of the food. Besides the conversion of the albuminous matter into peptone, another important change took place in the stomach, namely, the decomposition of the neutral fats and setting free of the fatty acids; this was an important decomposition, for the bile would form an emulsion with a fatty acid, but not with a neutral fat; some of the fat sometimes escaped decomposition, but the pancreatic secretion formed an emulsion with this portion. The formation of an emulsion seemed to depend on the incrustation of each globule with a layer of soap, which prevented the globules from coalescing, and increased their specific gravity, so that they remained for a long time suspended in the liquid. Dr. Marcet considered that in experiments on digestion it was always better to employ gastric juice obtained directly from the stomach of an animal, instead of an artificial compound, such as was employed by some physiologists. There was some dispute as to the nature of the free acid existing in the gastric juice,—some supposed it consisted of hydrochloric acid, while others imagined that other free acids, especially lactic acids, were present; since quantitative determinations of the amount of hydrochloric acid and of the bases present in the gastric juice showed that there was more hydrochloric acid than was sufficient to combine with all the base, it was evident that there must be some free hydrochloric acid present; it was highly probable, however, that other acids were present in a free state, for on placing some gastric juice in a dialyser and leaving it until all the hydrochloric acid had passed away, the remaining matter was found to be still acid. It had been supposed that the soda introduced in the shape of common salt with the hydrochloric acid of the gastric juice, was employed in the formation of bile; but it appeared from the interesting researches of Dr. Bence Jones that this was not exactly the case, for healthy blood was always alkaline, but appeared to have an incessant tendency to become acid; the acid was, however, as rapidly removed by the secreting organs; and it had been found that when the secretion of gastric juice was active the urine became less acid, and it gradually increased in acidity as the gastric secretion was moderated, so that the two actions balanced one another. It appeared that if no salt were supplied with the food eaten, the hydrochloric acid secreted was totally absorbed again with the food, furnishing an example of that wonderful power of adaptation to circumstances which enabled animal life to continue under varying external conditions. The only materials of the food that passed through the stomach and intestines undigested were such substances as hair, horns, &c.; together with these, however, a small quantity of excrementitious matter, obtained from the various secretions poured into the intestines, was always present, and a crystalline matter of definite chemical composition, and bearing some analogy to cholesterine, might be extracted from it.

Dr. Baly remarked that he did not altogether agree with Dr. Marcet as to the advantage of employing gastric juice obtained directly from the stomach in physiological experiments; he had found that an extract of the membrane of the stomach, when mixed with hydrochloric acid, acted very energetically; the substance ordinarily sold as pepsine was worthless in most instances, consisting principally of starch or other matters of a like nature.

Mr. Newlands observed that with respect to the non-detection of iron when mixed with albumen, he would suggest that it was possible that the lactic acid was the substance really affected, and that the oxide of iron being left in an uncombined state would be incapable of producing Prussian blue with ferrocyanide of potassium until an acid was added to neutralise the potash set free.

Mr. Porrett drew attention to the fact that a solution of protoxide of iron did not afford a blue precipitate with ferrocyanide, but on exposure to air the white precipitate formed under these circumstances soon changed to blue.

Dr. Gladstone remarked that the separation effected by dialysis would probably prove an important auxiliary in chemico-physiological experiments; it might be considered to have set at rest the question as to the presence of more than one free acid in gastric juice.

The next paper was by Mr. Perrins, "*On Berberine.*" Different sources of this organic base were mentioned, and an analysis of many salts had been performed, which had led to an alteration in the formula expressing the composition of this body; the old formula being

$C_{42}H_{18}NO_9$, and the new one $C_{40}H_{17}NO_8$. Among the salts were mentioned the double hyposulphite of berberine and silver, $C_{40}H_{17}NO_8,S_2O_2+AgO,S_2O_2$, the bichromate $C_{40}H_{17}NO_8,HO,2C_2O_3$. The hydriodate of biniodo-berberine, $C_{40}H_{17}NO_8I_2,HI$, formed by adding a solution of iodine in excess to a salt of the base, and a second iodide having the same composition as the former, but of a beautiful green colour, formed by adding a deficiency of iodine to a solution of a berberine salt. Berberine appeared to be a base very widely diffused in nature, and worthy of more notice than was usually bestowed upon it in Manuals of Chemistry.

Extracts from a paper by Mr. Greville Williams were also read. Availing himself of a reaction pointed out by Berthelot, to the effect that when olefiant gas and hydriodic acid were brought together, iodide of ethyl was formed, Mr. Williams had prepared from Boghead naphtha a series of iodides homologous with iodide of ethyl, and this method of formation might eventually prove to be of some commercial importance.

NOTICES OF PATENTS.

1580. *Compounds of India-rubber and Gutta-percha with other Substances.* J. F. Williams, Queen Square, London. Dated June 19, 1861.

This invention consists in combining with india-rubber or gutta-percha the black residuum obtained in the distillation of palm and other vegetable oils by superheated steam. These substances are intimately mixed by being masticated, and, when it is desired afterwards to vulcanise the india-rubber compound, a certain quantity of sulphur must be added and thoroughly combined with the other materials. It is recommended, in most cases, to introduce a small proportion of chalk into these compounds, and, when the nature of the application permits, some fibrous matter also to give strength to the mass.

1582. *Preserving Wood and Iron.* J. Cullen, North London Railway Works, Bow. Dated June 19, 1861.

A composition is prepared from coal-tar, quick-lime, and charcoal, the two latter ingredients being reduced to the state of fine powder and intimately incorporated with hot coal-tar. The wood to be preserved is dipped into the hot composition; iron may be painted with the same.

1592. *Improvements in the Manufacture of Fuel from Peat, and in Apparatus employed therein.* C. Hodgson, Ballard, Rathdrum, Wicklow. Dated June 20, 1861.

1593. *An Improved Method of Partially Drying Peat before removing the same from the Bog.* C. Hodgson, Ballard, Rathdrum, Wicklow. Dated June 20, 1861.

These specifications describe a mode of treatment adopted by the inventor for the preparation of compressed blocks of peat to be used as fuel. The top surface of the bog having been cleared from roots and the unconverted vegetable structures of recent origin, trenches are dug at intervals for the purpose of draining the peat. As soon as the surplus water has been thus got rid of, the upper layer is harrowed to the depth of three or four inches, and in this condition left exposed to the wind and air until, in the course of a day or two, the loosened turf has become sufficiently dry. This matter is then collected by raking, and submitted to great pressure without the application of heat, being retained in the press until it has been transformed into permanent blocks of the requisite degree of compactness, and dry enough to be used as fuel.

1650. *Manufacture of Coal Gas by the Surplus or Waste Heat of the Puddling and Blast Furnaces used in the Manufacture of Iron.* T. Swinterton, Dudley, Staffordshire. Dated June 28, 1861.

The patentee claims the adaptation of gas retorts to the before-mentioned iron furnaces for the purpose of economising the waste heat; using it in the production of coal gas in preference to rendering it available as a source of steam power, or for heating the air-blast.

1666. *Distillation of Solid and Liquid Combustible Matters.* W. Clark, Chancery Lane, London. A Communication. Dated June 29, 1861.

This invention consists in injecting into the midst of the combustible materials undergoing distillation a blast of heated gas not itself combustible. The gases best adapted for the purpose are nitrogen, carbonic acid, or a mixture of these procured by passing a current of air over red-hot charcoal. Under these circumstances the materials operated upon are said to be brought more immediately under the influence of heat, and to afford products differing from those obtained in forms of apparatus in which the heat is applied externally. The inventor further recommends the application of this system to the rectification of crude liquid and solid products, obtained as the first result of distillation, and particularly when it is desired to convert these into permanent gas. This method is stated to furnish excellent results when adopted in the case of the destructive distillation of peat, and carried out in the same manner as when coal gas is made in retorts through which a current of uninflammable gas is passed during the time heat is being applied.

CORRESPONDENCE.

On the Nitro-prusside of Sodium as a Test for certain Alkaloids.

To the Editor of the Chemical News.

Sir,—Since announcing the above test I find by using it in the manner stated, that the plea for the non-detection of strychnia in the presence of morphia no longer holds good, as subsequent experiments with five and even ten times the amount of morphia prove.

I find also that one drop of a solution of strychnia of one per cent. strength, agitated with one or two drops of a saturated solution of the nitro-prusside, produces an abundant crop of crystals for an infinitude of experiments with sulphuric acid,—the crystals under the microscope being in long nitre shaped tufts and needles. A similar experiment with brucia produces larger and broader needles having lancet points, totally different from strychnia, besides being different in its reaction with sulphuric acid.

A similar experiment with morphia also shows certain characteristics; thus, the crystals are for the most part of a peculiar star-fish shape for the larger compound crystals, which appear to be made up of plates or layers of single squares.

These crystals when collected on a filter and dried, produce the usual orange-red coloured reaction with nitric acid, but unlike pure morphia, when touched with sulphuric acid, assume a deep sepia brown with a purplish shade, which is more or less persistent,—totally different from the reaction on any other alkaloid.

Possibly, by applying this test to some other alkaloids, we may obtain some useful characteristics by which they can be recognised better than by some of our present methods.—I am, &c.

John Horsley.

Cheltenham.

INDEX.

Abel, F. A. Esq. F.R.S. lecture on some of the causes, effects, and military applications of explosions, 270
on a deposit occurring in teak wood, 95
Acetate of cyanogen, 210
Acetone, 122
Acetylene, synthesis of, by the direct combination of carbon with hydrogen, 184
Acid, arsenious, capacity of, for bases, 320
benzoic, new derivative of, 55
benzulmic, 126
carbolic, 182
some applications of, 19
carbonic, apparatus for estimation of, 142
formic, preparation of, by means of carbonic acid, 227
hydrochloric, preparation of pure, 41
leucic, and its salts, 42
nitric, on the action of, on picramic acid, 18
nitric and hydrochloric, solubility of sulphate of lead in, 52
nitric, new method of detecting small quantities of, 111
volumetric estimation of, 104
perchromic, 129
phosphoric, on the conversion of monohydrated into common, 61
estimation of, in presence of oxide of iron and earthy bases, 281
in primitive rocks, 210
picramic, on the preparation of, 60
picric, contributions to the history of, 5
prussic, 125
rhodizonic, 75
sulphindigotic, products of oxidation of, 252
sulphuric, arsenic in, 95
reduction of, by nascent hydrogen, 337
tartaric, action of chloracetyle on, 195
titanic in clay, 320
citric, butyeric, and valeric, 122
Acids, uric and hippuric, variation in urine, 51
Acidimetric method, generalisation of the, 145
Address of the chairman of the Great Central Gas Consumers' Company to the gas consumers of the city of London, 224
Adie, Mr. on ground ice, 94
Adulteration of bees'-wax, 84
of butter with animal fats, 43, 99
of food, 70
of articles of food and drink, law against, as affected by the 23 and 24 Vict. cap. 84, 306
of milk, 278
of tin foil, 59
Air and ammonia, action of, on copper, 113
Air, filtration of, and the influence which it exerts on fermentation, &c. 173
Alcock, Dr. Thomas, on the tongues of mollusca, 177, 192
Alkalies, absorption of carbonic oxide by, 144
quantitative determination of, in insoluble silicates, 121
Alkaline hydrates and carbonates, new method of estimating, and other compounds of this class, 311
matters, presence of rubidium in certain natural and industrial, 295
Alkaloids, poisonous, 229
a process for the extraction and investigation of poisonous, 229
nitroprusside of sodium as a test for certain, 355
Alloys in fusion, action of voltaic pile on, 326
of lead and tin, 121
of tungsten, 116
Aluminate of baryta and pure alumina salts for industrial purposes, 340
of soda and its uses, 296
Aluminium, 338
Aluminous minerals, or bauxite, analyses of, 102
Amalgams, experiments on some, 53
recent researches on metallic, and on the origin of their chemical properties, 259
Amber and copal, means of distinguishing by oil of cajuput, 184
American chrome iron ore, 56
Ammonia, action of bicarbonate of, upon the salts of magnesia, 235
and air, action of, on copper, 41, 113
bicarbonate of, 325
in contact with oxides, action of oxygen on, 89
Ammonium, chloride of, bluish vapours, 335, 349
compound of iron and, 154
chloride of, with chloride of mercury, double salts, 351
Amylaceous matter in fruits, 98
Anæsthesia, producing local, 56
Analysis of cast iron, 170
spectrum, 40, 201, 214, 251
Anderson, Prof. A. remarks upon the method of obtaining nitrogen by the decomposition of solution of ammonia by chlorine, 246
Dr. T. on the chemistry of opium, 265
Dr. growth of cinchona in India, 317
Aniline, application of voltaic electricity to the detection of, 178
on the nascent oxygen tests for, 71
colouring matters, 130, 141, 151, 155
products from manufacture of, 235
Answers to correspondents, 14, 28, 42, 56, 70, 84, 98, 112, 126, 140, 154, 168, 182, 196, 210, 224, 238, 266, 294, 308, 350
Anthonissen, H. making bread and obtaining starch from the materials employed simultaneously, 83
Antimony, atomic weight of, 126
a natural oxide of, stibiconise, 74
sulphide of arsenic in commercial sulphide of, 267
Apparatus for the estimation of carbonic acid, 142
Apples, use of, in dyeing, 140
Arsenic, atomic weight, 126
Arsenic, sulphide of, in commercial sulphide of antimony, 267
in sulphuric acid, 95
Arsenical pigments in common life, 75
thermal water, 6
Arseniate of soda, preparation of, 56
Arsenious acid, capacity of, for bases, 320
Arseniferous sulphur from the solfatara of Naples, and preparation of selenium, 337
Arsenites, on some, 320
Artificial spectra, effect of temperature on, 233
Asbestos paper, 56
Aschoff, M. H. on perchromic acid and the action of peroxide of hydrogen on the higher oxides, 129
Assay of silver by the moist way, 2
Atmosphere, chemical and physical modifications of, consequent on habitation, 146
electricity observations on, 175
Atomic weights of chromium, arsenic, and antimony, 126
weight of lithium, 139
Stas on the relations existing between, 1, 15, 29 (remarks on), 57
Attfield, Mr. J. on the mineral constituents of plants, 109
on the nutritive value of Dika bread, 109
poisons not always poisons, 120

Baldock, J. H. on the adulteration of tin foil, 50
Ballard, Edward, M.D. on the adulteration of butter with animal fats, 43, 99
Banquet, chemical international, 350
Baryta, aluminate of, for industrial purposes, 340
salts, use of, in dyeing, 42
Basins and reservoirs, unattackable by most acid or alkaline liquids, 200
Bassano, E. De, and A. Bruden, stearine, 97
Bandrimont, M. preparation of chlorosulphide of phosphorus, 41
Bauxite, or aluminous minerals, analysis of, 102
Bees-wax, adulteration of, 84
bleaching, 210
process for testing the purity of, 170
Bell, T. decomposition of the compounds of aluminium, and coating metals with aluminium or its alloys, 153
Benzoic acid, new derivative of, 55
anhydride, transformation of hydrochloric and hydrosulphuric acid gas, 196
Benzoylnaphthylamide, 324
Benzulmic acid, 126
Benzylmercaptan, 252
Berberine, occurrence of, in Zanthorrhiza apiifolia, 281
on, 354
Bernoulli, M. F. A. on tungsten, and some of its alloys, 116
Berthelot, M. on the absorption of carbonic oxide by alkalies, 144
on the synthesis of acetylene by the direct combination of carbon with hydrogen, 184
Besanez, Gorup, desulphuration of leucine, 195
new use for ozone, 110
Bessemer's steel, 154
Bicarbonate of ammonia, action of, upon the salts of magnesia, 235
Bilton, S. F. Esq. the law against adulteration of articles of food, and drink, &c. 306
Binitronaphthaline, reduction of, by sulphuric acid and zinc, 60
Biot, death of M. 98
Bird, Alfred; Downing, *v.* Chance, 224, 251
Bisulphide of benzyl, 252
Bleekrode, Dr. on mineral oils and
Blood, putrefaction of, 249
the Minjak Lantoeng of Java, 158
Bloxam, Prof. on the capacity of arsenious acid for bases, and on some arsenites, 320
on arsenic in sulphuric acid, 95
Thomas, F.C.S. preparation of pure hydrochloric acid, 41
Bluish vapours of chloride of ammonium, 335, 349
Boghead coal, on the indifferent hydrocarbons produced in the destructive distillation of, 151
Borax in milk, 98
Böttger, crystalline structure of wax, 139
Boudault, M. on pepsine, 235
Bouck and Co. sulphate of copper, 278
Bourne and Taylor, Messrs. improved funnel for laboratory use, 308
Boussingault, M. gases given off by plants under the influence of light, 98
nitrogen in meteoric iron, 41
Braun, M. C. D. on the volumetric estimation of nitric acid, 104
Bread, Dika, nutritive value of, 109
Briggs, J. artificial substances to be used as a covering for stone, bricks, &c. 39
British Association for the Advancement of Science, 336
Bromethylene on strychnine, action of, 266
Bromine, detection of, 254
and iodine, behaviour of essential oils to, 62, 117, 131, 148, 189, 211
Bronze for rifle-barrels, 350
Brooman, R. A. sizeing or preparing paper and textile fabrics in order to render them waterproof, and to increase the strength thereof, 69
shear-steel, 13
Broughton, J. Esq. on adjusting the needles of a galvanometer, 801
Browning, John, spectrum analysis, 40
Messrs. Spencer, and Co. new thermometer for general purposes, 112
Brudenn and E. De Bassano, stearine, 97
Brunfaut, method of obtaining the whole of the sulphur from metallic sulphides, 168
Bryant and May, Messrs. improved lucifer matches, 280
Buff, H. L. treatment of fatty and oily matters, 276

Butter, adulteration of, with animal fats, 43, 99
Butyric acid, 122

CADMIUM, sulphate of, a quick and easy method of preparing, 311
didymium, and cerium, isomorphism of sulphates of, 139
fusibility of oxide of, 97
Cæsium and rubidium in certain natural and commercial alkaline substances, 8
Caffein, test for, 189
Cajuput, oil of, to distinguish between amber and copal, 184
Calcining sulphur ores, 84
Calcium, iodide of, 337
Calico printing and dyeing, on improvements and progress in, since 1851, 164, 174, 190, 205
Calorific phenomena, notes on, 222
Calvert, Dr. F. Crace, on improvements and progress in dyeing and calico printing since 1851, 164, 174, 190, 205
on the composition of a carbonaceous substance existing in grey cast iron, 87
on the employment of galvanised iron for armour-plated ships, 193
iron-plated ships, 152
on some applications of carbolic acid, 19
wood for ship-building, 42
and Mr. Richard Johnson, on the conductibility of heat by amalgams, 53
Cameron, Dr. C. A. contributions towards a knowledge of the inorganic constituents of plants, 256, 282, 298, 314, 324
Carbolic acid, 182
or hydrate of oxide of phenyle, some applications of, 19
Carbon in iron, 252
estimation of, 5, 210
sulphide of, preparation of sulphur soluble in, 255
Carbonate of lime, experiments on the deportment of, at a high temperature, both with fluxes and alone, 116
of potash crystals, composition of, 115
and alkaline hydrates, new method of estimating, 311
Carbonic acid, a convenient apparatus for the estimation of, 142
as a means of preparation of formic acid, 227
oxide, dangers and detection of, 237
detection of in air, 209
poisonous effects of, 212
absorption of by alkalies, 144
Carius, M. on ferruretted hydrogen, 158
Caron, Captain, on the preparation of silicium, 102
Carstanjen, H. increasing the illuminating power of gas, 83
Cast-iron, 237
estimation of the carbon of, 5, 210
Caustic soda, preparation of, 255
Cavendish Society, 168
Cement, new, 9
manufacture of, 56
Cerium, cadmium, and didymium, isomorphism of sulphates of, 139
Chance, Downing *v.* 196, 224, 238, 251, 322
Charcoal, animal revivification of, 210, 253
application of, to the ventilation of sewers, 104
Chemical arts, a course of six lectures on, with reference to their progress between the two great Exhibitions of 1851 and 1862, 302, 327, 343
Discussion Association, 138
evidence, curiosities in, 182, 238
international banquet, 350
and physical modifications of the atmosphere consequent on habitation, 146
Chemical nomenclature, 41
notices from foreign sources, 27, 41, 55, 97, 111, 115, 125, 139, 168, 195, 210, 237, 252
Society, 51, 84, 94, 121, 151, 177, 206, 235, 265, 293, 319, 332, 354
tables, arranged for laboratory reference, 293
Chemistry and the manufacture of iron, 227, 296
at the International Exhibition, 239, 286, 301, 318
influence of the quantitative method in the development of scientific, 206
of soils, a handy-book of the, 306
Chevreul, resistance of starch on cotton tissue to solvents, 42
Chloracetyl, action of, on tartaric acid, 195
Chloride of ammonium, bluish vapours of, 335, 349
of lime as an insecticide, 247
of sodium and iodate of soda, double salt of, 189
Chlorides of mercury and ammonium, double salts of, 351
Chlorine, action of, on metallic oxides, 131
substitutions, new method of effecting, 52
Chlorobenzoyl, compound of nicotine with, 42
Chloroform, estimation of the purity of, 286
Chlorosulphide of phosphorus, 41
Chinese chemistry and medicine, 11
Chrome iron ore, 56
commercial analysis of, 199, 254
Chromium, atomic weight, 126
Church, A. H. notes on benzoyl-naphthylamide, 324
Mr. A. on the isolation of phenyl, 177
Mr. A. H. on silica, 95
Cinchona, growth of, in India, 317
Cinchonine and quinidine, sulphates of, in commercial sulphate of quinine, 258
Cinnabar, preparation of, 247
Citrate of lithia, 109
Citric acid, 122
and tartaric acids, separation of, 196
Clarifying saccharine liquids, juices, and syrups, and of revivifying the animal charcoal used in the manufacture of sugar, 253
wine, 224
Clark, W. composition for cleaning and polishing metals and glass, 307
electric telegraph apparatus, 209
manufacture of steel, 153
manufacture of artificial alizarine, 181
distillation of solid and liquid combustible matters, 355
Clifton, Professor, on the effect of increased temperature upon the nature of the light emitted by the vapour of certain metals or metallic compounds, 233
Coal gas, Hill's patent for the purification of, 112
tar to prevent potato disease, 42
Collodions, vesicating, 167
Colouring matters derived from aniline, 130, 141, 151, 155
naphthaline, 60
production of new, by decomposition of nitronaphthaline and dinitronaphthaline, 73
Committee on the sewage question, 141
Carbonaceous substance, composition of a, existing in grey cast iron, 87
Contributions towards a knowledge of the inorganic constituents of plants, 256, 282, 298, 314, 324
Copper and iron pyrites, estimation of sulphur in, 45
double sulphides of, 122
amalgam, 53
metallic as a test for sulphurous acid, 126
Copper, on the products resulting from the simultaneous action of air and ammonia on, 41, 113
sulphate of, 278
Croll, A. A. sulphate of alumina, 307
Mr. J. on specific heat in relation to chemical combination, 151
Crookes, W. on the divisibility of matter, 32
on a means of increasing the intensity of metallic spectra, 234
note on thallium, 349
Crossley, W. on sulphur determinations in coal, coke, &c. 245
Crum, Dr. W. on the action of mordants in dyeing, 319
Cryolite, 85
Crystallisation, 176
Crystallised gold, 210
platinum, 168
Cullen, J. preserving wood and iron, 355
Curiosities in chemical evidence, 182, 196, 238
Cuisinier, J. a new method of clarifying saccharine liquids, juices, and syrups, and of revivifying the animal charcoal used in the manufacture of sugar, 253
revivification of animal charcoal, 210
Cyanides of the platinum metals, 323
Cyanogen, acetate of, 210

D'ALBYTRE, M. candles, 208
Danchell, F. H. ascertaining and removing impurities contained in water, 181
Davies, Mr. T. on crystallisation, 176
Edward, adulteration of milk, 278
Debus, Dr. H on the influence of the quantitative method in the development of scientific chemistry, 206
Decjordin, crystalline structure of wax, 139
Delffs, protosulphate of manganese, 93
De Meley, A. C. A. Gerard, an improved treatment of natural phosphate of lime for several purposes, 152
Densities, vapour, 293
Derivatives, naphthalic, 72
Deville, M. H. St. Claire, on the analyses of aluminous minerals, or bauxite, 102
on cryolite, 85
on the formation of the topaz and the zircon, 57
on vapour densities, 293
Dialysis researches by Mr. Graham, lecture by Dr. W. Odling, 134, 161
Dianium, 41
Didymium, cadmium, and cerium, isomorphism of sulphates of, 139.
Diehl, Dr. Karl, atomic weight of lithium, 139
Diethylamine and ethylamine, reactions of, 127
Digestion, chemistry of, 354
Dika bread, nutritive value of, 109
Dinitronaphthaline and nitronaphthaline, on the production of new colouring matters by decomposition of, 73
Discovery, priority of, 126
Diseases, fermentation as a cause of various, 231
Divers, Dr. E. on the action of bicarbonate of ammonia upon the salts of magnesia, 235
Divisibility of matter, 32, 181
Don, T. drying and preparing grain, seeds, or berries for food, 39
Downing *v.* Chance, 196, 224, 238, 251, 322
Draper, Harry N. oil of cajuput as a means of distinguishing between amber and copal, 184
Drink and food, the law against adulteration, 306
Dropet, M. on the adulteration of tincture of iodine, and means of preventing it, 247
Dufour, M. on the solidification of certain substances, 11
Duncan, C. S. electric cables or ropes, 209
Dusart, M. L. on some naphthalic derivatives, 72
Dyeing and calico printing, on improvements and progress since 1851, 164, 174, 190, 205
Dyeing, mordants in, 319
Dyer, J. C. Esq. notes on calorific phenomena, 222
Dyeing, use of apples in, 140

EARLE, O. lubricating compound, 27
Eden, F. M. manufacturing silicate of lime, or hydraulic cement, 69
Edwards, W. shoes for horses, 39
Electrical storms, probable cause of, 192
Electricity, atmospheric observations on, 175
Enamelling iron, 140
Erdmann, M. J. on the characteristic reactions of some poisonous alkaloids, and some reactions of nitric acid, 229
Ether, oxalic, preparation of, 337
Ethylamine and diethylamine, reactions of, 127
Ethylene, oxide of, action of sodium amalgam on, 210
considered as a link between mineral and organic chemistry, 332
Ethyl bases, preparation of, by means of nitrate of ethyl, and their separation, 211
reaction of, with Dr. Knop's new hydrofluosilicic acid, 143
compounds of tellurium, 98
nitrate of, 158
Evidence of "Experts," 183
Exhaustion of soils, on some points in connection with, 284
Exhibition, International, chemistry, &c. at the, 239, 286, 301, 318, 326
chemical substances and products, and pharmaceutical processes, 301
Class III. substances used as food, 326, 342
Class III. and IV, animal and vegetable substances used in manufactures, 342
jottings from, 319
list of jurors in classes and sections, 278
Experts, evidence of, 183
Explosions, lecture by Professor Abel, 270

FAUCHER, M. L. on the preparation of sulphur soluble in sulphide of carbon, 255
Fermentation as a cause of various diseases, 230
influence of silicic acid on, 98
Ferri subcarbonas, 299
Ferruretted hydrogen, 158
Filtration of air, and the influence which it exerts on fermentation, putrefaction, and crystallisation, 173
Firmenich, Magnus, on the preparation of cinnabar, 247
Fitzgerald, D. G. obtaining electric currents for telegraphic purposes, 208
Fizeau, M. H. on the light emitted by sodium burning in air, 150
Flint glass which tarnishes, analysis of, 115
Food, adulteration of, 70
and drink, the law against adulteration, 306
substances used as, at the Exhibition, 326, 342
Foreign and English weights and measures, 308
Foreign sources, chemical notices from, *see* Chemical notices

Formic acid, preparation of, by means of carbonic acid, 227
Fournie, M. new way of producing local anæsthesia, 56
Fraunhofer's lines, 126
Fremy, amylaceous matter in fruits, 98
Fresenius, M. on the detection of bromine, 254
Fruits, amylaceous matter in, 98
Fuel in the manufacture of iron, 296
Fulwood *versus* Akerman, manufacture of cement, 56
Funnel for laboratory use 308

Galls, estimation of tannic acid in, 177
Galvanised or zinc covered pipes, corrosion of, when used for conducting water, 188
iron for armour-plated ships, 193
Galvanometer, on adjusting the needles of a, 301
Gases given off by plants under agency of light, 98
Gaudin, M. on aluminate of baryta and pure alumina salts for industrial purposes, 340
Generation, spontaneous, researches of Pasteur respecting the theory of, 225, 240
Gentele, M. J. C. on crystallised manganate of soda, 146
Gerardin, M. on the action of the voltaic pile on salts of potash and soda, and alloys submitted to igneous fusion, 326
Ghislin, T. G. preparing, applying, and adapting certain vegetable productions, to further the purposes of manufacture, 39
Gilbert, Dr. J. H. F.R.S. on some points in connection with the exhaustion of soils, 284
Girard, M. A. on the estimation of phosphoric acid in presence of oxide of iron and earthy bases, 281
Gladstone, Mr. on the reciprocal decomposition of salts in solution, 320
Glass, soluble, 210
which tarnishes, analysis of, 115
Glucosides, synthesis of, 238
Glucose, detection of, when mixed with cane sugar, 213
Glycerine, on the employment of, to prevent the efflorescence of salts on dried carmine of indigo, 171
Godefroy, P. A. gutta-percha, 124
treatment of india-rubber, 139
Gold, dyeing with, 196
transparency of, 84
Gore, Mr. G. on the quantitative determination of alkalies in fire-clays and other insoluble silicates, 121.
Graham, G. improvements relating to ornamental cotton fabrics, having Turkey red grounds, 69
Prof. researches on dialysis, 134, 161
Grandeau, M. L. on the presence of cæsium and rubidium in certain natural and commercial alkaline substances, 8
on the presence of rubidium in certain natural and industrial alkaline matters, 295, 310
Grape sugar to recognise beside cane sugar, 98
Gravity, specific determination of mineral substances, 246
of liquid and solid substances, 40
Gray, T. bleaching coloured rags and vegetable fibres, 335
Ground ice, 94
Guano, estimation of, 308
Peruvian, 268
Gueymard, M. on the estimation of platinum diffused through metallic beds or in the Alpine rocks of Dauphiny and Savoy, 7
Guignet, M. action of sodium amalgam on sulphide of carbon, 139
Gun-cotton, to make, 140
Guyon, M. on an arsenical thermal water, 6
Gye, F. obtaining light, 69

Hagar, the adulteration of beeswax by Japanese wax, 84
Hanson, Thomas A. utilisation of sewage, 209
Hardy, M. on the estimation of the purity of chloroform, 286
Harmonica chemica, 116
Hartnall, E. paper, 236
Hayes, A. A. M.D. on the corrosion of zinc-covered or "galvanised" pipes when used for conducting water, 188
Heat, absorption and radiation of, by gaseous matter, 136
conductibility of, by amalgams, 53
specific, in relation to chemical combination, 151
Heeren, Dr. Max, ethyl and methyl compounds of tellurium, 98
Henbane, 177
Hesse, M. on iodide of calcium, 337
Hill's patent for the purification of coal gas, 112
Hippuric and uric acids, variation in urine, 51
Historical and scientific facts about petroleum, 186
Hodgkinson, Prof. Eaton, F.R.S. a memoir of the late, 175
Hodgson, C. improvements in the manufacture of fuel from peat, 355
Holmes, Mr. on some double salts containing chloride of mercury and chloride of ammonium, 351
Hopkins, Mr. T. on the present state of meteorology, 137, 191
Hofmann, Dr. A. W. F.R.S. arsenical pigments in common life, 75
on the colouring matters derived from aniline, 130, 141, 152, 155
Horsley, John, F.C.S. on the manufacture of iodine, 125
on the apparent difficulty of detecting strychnia in the presence of morphia, discovery of a more powerful reagent for strychnia, 341
on the nitroprusside of sodium as a test for certain alkaloids, 355
House of Lords, 279
Hudson, Fearnside, utilisation of sewage, 237
Hunt, J. R. gutta-percha and compounds thereof, 138
Hydrastine, 204
Hydrates, alkaline, and carbonates, new method of estimating, 311
Hydrocarbons, indifferent, produced in the destructive distillation of Boghead coal, 151
Hydrochloric acid, preparation of pure, 41
Hydrofluosilicic acid, reaction of, ethyl bases with Dr. Knop's, 143
Hydrogen, ferruretted, 158
nascent, reduction of sulphuric acid by, 337
peroxide, action of, on higher oxides, 129
pure, 84

Ice, ground, 94
Indigo, valuation of, 42
Insecticide, chloride of lime as an, 247
Intensity of metallic spectra, means of increasing the, 234
Iodal, 139
Iodate of soda and chloride of sodium, double salt, 139
Iodide of potassium, and iodides in general, preparation of, 205
of calcium, 337
proto- of iron, 10
Iodine, action of, on tin, 210
alteration of tincture of, and the means of preventing it, 247
and bromine, behaviour of essential oils to, 62, 117, 131, 148, 189, 211
Iodine, new method of separating, 111
manufacture of, 125
Iridium, cyanides of, 323
Iron amalgam, 53
and ammonium compound, 154
carbon in, 252
estimation of, 5, 210
cast, 237
analysis of, 170
chemistry and the manufacture of, 227, 296
and copper, double sulphides of, 122
pyrites, on the estimation of sulphurous, 45
estimation of, 320
enamelling, 140
galvanised, for armour-plated ships, 193
grey cast, on the composition of a carbonaceous substance existing in, 87
meteoric, nitrogen in, 41
and manganese, oxides of, and certain sulphates, 9
ore, chrome, 56
on the oxalates of, 16
proto-iodide of, 10
pyrophoric, 139

Jaquemin, M. E. on the reduction of binotronaphthaline by sulphuric acid and zinc, 60
Jarlot, J. B. manufacture of artificial fuel, 138
Jame, W. R. manufacture of kamptulicon, 250
Jevons, W. S. spectrum analysis, 251
Johnson, J. H. electric telegraph apparatus, 83
decoloration and disinfection of liquids, 138
Mr. R. and Dr. F. C. Calvert, F.R.S. on the conductibility of heat by amalgams, 53
Professor S. W. on soil analysis, 156, 169, 197, 226, 244
W. W. and R. and Sons, manufacture of white lead, 70
Jones, Dr. H. Bence, on crystalline xanthin, 94
on the simultaneous variations of hippuric and uric acids in healthy human urine, 51
Jottings from the International Exhibition, 319
Joule, J. P. LL.D. experiments on some amalgams, 53
Dr. on the probable cause of electrical storms, 192
Jungst, detection of rosin oils in fatty oils, 111
Jurors, list of, of classes and sections in the International Exhibition, 278

Kalisch, M. H. on the construction of basins and reservoirs unattackable by most acid or alkaline liquids, 200
Keates, T. W. F.C.S. chemical tables arranged for laboratory reference, 293
Kekulé's lehrbuch der organischen chemie oder der chemie der Kohlenstoffverbindungen, 124
Kelp, manufacture of, 167
Kesster, atomic weights of chromium, arsenic, and antimony, 126
Kidd, Charles, M.D. dangers and detection of carbonic oxide, 237
Kirnot, C. N. obtaining ammoniacal salts and other valuable products from liquors or substances containing ammonia, 54
Kolbe, M. on the preparation of oxalic ether, 337
on the constitution and artificial formation of taurin, 121
preparation of formic acid by means of carbonic acid, 227
reduction of sulphuric acid by nascent hydrogen, 337
Kreatinine in urine, 97
Kuhlmann, M. Fred. on the oxides of iron and manganese, and certain sulphates considered as carriers of oxygen of the air to combustible matters, 9
use of baryta salts in dyeing and printing, 42

Laboratory reference, chemical tables arranged for, 293
Laing, R. treatment of certain ores containing metals, 96
Laming, R. alkaline carbonates, 236
Langer, M. E. generalisation of the acidimetric method, 145
Latham, Mr. A. G. on the cause of the metallic lustre on the wings of the lepidoptera, both diurnal and nocturnal, 124, 176
Lawes, J. B. F.R.S. on some points in connection with the exhaustion of soils, 284
Lead, quantitative determination of, in chemical analysis, 144
solubility of sulphate of, in acids, 52
Lea, Mr. C. on the action of nitric acid on picramic acid, 18
contributions to the history of picric acid, 5
further remarks on the preparation of the ethyl bases by means of nitrate of ethyl, and their separation, 211
on the estimation of nitrogen, 28
on nitrate of ethyl, 158
on the production of new colouring matters by decomposition of nitronaphthaline and dinitronaphthaline, 73
on the production of nitrate of methyl, 310
reaction of ethyl bases with Dr. Knop's new hydrofluosilicic acid, 143
on the reactions of ethylamine and diethylamine, 127
Lead and tin, alloys of, 121
Lepidoptera, metallic lustre on wings of, 124, 176
Leblay, M. revivification of animal charcoal, 210
Lemaire, M. coal-tar to prevent the potato disease, 42
Leplay, H. a new method of clarifying saccharine liquids, juices, and syrups, and of revivifying the animal charcoal used in the manufacture of sugar, 253
Letchford and Co. Messrs. recent improvements in lucifer matches, 209, 224, 266
Letheby, Dr. adulteration of food, 70
on the application of charcoal to the ventilation of sewers, 104
on the application of voltaic electricity to the detection of aniline, 178
on the nascent oxygen tests for aniline, 71
the poisonous effects of carboni oxide, 212
Leuchs, J. C. influence of silicic acid on fermentation, 98
Leucic acid and its salts, 42
Leucine, desulphuration of, 195
Levol, M. A. on the quantitative determination of lead in chemical analysis, 144
Liebig on Peruvian guano, 268
Lienan, M. preparation of pure nitrate of silver, 200
Light, a course of six lectures on, by Prof. Tyndall, 20, 33, 47, 65, 76, 89
emitted by sodium burning in air, 150
emitted by the vapour of certain metals or metallic compounds, effect of increased temperature on, 233
gases given off by plants under agency of, 98
Lime, experiments on the deportment of carbonate of, at a big temperature, both with fluxes and alone, 116

Lime, phosphate of, in teak, 95
Lippman, temperatures at which starches dissolve, 98
Lithia, citrate of, 109
Lithium, atomic weight of, 139, 183
in meteoric stones, 208
"London Review" on thallium, 350
Luchs, Dr. new method of separating iodine, 111
Lucifer matches, recent improvements in, 266, 280

MACARDLE, J. S. treatment of seaweeds, 276
Magnesia salts, action of bicarbonate of ammonia upon, 235
M'Gauley, M. on aluminium, 338
Maisch, John M. on the behaviour of essential oils to iodine and bromine, 62, 117, 131, 148, 189, 211
on the conversion of monohydrated into common phosphoric acid, 61
Makins, Mr. transparency of gold, 84
Malcolm, J. D. improvements in the manufacture of nitric acid and caustic soda, 69
Manchester Literary and Philosophical Society, 12, 26, 53, 80, 122, 137, 175, 191, 222, 249, 333
Manganate of soda, crystallised, 146
Manganese and iron, on the oxides of, and certain sulphates, artificial production of a new cement, 9
protosulphate of, 98
in the scoria from old copper workings, 210
Manning, J. A. collecting ammonia from the waste gases arising from the combustion of coal, 124
Manual of domestic economy, 334
Manufacture of white lead, 55
Manure, artificial, analysis of, 228
and Sanatory Company, 294
Manures, phosphates as, 112
Marcet, Dr. on the chemistry of digestion, 354
Marignac, M. remarks of M. Stas's researches on the relations existing between atomic weights, 57
Marriage, Mr. on the estimation of tannic acid in galls, 177
Martius, Dr. on Aribin, 42
on some cyanides of the platinum metals, 323
Matches, lucifer, recent improvements in, 209, 224, 266, 280
paraffin, 224
Matter, divisibility of, 32, 181
Matthieson, Dr. on alloys of lead and tin, 121
Matthews, Henry, F.C.S. fusibility of oxide of cadmium, 97
Maumené, M. on the analysis of mixtures of potash and soda, 72
McKay, J. cements or adhesive solutions for joining or connecting together surfaces or articles of leather, wood, paper, &c. 335
Measures and weights, foreign and English, 308
Medlock, Henry, preserving fermented liquors, 27
Meidinger, Dr. compound of iron and ammonia, 154
Mennons, M. A. F. an improved combination of metals for the production of a white alloy resisting the action of vegetable acids, 251
manufacture of paper and cardboard from a fibrous vegetable matter, 97
Ménétriès, action of bromethylene on strychnine, 266
Mercury, chloride of, with chloride of ammonium double salts, 351
Metallic spectra, means of increasing intensity of, 234
Metallurgy, Dr. Percy's, 111, 189, 207, 240
Meteorology, on the present state of, 137, 191
Methyl, compounds of tellurium, 98
production of nitrate of, 310
Meyer, J. certain new chemical combinations, and for the application thereof to fixing aniline and pigment colours in printing and dyeing, and other industrial purposes, 97
Microscopic objects, in mounting, 97
Milk adulteration, 278
composition of, 293
quality of, sold in the poor and other districts of Dublin, 242
Miller, Dr. on spectrum analysis, 201, 214
Millon, M. prussic acid, 125
Millward, P. A. coke, 209
Minary, M. cast iron, 237
Mineral constituents of plants, 109
oils and the Minjak Lantoeng of Java, 158
Mingand, P. obtaining jellies, syrups, and drinks from the tree *Arbutus unedo*, 83
Molecular dissymmetry of natural organic products, 107, 119, 133, 149, 160, 203, 231, 248, 260
Mohr, M. on the analysis of mixtures of potash and soda in neutral and alkaline solutions, 71
Mollusca, tongues of, 177, 192, 334
Mordants in dyeing, 319
Monochlorinated sulphuric acid 224
Morin, M. Paul, on aluminate of soda and its uses, 296
Morphia, influence of, in disguising the usual colour test of strychnia, 316
difficulty of detecting strychnia in presence of, 341
modifying the colour-tests of strychnia, 341, 352
Mosling, transformation of benzoic anhydride by hydrochloric and hydrosulphuric acid gases, 196
Mulder, E. errors in the estimation of nitrogen of combustion with soda lime, 238
on the estimation of the carbon of cast iron, 5, 210
G. J. chemical researches on the assay of silver by the moist way, 2
Müller, Dr. Hugo, on a new mode of effecting chlorine substitutions, 52

NAPHTHALINE, on the coloured derivatives of, 8, 19, 60, 72
Needles of a galvanometer, adjusting, 301
Newbauer, Dr. C. kreatinine in urine, 97
Newlands, J. A. R. F.C.S. chemical nomenclature, 41
Newton, R. V. bleaching and refining oils, 96
W. A. lubricating compound, 138
refining and purifying iron, and converting the same into steel, 209
treatment of copper ore, 125
Nicotine, compound of, with chlorobenzoyle, 42
Niobium, 41
Nitrate of ethyl, 158
methyl, 310
soda, on the production of, at Iquique, (Peru), 31
silver, preparation of pure, 200
soda, action of, on sulphide of sodium, 80
Nitre, analysis of, 294
preparation of, from carbonate of potash and nitrate of soda, 268
Nitric acid, action of, on picramic acid, 18
new method of detecting small quantities, 111
on the volumetric estimation of, 104
Nitrogen, errors in the estimation of, by combustion with soda lime, 238
in meteoric iron, 41
on the estimation of, 28
Nitrogen, preparation of, by action of chlorine on ammonia, 246
protoxide of, preparation of, by the moist way, 254
Nitronapthaline and dinitronaphthaline, on the production of new colouring matters by decomposition of, 73
Nitroprusside of sodium as a test for certain alkaloids, 355
Noble. Alfred, crystallised platinum, 168
on Schnitzer's process for the preparation of nitrate of potash, 293
Nomenclature, chemical, 27, 41
Normandy, Dr. refrigerating the fresh water produced by condensing steam, 348
Noxious and offensive vapours, 267

ODLING, Dr. W. lecture on dialysis, 134, 161
Oil of Cajuput as a means of distinguishing between amber and copal, 184
paraffin, report on the quality of illuminating oils sold in Manchester and the neighbourhood, 312
Oils, behaviour of essential, to iodine and bromine, 117, 131, 148, 189, 211
mineral and the Minjak Lantoeng of Java, 158
O'Neill, Charles, on the commercial analysis of chrome ores, 199
paraffin oil, report on the quality of illuminating oils sold in Manchester and the neighbourhood, 312
Opium, chemistry of, 265
Organic products, natural, on the molecular dissymmetry of, 107, 119, 133, 149, 160, 203, 231, 248, 260
Osmium, cyanides of, 323
Oudesluys, Mr. American chrome iron ore.—Asbestos paper, 56
on the commercial analysis of chrome ores, 254
Oxalates of iron, 16
Oxalic ether, preparation of, 337
Oxide of cadmium, fusibility of, 97
carbonic, absorption of by alkalies, 144
dangers and detection of, 237
detection of, in air, 209
poisonous effects of, 212
of ethylene considered as a link between mineral and organic chemistry, 332
Oxides, metallic, action of chlorine on, 131
Oxygen, action of, on ammonia in contact with oxides, 89
nascent, tests for aniline, 71
contributions to the history of, 114
Oxyhydrogen light, 222
Ozone, 350
new use for, 110

PARAFFIN matches, 224
oil, report on the quality of illuminating oils sold in Manchester and the neighbourhood, 312
Parrish, E. on the preparation of resin of podophyllum, 229
Pasteur, L. on the molecular dissymmetry of natural organic products, 107, 119, 133, 149, 160, 203, 231, 248, 260
Patent laws, 309
Eureka Sanatory and Manure Company, 294
Patents, chemical:—
sulphuric acid, 96
sulphate of alumina, 307
alkaline carbonates, 236
improvements in the manufacture of nitric acid and caustic soda, 69
obtaining ammoniacal salts and other valuable products from liquors or substances containing ammonia, 54
bleaching and refining oils, 96
Patents, chemical:—
an improved treatment of natural phosphate of lime for several purposes, 152
ascertaining and removing impurities contained in water, 181
matches, 209
manufacturing oxygen gas, and obtaining certain other products, 96
fatty and oily matters, treatment of, 276
stearine, 97
Patents for colour, &c.:—
colour, 13
improvements relating to ornamental cotton fabrics, having Turkey red grounds, 69
certain new chemical combinations, and for the application thereof to fixing aniline and pigment colours in printing and dyeing, and other industrial purposes, 97
manufacture of artificial alizarine, 181
colouring matters derived from naphthaline, and application of the same to the dyeing and printing of fabrics, 97
a new method of colouring, as a substitute for saffron, in the manufacture of cheese, &c. 39
fixing colours in connection with the printing and dyeing of woollen fabrics and yarns, 97
materials to be used in the process of dyeing and printing, 180
Patents, electrical:—
electric telegraphs, 97
electric telegraph apparatus, 83, 209
cables or ropes, 209
improvements in the manufacture of insulators for telegraphic wires, and in materials and machinery for coating telegraphic wires, 251
batteries for producing voltaic electricity, together with certain metallic products, 208
obtaining electric currents for telegraphic purposes, 208
Patents for gas, fuel, &c.:—
obtaining light, 69
gas, improvements in the manufacture of, and in the apparatus connected therewith, 251
purification of coal, 96
increasing the illuminating power of, 83
obtaining products from coal, 236
collecting ammonia from the waste gases arising from the combustion of coal, 124
candles, 208
coke, 209
manufacture of artificial fuel, 138
combustion of fuel for generating steam, 54
economising fuels in evaporating and concentrating, 307
heating by means of lamps, 348
manufacture of coal gas by the surplus or waste heat of the puddling and blast furnaces used in the manufacture of iron, 355
distillation of solid and liquid combustible matters, 355
improvements in the manufacture of fuel from peat, and in apparatus employed therein, 355
an improved method of partially drying peat before removing the same from the bog, 355
Patents, metallurgical:—
treatment of certain ores containing metals, 97
decomposition of the compounds of aluminium, and coating metals with aluminium or its alloys, 153
refining and purifying iron, and converting the same into steel, 209

Patents, metallurgical :—
treatment of copper ore, 125
manufacture of steel, 153
shear steel, 13
purifying lead, 152
an improved combination of metals for the production of a white alloy resisting the action of vegetable acids, 251
Patents, paper, &c. :—
paper, 236, 348
manufacture of paper and cardboard from a fibrous vegetable matter, 97
converting vegetable fibrous substances into pulp, 335
sizeing or preparing paper and textile fabrics in order to render them waterproof, and to increase the strength thereof, 69
bleaching coloured rags and vegetable fibres, 335
Patents for building, &c. :—
an improved composition for building, to be used in substitution for brick and stone, and an improved method of constructing walls and roofs for houses, &c. 276
preparation for hardening and preserving stone and cement, 307
improvements in the manufacture of artificial stone and cement, or plaster, and in treating timber, for the purpose of preserving the same, 39
artificial substances to be used as a covering for stone, bricks, wood, &c. 39
manufacturing silicate of lime or hydraulic cement, 69
combining together siliceous powder into solid masses of any form by means of sulphur, 181
Patents, food, &c. :—
making bread, and obtaining starch from the materials employed simultaneously, 83
drying and preparing grain seeds, or berries for food, 39
preserving fermented liquors, 27
obtaining jellies, syrups, and drinks from the tree *Arbutus unedo*, 83
decoloration and disinfection of liquids, 138
refrigerating the fresh water produced by condensing steam, 348
pepsine, 235
Patents, gutta-percha, india-rubber, &c. :—
gutta-percha, 124
and compounds thereof, 138
treatment of india-rubber, 139
kamptulicon, manufacture of, 250
preparing, applying, and adapting certain vegetable productions, to further the purposes of manufacture, 39
impermeable varnish for leather, 535
treating skins, 335
compounds of india-rubber and gutta-percha with other substances, 355
Patents, miscellaneous :—
artificial manures, 96
apparatus for receiving, drying, and deodorising human excrement, 54
cements or adhesive solutions for joining or connecting together surfaces or articles of leather, wood, paper, &c., 335
composition for cleaning and polishing metals and glass, 307
enamel, 125
preserving wood and iron, 355
lubricating compound, 27, 183
shoes for horses, 39
sea-weeds, treatment of, 276
grants of provisional protection for six months, 13, 27, 39, 55, 69, 83, 111, 125, 139, 153, 181, 194, 236, 277, 320, 34
Patents miscellaneous :—
notices to proceed, 14, 27, 55, 83, 111, 125, 139, 153, 195, 209, 236, 277, 321, 349
inventions protected for six months by the deposit of complete specifications, 27, 125, 321
Pattison, R. T. fixing colours in connection with the printing and dyeing of woollen fabrics and yarns, 97
Pauli, Dr. on the preparation of pure caustic soda on the large scale, 351
Payen, M. amylaceous matter in fruits, 98
Peligot, M. on the products resulting from the simultaneous action of air and ammonia on copper, 41, 113
Pelouze, M. J. on the estimation of sulphur in iron and copper pyrites, 45
amylaceous matter in fruits, 98
Perchromic acid, and the action of peroxide of hydrogen on the higher oxides, 129
Percy, Dr. John, F.R.S. metallurgy, 110, 180, 207, 249
Perrins, J. Dyson, berberine in *Xanthorrhiza apiifolia*, 281
on hydrastine, 204
Mr. on berberine, 354
Personne, M. action of iodine on tin, 210
Peruvian guano, 268
Persoz, M. J. contributions to the history of naphthaline, 19
new method of estimating alkaline hydrates, and carbonates, and other compounds of this class, 311
Petroleum, historical and scientific facts about, 186
Pharmaceutical processes at the International Exhibition, 286, 301, 318
Pharmaceutical Society, 109, 120, 167, 177, 204, 294
Phenomena, calorific notes on, 222
Phenyle, isolation of, 177
oxide of, some applications of, 19
Phillips, W. H. combustion of fuel for generating steam, 54
Phipson, Dr. T. L. analysis of an artificial manure, 228
on crystallised platinum, 144
determination of the specific gravity of mineral substances, 246
on a new harmonica chemica, 116
potabilisation of sea-water by the electric current, 322
on some transformations of citric, butyric and valeric acids, 122
on stibiconise, a natural oxide of antimony from Borneo, 74
on the arsenicferous sulphur from the solfatara of Naples, and preparation of selenium, 337
Phosphate of lime in teak, 95
Phosphoric acid, on the conversion of monohydrated into common, 61
estimation of, in presence of oxide of iron and earthy bases, 281
in primitive rocks, 210
Phosphorus, chloro-sulphide of, 41
Picramic acid, on the action of nitric acid on, 18
preparation of, 60
Picric acid, contributions to the history of, 5
Pilz, action of chloracetyl on tartaric acid, 195
Plants, contributions towards a knowledge of the inorganic constituents, 256, 282, 298, 314, 324
gases given off by, under agency of light, 98
mineral constituents of, 109
Platinum, crystallised, 144, 168
estimation of, diffused through metallic beds or in the Alpine rocks of Dauphiny and Savoy, 7
Platinum metals, cyanides of, 323
standard kilogrammes, 64
Playfair, Dr. Lyon, F.R.S. a course of six lectures on some of the chemical arts, with reference to their progress between the two great Exhibitions of 1851 and 1862, 302, 327, 343
Plumbago in India, 154
Podophyllum, preparation of resin of, 229
Pohl, M. analysis of a kind of flint glass which tarnishes, 115
on the composition of carbonate of potassa crystals, 115
on the employment of glycerine to prevent the efflorescence of salts on dried carmine of indigo, 171
facts relative to the solubility of chemical combinations, 115
Poisonous alkaloids, on the characteristic reactions of some, 229
process for the extraction and investigation of, 229
Poison, on the detection of strychnia, and on the influence of morphia in disguising the usual colour test, 316
Poisons not always poisons, 120
Polli, M. on fermentation as a cause of various diseases, 230
Polytechnic Institution, 14
Pontifex, S. R. quality of the milk sold in the poor and other districts of Dublin, 242, 293
Ponton, A. C. combining together siliceous powder into solid masses of any form, by means of sulphur, 181
Potabilisation of sea-water by the electric current, 322
Potato disease, coal-tar to prevent, 42
Potash, crystals of carbonate of, composition of, 115
nitrate of, Schnitzer's process for the preparation of, 293
preparation of, 268
and soda, salts and alloys submitted to igneous fusion, action of the voltaic pile on, 226
analysis of mixture of, 71, 72
Potassium, iodide of, preparation of, 205
Prince, A. improvement in the manufacture of gas, 251
Protoxide of nitrogen, preparation of, by the moist way, 254
Prussic acid, 125
Pyrites, iron, and copper, estimation of sulphur in, 45
Pyrophoric iron, 139

Quantitative method, influence of, in the development of scientific chemistry, 206
Quinidine and cinchonine, sulphates of, in commercial sulphate of quinine, 258

Rain following discharge of ordnance, 12
Rammelsberg, double salt of iodate of soda and chloride of sodium, 139
the sulphates of cadmium, didymium, and cerium isomorphous, 139
Ransome, F. artificial stone, 308
improvements in the manufacture of artificial stone and cement, or plaster, and in treating timber for its preservation, 39
Rape, detection of, in other oils, 111
Reese, John J. on the detection of strychnia as a poison, and on the influence of morphia in disguising the usual colour test, 316
Refraction, index of, 42
Regnauld, M. J. recent researches on metallic amalgams, and on the origin of their chemical properties, 259
Regnault, M. on platinum standard kilogrammes, 64
Reinsch, metallic copper as a test for sulphurous acid, 126
Report on the past, present, and future of the Royal Institution, 178, 193
Résal, M. cast iron, 237
Reservoirs and basins, construction of, unattackable by most acid or alkaline liquids, 200
Resin of podophyllum, preparation of, 229
Rex, T. A. on ferri subcarbonas, 299
Reynolds, Emerson J. a convenient apparatus for the estimation of carbonic acid, 142
on the oxalates of iron, 16
R. F.C.S. sulphide of arsenic in commercial sulphide of antimony, 268
Rhodium, cyanides of, 323
Rhodizonic acid, 75
Richardson, T. purification of coal gas, 96
Riley, J. materials to be used in the process of dyeing and printing, 180
on the presence of titanic acid, and means of determining iron in clay, 320
Robbins, J. on the preparation of silicium, 102
Robinaud, M. process for testing the purity of bees-wax, 170
Rodwell, G. F. Esq. on the solubility of sulphate of lead in hydrochloric and nitric acids, 52
Roeber, H. G. B. improvements in the manufacture of insulators for telegraphic wires, &c. 251
Roger, M. on the determination of the presence of sulphates of cinchonine and quinidine in commercial sulphate of quinine, 258
Roman, J. economising fuels in evaporating and concentrating, 307
Roscoe, Prof. on the action of nitrate of sodium on sulphide of sodium at different temperatures, 80
on the effect of increased temperature upon the nature of the light emitted by the vapour of certain metals or metallic compounds, 233
a course of three lectures on spectrum analysis, 218, 261, 287
Rose, G. experiments on the deportment of carbonate of lime at a high temperature, both with fluxes and alone, 116
M. H. on the estimation of selenium, 185
Rosenstiehl, monochlorinated sulphuric acid, 224
synthesis of glucosides, 238
Rosin oil, detection of, in fatty oils, 111
Roussin, M. Z. on the coloured derivatives of naphthaline, 8, 97
Routledge, T. on paper, 348
Rowland, O. electric telegraphs, 97
Royal Institution of Great Britain, a lecture by T. A. Abel, Esq. on some of the causes, effects, and military applications of explosions, 270
lectures by Dr. W. Odling, F.R.S. on Mr. Graham's researches in dialysis, 134, 161
a course of six lectures by Dr. Playfair, on some of the chemical arts, with reference to their progress between the two great Exhibitions of 1851 and 1862, 302, 327, 343
a course of three lectures by Prof. Roscoe, on spectrum analysis, 218, 261, 287
a course of six lectures by John Tyndall, Esq. F.R.S. &c. on light (adapted to a juvenile auditory), 20, 33, 47, 65, 76, 89
a lecture by Prof. Tyndall, F.R.S. on the absorption and radiation of heat by gaseous matter, 136

Royal Institution of Great Britain, report on the past, present, and future of, 178, 193
Rubidium and cæsium, on the presence of, in certain natural and commercial alkaline substances, 8
presence of, in certain natural and industrial alkaline matters, 295
presence of, in certain vegetable substances, 310
Russe, G. a new method of colouring, as a substitute for saffron in the manufacture of cheese, &c., 39
Rust, Jesse, preparation for hardening and preserving stone and cement, 307
Rutter, E. J. the divisibility of matter, 181

SACCHARINE liquids, &c. a new method of clarifying and of revivifying the animal charcoal used in the manufacture of sugar, 253
Salter, Thomas, F.C.S. fusibility of oxide of cadmium, 97
Salts in solution, reciprocal decomposition of, 320
Sanatory and Manure Company, 294
Schiel, presence of phosphoric acid in primitive rocks, 210
Schiff, M. H. on the preparation of protoxide of nitrogen by the moist way, 254
Schliesser, products of the oxidation of sulphindigotic acid, 252
Schlosing, tobacco poison, 28
Schmidt, detection of glucose mixed with cane sugar, 213
preparation of formic acid by means of carbonic acid, 227
to recognise grape sugar beside cane sugar, 98
Schneider, F. detection of rape in other oils, 111
Schnitzer, M. G. on the preparation of nitrate by means of carbonate of potash and nitrate of soda, 268
process for the preparation of nitrate of potash, 293
Schoonbroodt, M. iodal, 139
Schönbein, M. on the action of oxygen on ammonia in contact with oxides, 89
contributions to the history of oxygen, 114
on ozone, 350
Schroeder, M. H. on the filtration of air and the influence which it exerts on fermentation, putrefaction, and crystallisation, 173
Schrötter, bicarbonate of ammonia, 325
Schutzenberger, acetate of cyanogen, 210
benzulmic acid, 126
and Sengenwald, new derivative from benzoïc acid, 55
Schulze, Professor, new method of detecting small quantities of nitric acid, 111
Schwarzenbach, test for caffein, 139
Science a civiliser, 70
Scoffern, John, "a Handy-book of the Chemistry of Soils," &c. 306
Sea-water, potabilisation of, by the electric current, 322
Selenium, estimation of, 185
on the preparation of, 337
Sengenwald, M. benzulmic acid, 126
and Schutzenberger, new derivative from benzoic acid, 55
Sewage question, select committee on, 141
utilisation of, 209, 237
of towns, 308
Sewers, on the application of charcoal to the ventilatation, 104
Shearman, Dr. C. J. on the causes of sickness and mortality in the manufacturing towns of the north-west of England, 123
Shepard, W. A. obtaining products from coal, 236
Sheuerer-Kestner, M. on the colouring matters derived from naphthaline, 60
Ship-building, wood for, 42
Silica, 95
influence of, on fermentation, 98
Silicates, insoluble, determination of alkalies in, 121
Silicium, preparation of, 102
Silver, assay of, by the moist way, 2
nitrate of, preparation of pure, 200
Smith, Dr. R. A. on the putrefaction of blood, 249
Society of Arts, 164, 174, 190, 205
Chemical, *see* Chemical Society
Manchester Literary and Philosophical, *see* Manchester
Pharmaceutical, *see* Pharmaceutical Society
Soda, action of nitrate of, on sulphide of sodium, 80
aluminate of, and its uses, 296
caustic, preparation of, 255
crystallised manganate of, 146
iodate of, and chloride of sodium double salt, 139
preparation of arseniate of, 56
on the production of nitrate of, at Iquique (Peru), 31
and potash, analysis of mixtures of, 71, 72
and potash salts and alloys submitted to igneous fusion, action of the voltaic pile on, 326
on the preparation of pure caustic, on the large scale, 351
Sodium amalgam, action of, on oxide of ethylene, 210
action of, on sulphide of carbon, 139
light emitted by, when burning in air, 150
nitroprusside of, as a test for certain alkaloids, 355
Soil analysis, 156, 169, 197, 226, 244
Soils, chemistry of, a handy-book, 306
some points in connection with the exhaustion of, 284
Solidification of certain substances, 11
Solubility of chemical compounds, 115
Soluble glass, 210
Specific gravity of liquid and solid substances, 40
of mineral substances, determination of the, 246
Spectra, metallic means of increasing the intensity of, 234
Spectrum analysis, 40, 201, 214, 251
a course of three lectures, by Dr. Roscoe, 218, 261, 287
Spence, Mr. Peter, calcining sulphur ores, 84
Spontaneous generation, researches of Pasteur respecting the theory of, 225, 240
Squire, W. S. Ph.D. on the preparation of iodide of potassium and iodides in general, 205
Standfast, S. an improved composition for building, to be used in substitution for brick and stone, and an improved method of constructing walls and roofs of houses, &c. 279
Stanford, Mr. E. C. on the manufacture of kelp, 167
Starch on cotton tissues, resistance of, to solvents, 42
temperature at which it dissolves, 98
Stas on the relations existing between atomic weights, 1, 15, 29
(remarks on), 57
Steart, J. treating skins, 335
Stebbing, J. carbon in iron, 252
Steel, Bessemer, 154
Stevens, C. artificial manures, 96
impermeable varnish for leather, 235
Stibiconise, a natural oxide of antimony from Borneo, 74
Stone, artificial, 308
Storms, electrical, probable cause of, 192
Streng, Dr. Aug. preparation of arseniate of soda, 56
Strychnine, action of bromethylene on, 266
Strychnia, apparent difficulty of detecting in the presence of morphia—discovery of a more powerful reagent for strychnia, 341
colour tests of, as modified by the presence of morphia, 341, 352
on the detection of, as a poison, and on the influence of morphia in disguising the usual colour test, 316
Sugar, to recognise grape beside cane, 98
Sulphates of cadmium, didymium, and cerium isomorphous, 139
of cadmium, a quick and easy method of preparing, 311
cinchonine and quinidine in commercial sulphate of quinine, 258
copper, 278
lead, solubility of, in acids, 52
(proto-) of manganese, 98
Sulphide of arsenic in commercial sulphide of antimony, 267
copper and iron, 122
sodium, action of nitrate of soda on, 80
Sulphindigotic acid, products of the oxidation of, 252
Sulphur, determinations in coal, coke, &c. 245
estimation of, in iron and copper pyrites, 45
arseniferous, from the solfatara of Naples, 337
method of obtaining the whole from metallic sulphides, 168
ores, calcining, 84
soluble, preparation of, in sulphide of carbon, 255
Sulphuric acid, arsenic in, 95
monochlorinated, 224
reduction of, by nascent hydrogen, 337
metallic copper, a test for, 126
Swinterton, T. manufacture of coal gas by the waste heat of iron furnaces, 355
Symons, W. use of apples in dyeing, 140
Synthesis of acetylene by the direct combination of carbon with hydrogen, 184

TANNIC acid in galls, on the estimation of, 177
Tartaric and citric acids, separation of, 196
Taurin, 121
Taylor, F. apparatus for receiving, drying, and deodorising human excrement, 54
W. B. heating by means of lamps, 348
Teak, phosphate of lime in, 95
Tegetmier, W. B. a manual of domestic economy, 334
Tellurium, methyl, and ethyl compounds of, 98
Temperature, connecting equilibrium of, in the atmosphere, 81
effect of, upon the nature of the light emitted by the vapour of certain metals or metallic compounds, 233
Terreil, M. manganese in the scoria
Tests, nascent oxygen for aniline, 71
from old copper workings, 210
Thénard, M. phosphates as manures, 112
Thallium, note on, 349
Thermometer for general purposes, 112
J. Williams on, 350
Thomas, R. P. on the colour-tests of strychnia as modified by the presence of morphia, 341, 352
Thompson, Dr. plumbago in India, 154
Thomson, Prof. Wm. LL.D. F.R.S. on the connective equilibrium of temperature in the atmosphere, 82
observations on atmospheric electricity, 175
Tichborne, Mr. research on the separation, composition, and physiological source of normal urinary pigment, 171
on vesicating collodions, 167
Tin, action of iodine on, 210
estimation of, 86
foil, adulteration of, 59
and lead, alloys of, 121
Titanic acid in clay, 320
Tobacco poison, 28
Topaz and the zircon, formation of the, 57
Transparency of gold, 184
Troost, M. L. on the equivalent of lithium, 183
Tungsten and some of its alloys, 116
Turpentine, combustion of, in chlorine gas, 112
Tyndall, Prof. F.R.S. on the absorption and radiation of heat by gaseous matters, 136
John, Esq., F.R.S. a course of six lectures on light, 20, 33, 47, 65, 76, 89

URIC and hippuric acids, variation in urine, 51
Urinary pigment, researches on, 171
Urine, kreatinine in, 97
Uslar, M. and J. Erdmann, process for the extraction and investigation of poisonous alkaloids, 229
Utilisation of sewage, 209, 237

VALENTINE, Mr. W. on a new source of acetone, 122
Valeric acid, 122
Vanadic acid, 103
Vanadium, 86
Vapour densities, 293
Vapours, bluish, of chloride of ammonium, 335, 349
noxious and offensive, 267
Ventilation of sewers, on the application of charcoal to the, 104
Versmann, F. colour, 13
Vezu, M. preservation of protoiodide of iron, 10
Vignon, J. V. enamel, 125
Vogt, benzyl mercaptan and bisulphide of benzyl, 252
Vondei, C. J., crystallised gold, 210
Von Kobell, dianium or niobium, 41
Voltaic electricity, application of, to the detection of aniline, 178
batteries for producing, together with certain metallic products, 208
pile, action of, on salts of potash, and soda, and alloys submitted to igneous fusion, 326
Volumetric estimation of nitric acid, 104

WAAGE, P. leucic acid and its salts, 42
Wanklyn, M. on ferruretted hydrogen, 158
Ward, P. sulphuric acid, 96
Water, on an arsenical thermal, 6
colour of, 55
composition of certain boiling springs in New Zealand, 96
Watt, J. converting vegetable fibrous substances into pulp, 335
Wawinkiewiez, M. R. generalisation of the acidimetric method, 145
Wax, crystalline structure of, 139
Weber, M. R. on the action of chlorine on metallic oxides, 131
Webster, J. manufacturing oxygen gas and obtaining certain other products, 96

Weights, atomic, Stas on the relations existing between, 1, 15, 29 (remarks on), 57
and measures, foreign and English, 308
Weil, M. W. on the analysis of cast iron, 170
White lead, manufacture of, 55, 70
Will, H. composition of rhodizonic acid, 75
Williams, C. Greville, F.R.S. on the indifferent hydrocarbons produced in the destructive distillation of Boghead coal, 151
J. note on thallium, 350
W. Matticu, bluish vapours of chloride of ammonium, 335
Downing *v.* Chance, 322
J. F. compounds of india-rubber and gutta-percha with other substances, 355
Wine, clarifying, 224
Wittstein, colour of water, 55
Woehler, M. on the preparation of caustic soda, 255
Wood for ship-building, 42
Woulor, W. J. quality of the milk sold in the poor and other districts of Dublin, 242 293,
Wurz, M. action of sodium amalgam on oxide of ethylene, 210
XANTHORRHIZA apiifolia, occurrence of berberine in, 281
Xanthin, crystalline, 94
ZANTEDESCHI, Professor, Fraunhofer's lines, 126
Zenner, D. purifying lead, 152
Zinc-covered or "galvanized" pipes, corrosion of, when used for conducting water, 188
Zircon and the topaz, on the formation of, 57

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